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The American Psychological Association, founded in 1892 and incorporated in 1925, is the major psychological organization in the United States. The purpose of the APA is to advance psychology as a science, and as a means of promoting human welfare. It attempts to accomplish these objectives by holding annual meetings, publishing psychological journals, and working toward improved standards for psychological training and service.

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Racial and Ethnic Bias in Psychological Tests

Divergent Implications of Two Definitions of Test Bias

FRANK L. SCHMIDT *Michigan State University*[1]
JOHN E. HUNTER *Michigan State University*

In recent years, the question of possible ethnic and racial bias in psychological tests has occupied a great deal of the attention of psychologists concerned with educational and employment testing. In this connection, agreement on the definition of test bias was necessary before meaningful research—or even meaningful discussions—could be carried out. Very early in this effort, agreement was reached that the position which defines a test as fair only if there are no mean score differences between population subgroups was untenable (Cleary, 1968; Guion, 1966). Such a definition eliminates a priori any possibility of real group differences on various psychological traits, a position which is more than questionable in light of empirical evidence indicating the widespread existence of such differences (Tyler, 1965). The definition that came to be accepted by most working in this area was the one given by Cleary (1968):

> A test is biased for members of a subgroup of the population if, in the prediction of a criterion for which the test was designed, consistent nonzero errors of prediction are made for members of the subgroup. In other words, the test is biased if the criterion score predicted from the common regression line is consistently too high or too low for members of the subgroup. With this definition of bias, there may be a connotation of "unfair," particularly if the use of the test produces a prediction that is too low. If the test is used for selection, members of a subgroup may be rejected when they were capable of adequate performance [p. 115].

This definition of unfair test bias has been endorsed and advocated by educational researchers (Kallingal, 1971; Linn & Werts, 1971; Pfeifer & Sedlacek, 1971; Stanley & Porter, 1967), industrial psychologists concerned with selection (Bartlett & O'Leary, 1969; Einhorn & Bass, 1971; Grant & Bray, 1970; Tenopyr, 1967), and textbook authors (Anastasi, 1968, p. 559; Cronbach, 1970, pp. 305–306). Even psychologists as highly critical of current testing practices as Kirkpatrick (1970) have endorsed the Cleary definition (Kirkpatrick, Ewen, Barrett, & Katzell, 1968, p. 7). A detailed treatment of the statistical and psychometric requirements of this definition (Potthoff, 1966) has been written and has received wide acceptance and usage by researchers in this area.

More recently, an alternative definition of test bias has been advanced by Thorndike (1971). Thorndike's definition holds that a test used for educational or employment selection is fair only if, for any given criterion of success, the test admits or selects the same proportion of minority applicants that would be admitted or selected by selection on the criterion itself or on a perfectly valid test. For example, if it is known, based on past experience, that 37% of minority applicants equal or exceed the average majority group member in actual performance on the job or in the educational institution, and if the selection ratio is such as to admit 50% of the majority applicants, the test must admit 37% of the minority applicants to be considered fair.

Both the Cleary and the Thorndike definitions have a ring of reasonableness about them. Certainly, given acceptance of the principle of individualized treatment based on individual merit, it appears unfair to overpredict or underpredict the performance of any individual or group of indiduals (Cleary's definition); likewise, it seems unfair to accept onto the job or into the educational institution a smaller proportion of any population subgroup than could in fact meet the criterion or criteria of success as set by the organization. In fact, it may appear that these two concepts of test fairness are conceptually equivalent—merely different ways of stating the same principle. This is not the case, however; the two definitions are very different, and in most testing situations, they lead to different conclusions about the fairness of a given psychological test. In this article, the two competing definitions of test fairness and their differing impli-

[1] Requests for reprints should be sent to Frank L. Schmidt, Department of Psychology, Michigan State University, East Lansing, Michigan 48824.

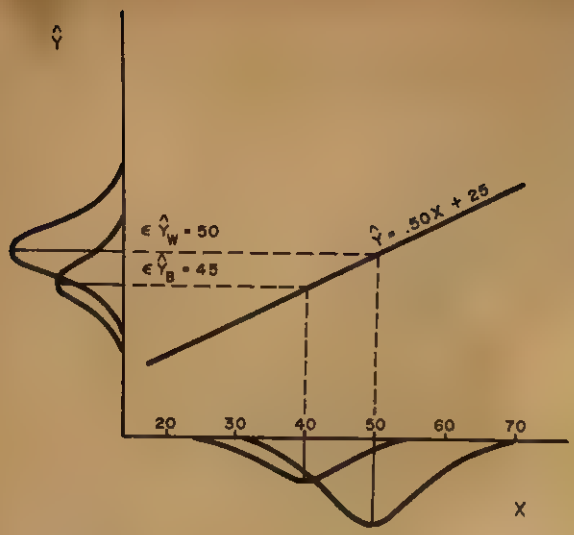

Figure 1. Prediction of black and white criterion scores from the white regression equation when $\epsilon X_W - \epsilon X_B = \epsilon Y_W - \epsilon Y_B = 1SD_x = 1SD_y = 10$. $\epsilon \hat{Y}_W - \epsilon \hat{Y}_B = 1SD_{\hat{y}}$.

cations are explained and illustrated.[2] In addition, data from a number of published studies on test bias are examined in light of these definitions.

It should be pointed out that the concern here is with the test bias, not with differential validity. When a test is not related to the criterion of interest within a population subgroup (i.e., has zero validity for the subgroup), there is no justification for its use with that subgroup. Of course, if such a test is used, it may, under certain circumstances, result in unfair discrimination against one or more population subgroups (Bartlett & O'Leary, 1969). The concern in the present article, however, is with unfair bias that may exist in tests that are approximately equally valid for both majority and minority groups. Approximately equal validity for psychological tests within population subgroups appears to be the general rule. A recent study (Schmidt, Berner, & Hunter, 1973) has shown that when differences between the races in sample sizes used are taken into account, the frequency with which employment tests are reported in the literature to be valid for one race but not the other is no greater than would be expected on the basis of chance alone. It is likely that a similar situation obtains in the educational setting (Anastasi, 1968, pp. 561–562; Stanley, 1971). Tests equally valid for blacks and whites are equally successful in properly rank ordering individuals *within* both races on future criterion performance; however, they may be unfairly biased in that they place one race *as a group* too high or too low relative to the other.[3]

Closer Examination of Cleary and Thorndike Definitions

Consider the test-criterion relationship shown in Figure 1. X is a test used to predict a criterion variable, Y. Values of Y predicted from a regression equation are symbolized $\hat{Y}$. The symbols r_{xy} and SD will be used for both population and sample values, although discussion is essentially in terms of relations existing in the population. In order to simplify the presentation, the following assumptions are made: r_{xy} and marginal SDs (SD_x and SD_y) are assumed equal within and between cultural groups; conditional SDs of the criterion ($SD_{y \cdot x}$) are assumed equal between cultural groups and within each group across levels of X. These assumptions imply equal regression slopes in each subgroup.[4] The criterion

[2] A third concept of test bias, Definition 3 of Darlington (1971), is not examined in this article. This definition holds that a test is fair only when $r_{ex.y} = 0$ (where e = ethnic group membership, x = test score, and y = the criterion measure). It is stated in terms of a partial r in which the variable partialed out is (or should be) assessed at a point later in time than the other two variables. Thus, it is very difficult to assess the quality of this definition in reference to the usual and most plausible assumptions about the causal relations among the variables of ethnic group membership, ability, and performance, all of which would be stated in terms of later performance being caused by earlier variables of ability, ethnic group membership, etc. Probably because of this conceptual problem, this definition has received little attention in the research literature. (The conceptual problems associated with Definition 3 will be explored more fully in a subsequent paper by the same authors.) Darlington's (1971) Definitions 1 and 2 correspond to the Cleary and Thorndike definitions, respectively. Darlington's Definition 4 corresponds to the stipulation (discussed above) that a test is fair only if it shows no mean score differences between population subgroups.

[3] It should perhaps be noted here that the much-publicized U.S. Supreme Court ruling (*Griggs v. Duke Power*, 1971) concerning employment tests was directed to the issue of test validity, not test bias. The effect of the ruling was to uphold the legality of the Equal Employment Opportunity Commission (EEOC) requirement that tests or other selection devices on which minorities average below the white majority be shown to be related to job performance. EEOC guidelines (U.S. EEOC, 1970) require that, where feasible, such validity be demonstrated separately for both majority and minority groups. The guidelines devote only a few lines to test bias per se. Although the treatment is somewhat unclear, unfair bias appears to be defined in terms of Cleary's (1968) concepts.

[4] Working under these assumptions precludes examination of unfair biases that might result from differences between groups in regression slopes, marginal and conditional variances, validities, or any combination of these factors. Possible effects of differences of this sort have been discussed elsewhere (Bartlett & O'Leary, 1969; Einhorn & Bass, 1971).

measure is assumed to be free of unfair bias and contamination,[5] and the joint distribution of X and Y is assumed to be bivariate normal in each cultural group. It is also assumed that $r_{xy} > 0$, because there is little point in discussing the cultural fairness of an invalid test.

In discussing Figure 1, the subscript W (for white) will be used to designate the majority group and B (for black) to designate the minority group.[6] Assume, then, $SD_{x_W} = SD_{x_B} = SD_{y_W} = SD_{y_B} = 10$. Although not shown, the black mean on Y in our illustration falls one standard deviation below the white mean, just as is the case on X, that is, $\epsilon Y_B = 40$ and $\epsilon Y_W = 50$ (where ϵ symbolizes the operation of taking the mean). If $r_{xy} = .50$, the white regression equation is $\hat{Y}_W = .50X + 25$. Now, $\epsilon Y_W = \epsilon \hat{Y}_W = 50$, by definition, but $\epsilon Y_B = 40 < \epsilon \hat{Y}_B = 45$. Thus, this test predicts an average criterion performance score of 45 for blacks, when in fact the average score earned by blacks is only 40. According to Cleary's definition, then, the test shown in Figure 1 is biased; it systematically overpredicts the criterion scores of blacks. But it can be seen that the position of the minority group relative to the majority group is the same on $\hat{Y}$ as on X and Y. Approximately 16% of the minority group is above the majority mean on X, Y, and $\hat{Y}$, despite the overprediction of black criterion scores. This obtains because $SD_{\hat{y}} = .50SD_x$ ($SD_{\hat{y}} = \beta SD_x$). The overprediction of black criterion scores is "compensated for" by the reduction in SD that occurs as we move from X to $\hat{Y}$. Despite the overprediction, then, a black with a given standing in the white X distribution, say the 50th percentile, will have that same standing on $\hat{Y}$. According to Thorndike's definition, this test is therefore unbiased because it assigns the same *relative standing* to the two groups on $\hat{Y}$ as they have on the criterion measure itself. If the selection ratio were such that all applicants scoring at or above the white mean (50 or above) were selected, approximately 50% of the white and 16% of the black applicants would be selected—exactly the same percentage of each group that would in fact be above the white mean on the criterion, that is, on actual performance. Thus it can be seen that Thorndike's concept of test fairness leads to the paradoxical conclusion that in order to be fair, a valid test used for selection or admissions purposes must overpredict the actual criterion scores of members of population subgroups with lower mean criterion scores.

TABLE 1

Percentage of Minority Group Applicants Accepted under Different Selection Criteria

Selection ratio	Criterion (Y)	Cleary $\hat{Y}$	Thorndikean $\hat{Y}$
Top one sixth	3.14	.22	3.14
Top one third	9.68	1.19	9.68
Top one half	19.77	3.12	19.77
Top two thirds	34.83	10.93	34.83

Note. Percentage of minority in applicant pool = 20%.

On the other hand, if one follows Cleary's definition and considers the test depicted in Figure 1 to be biased, he would make use of the test "fair" by computing and using a separate regression equation for blacks. The intercept for this equation would be 20 and $\epsilon \hat{Y}_B = \epsilon Y_B = 40$; that is, the equation would neither underpredict nor overpredict for blacks. Figure 2 shows that when separate minority and majority regression equations are used with the test depicted in Figure 1, $\epsilon \hat{Y}_W - \epsilon \hat{Y}_B = 2SD_{\hat{y}}$ rather than $1SD_{\hat{y}}$ ($SD_{\hat{y}}$ remains unchanged at 5), and thus only about 2.3% of the minority group members reach or exceed $\epsilon \hat{Y}_W$. If one could select on the criterion itself (or on a perfectly valid test), and if the cutoff score were ϵY_W, approximately 16% of the minority group members would be selected. Use of test X with separate regression equations results in the selection of only 2.3% of the minority applicants. The dramatically different implications of the Cleary and Thorndike definitions can be seen in Table 1. For all selection ratios, selection on the basis of a test meeting Cleary's definition of fairness results in the acceptance of a markedly smaller percentage of minority applicants than selection on a test meeting Thorndike's definition. Thus these two concepts of test fairness can not only lead to opposite conclusions about the fairness of a given test usage, they can also have very different practical implications.

Holding the differences between the groups on the criterion constant, the larger the validity coefficient, r_{xy}, the smaller is the overprediction of black cri-

[5] As has been pointed out repeatedly (Cleary, 1968; Guion, 1966; Thorndike, 1971), if the criterion measure is unfairly biased to an unknown degree and in unknown directions, no meaningful analysis of test fairness is possible. For this reason—and for others more obvious—great care should be taken to insure that racial or ethnic prejudice or favoritism do not contaminate performance evaluations made of individuals. Although this may be difficult in certain situations, it is not impossible (e.g., see Grant & Bray, 1971).

[6] Obviously blacks are not the only minority group showing a tendency to score below the white mean on psychological tests. Symbols used here were chosen for convenience and because the bulk of the published research in this area has focused on black–white comparisons.

TABLE 2

Conditions under Which Cleary and Thorndike Concepts Are Equivalent and Nonequivalent

Condition	Validity	
	$r_{xy} = 1.00$	$r_{xy} < 1.00$
Criterion performance[a]		
Equal	Identical	Identical
Unequal	Identical	Different

[a] For population subgroups.

terion scores necessary to meet the requirements of the Thorndike definition. When validity is 1.00 for both groups, no overprediction is necessary, and Thorndike's definition of test bias becomes identical to the Cleary definition. Holding test validity constant, the smaller the between-groups difference on the criterion, the smaller the overprediction of black criterion scores necessary to meet Thorndike's definition. When the two groups do not differ on criterion performance, no overprediction is necessary, and the Thorndike and Cleary definitions are once again identical. Table 2 perhaps clarifies these distinctions somewhat. In three of the four quadrants, the two definitions are identical, but two of these three essentially do not occur in reality; there are no known reports of tests with perfect validity. The situation in which there are no subgroup differences in criterion performance occurs with some degree of frequency, especially in employment settings (Kirkpatrick et al., 1968; Lopez, 1966; Rhode, 1971). But the situation in which test validity is less than perfect *and* in which there exists a between-groups difference in criterion performance is probably the most common, especially in educational institutions (Anastasi, 1968, pp. 561–562; Cronbach, 1970, pp. 302–307). The Cleary and Thorndike definitions of test fairness will therefore usually be in conflict.

Reanalysis of Published Data

Most research on the question of test fairness has been based on the Cleary definition and, whether carried out in employment or educational settings, has found a tendency for regression equations developed on white or combined samples to overpredict the actual criterion performance of blacks. In the educational setting, the best known and most quoted of these studies is that of Cleary (1968) herself. Using Scholastic Aptitude Test Verbal and Quantitative scores as predictors, she compared actual and predicted grades of black and white students attending three integrated colleges. In all three of these colleges, actual grades of blacks were lower than predicted grades, and this difference was significant in one of the colleges. Temp (1971) found that the grades of blacks were overpredicted, in varying degree, by the regression equation developed on whites in 12 of the 13 colleges he studied. Other educational studies have reported similar results (Kallingal, 1971; Pfiefer & Sedlacek, 1971; Wilson, 1969). Similar findings have been reported for the employment setting (Campbell, Pike, & Flaugher, 1969; Gael & Grant, 1972; Grant & Bray, 1970; Guinn, Tupes, & Alley, 1970; Tenopyr, 1967). Following Cleary's definition, these researchers have almost invariably concluded that the tests and regression equations investigated were either fair to both races or were biased significantly in favor of blacks. But, as seen above, a test meeting Thorndike's definition of fairness will of necessity show overprediction of minority criterion scores in most situations.[7]

With this consideration in mind, data from two educational (Cleary, 1968; Temp, 1971) and two employment (Gael & Grant, 1972; Guinn et al., 1970) studies were reanalyzed using the criterion of fairness proposed by Thorndike.

Assuming normal distributions for both population subgroups on Y and $\hat{Y}$, Thorndike's definition requires that the standard score in the Y_B distribution corresponding to ϵY_W be equal to the corresponding standard score in the $\hat{Y}_B$ distribution for $\epsilon \hat{Y}_W$. Given ϵY_B, SD_{y_B}, and ϵY_W, the standard score in the Y_B distribution corresponding to the ϵY_W was computed, and the percentage of minority group members falling above this score was obtained from an ordinary normal curve table. Using $\epsilon \hat{Y}_B$, $SD_{\hat{y}_B}$, and $\epsilon \hat{Y}_W$, the same thing was done for the $\hat{Y}$ distribution.[8] (All $\hat{Y}$s were, of course, those produced using the white-derived regression

[7] Significantly, the three studies that reported findings of unfair bias *against* lower scoring minority groups did not work from Cleary's concept of bias (Kirkpatrick et al., 1968; Lopez, 1966; Rhode, 1971). In these studies, the quantities $\epsilon Y_W - \epsilon Y_B$ and $\epsilon X_W - \epsilon X_B$ were compared by simple observation or with respect to differential statistical significance. These procedures are equivalent to application of the Thorndikean concept of test fairness.

[8] $SD_{\hat{y}_B} = r_{xy}$ (or the multiple correlation coefficient) times SD_{y_B}. Where r_{xy} or the multiple r was not given, as in Cleary's data, $SD_{\hat{y}_B}$ was computed as $B'C_{xx_B}B$, where B = the majority equation regression weights and C_{xx_B} = the test variance–covariance matrix for the minority group.

equation.) Then for each test, the difference between these two percentages was tested for significance. The Thorndikean test for unfair bias can thus be seen to be quite simple and straightforward.

It can be seen in Table 3 that in all three of the colleges studied by Cleary, the percentage of blacks above the white mean on $\hat{Y}$ was significantly smaller than the percentage above the white mean on Y. This discrepancy was as great or greater for Colleges 1 and 2 as for College 3, though College 3 was the only college in which the overprediction of black grades was statistically significant. The results are similar for the data from Temp's (1971) study. In 12 of the 13 colleges he studied, the trend, according to the Thorndike definition, was in the direction of unfair bias against black students, and in 8 of the colleges this effect reached statistical significance. This occurred despite the fact that in 10 of the 13 colleges, actual black grades were significantly overpredicted. In both studies, the white regression equation was overpredicting for blacks but not to a great enough extent to make the tests fair under the Thorndike definition.

TABLE 3

Results of Thorndikean Bias Analysis for Data from Two Educational Studies

College	N	r[b]	% of blacks above white mean on			
			Y	$\hat{Y}$	SAT–V	SAT–Q
Cleary (1968) data						
1	B-59 W-118	—	29.12	15.87**	24.20	26.76
2	B-83 W-365	—	42.07	7.78**	40.52	23.58**
3[a]	B-131 W-258	—	15.39	2.81**	8.38*	8.69*
Temp (1971) data						
1[a]	B-100 W-100	.27	23.58	10.93*	16.60	12.51
2[a]	B-98 W-99	.23	15.87	14.01	15.87	16.60
3[a]	B-104 W-104	.38	17.11	10.20	4.01**	1.43**
4[a]	B-92 W-93	.33	16.35	3.29**	8.08	2.87**
5[a]	B-140 W-140	.55	11.31	11.70	9.34	4.65*
6[a]	B-106 W-92	.39	19.49	5.48**	7.78*	10.03
7[a]	B-99 W-100	.15	15.87	1.79**	4.09**	3.59**
8	B-100 W-97	.51	20.90	6.18**	6.68**	6.43**
9[a]	B-100 W-100	.45	15.87	6.43*	7.21*	6.18*
10[a]	B-100 W-95	.25	11.51	2.62*	5.94	4.46
11	B-68 W-69	.43	30.50	16.85	17.11	14.92*
12	B-39 W-100	.49	29.12	11.31*	8.38**	21.48
13[a]	B-104 W-109	.46	30.85	24.83	25.46	12.92**

Note. Abbreviations: SAT = Scholastic Aptitude Test; V = Verbal; Q = Quantitative; B = blacks; W = whites.
[a] Significant overprediction of black grades; $p < .05$.
[b] Multiple correlation for blacks.
* Significantly smaller than percentage in Y column; $p < .05$.
** Significantly smaller than percentage in Y column; $p < .01$.

TABLE 4

Results of Thorndikean Bias Analysis for Data from Two Employment Studies

	N	r[a]	% of blacks above white mean on	
			Y	$\hat{Y}$
Guinn, Tupes, & Alley (1970) data				
Technical area				
1	B-142 W-1989	.36	41.29	19.22**
2[b]	B-180 W-2331	.45	28.43	17.62**
3	B-148 W-1535	.32	40.52	38.28
4	B-159 W-2024	.22	46.81	40.52*
5[b]	B-444 W-2085	.34	26.11	24.20
6	B-150 W-3138	.43	39.74	30.15*
7[b]	B-210 W-2322	.40	32.64	28.10
8[b]	B-70 W-741	.39	40.90	37.45
8[b]	B-70 W-741	.28	40.90	16.60**
9	B-109 W-1476	.44	30.50	22.66*
10	B-19 W-462	.40	29.81	35.94
Gael and Grant (1972) data				
Equation				
1	B-97 W-184	.28	35.57	22.96*
2[b]	B-106 W-193	.33	35.57	23.58*
3[b]	B-97 W-184	.22	35.57	37.83
4[b]	B-101 W-184	.18	35.57	50.00†

Note. Abbreviations: B = blacks; W = whites.
[a] Black performance significantly overpredicted; all $p < .05$.
[b] There were two Aptitude Indices for this technical area.
[c] Multiple correlation for blacks.
* Significantly smaller than percentage in Y column; $p < .05$.
** Significantly smaller than percentage in Y column; $p < .01$.
† Significantly *larger* than percentage in Y column; $p < .01$.

It can be seen in Figure 1 that when the white equation is applied to both groups, percentage of overlap on $\hat{Y}$ is identical to that on the test or test composite. Thus, the source of "unfair bias" is to be found in the tests themselves; it is not an artifact of the regression technique. The last two columns in Table 3 present the Thorndikean bias analysis for the actual predictors used in the Cleary (1968) and Temp (1971) studies, the verbal and quantitative sections of the well-known Scholastic Aptitude Test. The general trend is for the tests themselves to be somewhat less biased than the $\hat{Y}$

composite,[9] but in every case the difference between overlap on Y and on X was in the direction of Thorndikean bias against black students. In 17 of the 32 comparisons, this effect reached significance.

In the two employment studies, results were similar. Guinn et al. (1970) used various Air Force Aptitude Indices as single tests in the prediction of final grades for white and black airmen in each of 10 technical training courses. Black grades were overpredicted in all 10 technical areas, and the overprediction was statistically significant in 3 of the groups. But, as can be seen in Table 4, there was nevertheless a trend toward unfair bias against blacks in the Thorndikean sense in each of the 10 technical areas, and in 6 of the areas this effect reached significance. Thorndikean bias was not invariably less in the technical areas showing significant overprediction of black grades than in the other areas. Also shown in Table 4 are results from the Gael and Grant (1972) study. Each of the four regression equations presented for this study contains a single predictor, and all equations are those derived on the white subgroup.[10,11] The performance criterion was a carefully developed composite of a job knowledge test and several job sample measures. In each case, the white regression equation overpredicts black criterion scores, but the first and second equations are nevertheless significantly biased against blacks according to the Thorndike definition. Equation 3 shows no significant bias, and the fourth equation is significantly biased in favor of blacks.

The Two Definitions: Practical Implications

Obviously, then, the two definitions of test bias can and do lead to very different conclusions about the fairness of tests used in everyday life to make decisions about people. Adoption of the Cleary definition leads to the conclusion that many educational and employment tests, as used, are probably either fair to, or biased in favor of, lower scoring minority groups; acceptance of the Thorndike definition, on the other hand, means that many currently accepted test usages are probably unfair to such minority groups.

But there are implications beyond this one. The Cleary definition is based on the principle of least squares estimation of criterion scores; as such, it insures that the average performance level of selectees will be maximized. College students, workers, or management trainees selected under the Cleary definition will, on the average, show higher levels of performance than those selected using the Thorndike definition. There can be little doubt that the maximization of the individual-occupational role fit which results from application of the Cleary definition is of great benefit to the individual in terms of his educational and occupational success and satisfaction in life and to society in terms of performance maximization, not only of individuals but also of social institutions as institutions. Simple statistical principles can be used to demonstrate that adoption of Thorndike's definition of test fairness leads to a greater incidence of placement of individuals in educational and occupational roles for which they are unsuited psychologically. Conversely, use of the Thorndike definition means that certain majority applicants will be rejected in favor of minority applicants with lower statistical probabilities of success on the criterion—a situation that would be considered reverse discrimination by many.

There is a final practical problem associated with the use of the Thorndike definition. Thorndike's definition is based essentially on probability matching; it states that, for a given definition of adequate or successful performance, the proportions of the minority and majority subgroups chosen should correspond to the probabilities of success for individuals drawn at random from each of the groups. If lower scoring ethnic groups other than blacks, Chicanos, Indians, etc., were to demand these same probability matching privileges for themselves, and if probability matching were imposed on higher scoring and performing ethnic and social groups (where it would *reduce* chances of selection or admission), much if not most of the societal benefit accruing from the use of valid selection procedures would be lost. In fact, if lower scoring members of the majority group defined themselves as a group and demanded and received probability matching privileges, differential selection and placement would become a virtual impossibility. For example, consider again

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[9] $\hat{Y}$ is a composite of both tests and therefore somewhat more reliable than either test taken individually. Linn and Werts (1971) have shown that, under these circumstances, overlap of two distributions with dissimilar means will be smaller for the more reliable measure.

[10] It should be apparent from the previous analysis that degree of Thorndikean bias against blacks would have been even greater had the black-derived regression equation been used to predict for blacks and the white-derived equation used for whites. The prediction situation would then have been similar to that shown in Figure 2 instead of Figure 1.

[11] The predictor in the first equation is the raw score sum of the two most valid tests. Equations 2, 3, and 4 were not presented as such in the study; they were calculated from information given.

the situation illustrated in Figure 1—in which the test is fair to the minority group under Thorndike's definition and the minimum score for selection is 50. There is nothing to prevent majority group members who score below 50 from maintaining that the test is unfair since the percentage of their group selected (0%) is less than the percentage that would in fact be successful if all in their group were selected (33%).

On the other hand, in many situations, application of the Cleary definition means that the probability of acceptance for minority group individuals is reduced essentially to zero. When validity, though statistically significant, is relatively low and when the difference on the criterion favors the majority group, the Cleary definition is particularly disadvantageous to minority groups. Figure 2 can be used to illustrate a situation of this sort. The reader is asked to consider the effect of lowering the validity of the test shown in Figure 2 from .50 to .30. The new regression equations become $\hat{Y}_W = .30X + 35$ for whites and $\hat{Y}_B = .30X + 28$ for blacks. $SD_{\hat{Y}}$ is now 3.0 instead of 5.0, and the white and black means on $\hat{Y}$ are now separated by 3.33 instead of $2.00SD_{\hat{Y}}$s. Under these circumstances, if the white mean on $\hat{Y}$ (i.e., predicted criterion performance of 50) were once again used as the selection cutoff score, the probability of being selected or admitted for minority group members would be less than .0005.[13] Stated another way, whenever performance differences favor the majority group, the lower scoring minority group *as a group* is disadvantaged in inverse proportion to the validity of the test—a situation that might plausibly be argued to be unfair.

Although both concepts of test fairness have advantages, the disadvantages of each are perhaps more obvious. It might appear, then, that the best solution would simply be the elimination of educational

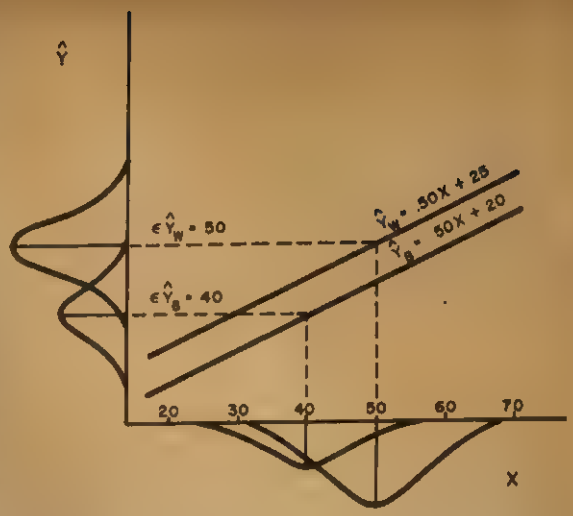

Figure 2. Prediction of black and white criterion scores with separate regression equations when $\epsilon X_W - \epsilon X_B = \epsilon Y_W - \epsilon Y_B = 1SD_x = 1SD_y = 10$. $\epsilon\hat{Y}_W - \epsilon\hat{Y}_B = 2SD_{\hat{Y}}$.

and employment selection and placement tests. This solution, however, is no solution at all. Whether tests are used or not, decisions must be made about people. A given college must decide which applicants will be admitted and which will not; usually not all can be admitted. A personnel director must decide who will be hired onto a specific job and who will be rejected; typically there are more applicants than positions. If tests are not used as aids in making these necessary decisions, then other devices—such as the interview, letters of reference, and subjective hunch—must be heavily weighted. The record of validity for such procedures is a poor one relative to that of psychological tests (Anastasi, 1968; Ghiselli, 1966; Mosel & Goheen, 1958; Ulrich & Trumbo, 1965). But more important for the present discussion is the fact that these procedures—and other alternative procedures—are characterized by the same problems of potential unfair bias discussed above in relation to tests. In the selection setting, there is no essential conceptual or practical distinction that can be drawn between test scores and scores or ratings produced, for example, by the interview. Both are potentially susceptible to biases of various kinds, the interview perhaps more so than the typical psychological test (Anastasi, 1968, p. 563; APA Task Force on Employment Testing, 1969). Obviously, then, assessments of applicants produced by the interview, references, and similar procedures can and should be analyzed for unfair

[13] Although separate regression equations will usually be theoretically appropriate under Cleary's definition, it should be noted that the existence and use of a separate (lower) equation for the minority group is not a necessary condition for this effect. For example, if test validity were .30, mean black criterion performance (ϵY_B) were 41 and mean black test performance (ϵX_B) were 20, then (with all standard deviations again equal to 10 and mean white performance on both test and criterion again at 50) the regression equation $\hat{Y} = .30X + 35$ would be appropriate for both races. Because it would neither underpredict nor overpredict for either race, the test (used with a single regression equation or in raw score form) is unbiased according to the Cleary definition. But while 18.4% of blacks would be above the white mean on actual performance (Y), less than 1% would be above the mean on predicted performance ($\hat{Y}$).

racial and ethnic bias.[13] And in order to do this, there must be some agreement as to what constitutes unfair bias. Of course, the problem can be covered over and concealed by deliberately keeping all assessments, both of performance capability and of actual performance, is unquantified form. But it should be obvious that obscuring the problem does not eliminate or solve it.[14]

There appears to be no real alternative to a serious and intelligent consideration of the relative merits of different definitions of selection fairness.

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[13] In this connection, it should be noted that EEOC validation requirements are not limited to tests; the same evidence of practical validity required for tests is required for nontest procedures such as the interview, references, etc.

[14] Such assessments *must* be put into quantitative form, however crudely, in order to meet EEOC validation requirements. (See Footnote 13.)

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The Politics of Empiricism

Research Recommendations of the Joint Commission on Mental Health of Children

NORMAN H. HAMM *University of Nebraska at Omaha* [1]

The final report of the Joint Commission on Mental Health of Children (1970) is a provocative social mandate for America's children, calling for a general reorganization of the child mental health enterprise around *prevention* rather than the traditional *treatment* of mental disorders once they occur. Accordingly, a fourfold approach is forcefully presented: (*a*) the development and implementation of a child advocacy system; (*b*) a comprehensive health, support, and remedial care system for children, particularly for the socially disadvantaged; (*c*) expanded, individually based, applied and basic research on child mental health; and (*d*) new approaches to the development of mental health manpower. Additional chapter summaries deal with aspects of poverty, racism, employment, education, and the relation of these aspects to child mental health.

While the four general recommendations are obviously interrelated and difficult to separate, this article will critically examine the efficacy, training, and administration of research in terms of meeting the goal of children's mental health.

The author's method is limited, if not indeed somewhat paradoxical. What is offered is a *rational analysis* of our empirical enterprise as it applies to practical payoff, especially mental health. What surely is needed is a science of science (not to be confused with a philosophy of science), or research performed on research policy; however, in the profound absence of such knowledge, one can only guess and offer hypotheses about which principles will produce the most scientific progress.

The Efficacy of Research on Child Mental Health

Although the report places no monetary value on any of its recommendations, research is one of the four major areas with which it deals. The Commission recommends increased support of both pure and applied research together with the establishment of 10 child mental health research centers under the auspices of the National Institute of Mental Health or the National Institute of Child Health and Human Development. Such a conceptual, if not monetary, acknowledgment presumes that scientific research can have a major impact an children's mental health. Such an assumption is clearly an important one, particularly for a preventive approach to mental health.

An analysis of presumed research payoff has two levels, one fairly simple and the other quite complex. Regarding the former, adequate evidence already exists regarding the variables, particularly early experiences, that greatly influence mental health. At the present time, a psychological prescription could be written during childhood which would minimize the causes of mental illness later in life. Knowledge of proper environmental dosages may not be as exact as in the practice of internal medicine, but they nevertheless exist. Additional research of the type recommended in the Commission's report would undoubtedly add greatly to the precision of mental health prescription.

The assumption becomes inexorably complex when consideration is given to whether society will use such information in a preventive manner. While the actual choice of therapeutic procedures may be made on a personal moral basis instead of an empirical one (see Baer, 1970; Johnston, 1972; Solomon, 1964), mental health practitioners would probably implement new knowledge as it becomes available. Their professional consciences would

[1] Requests for reprints should be sent to Norman H. Hamm, Department of Psychology, University of Nebraska, P.O. Box 688, Omaha, Nebraska 68101.

probably dictate change. In the case of the lay public, however, knowledge concerning mental health may continue to be largely unused. The question is whether the public will use the results of sound behavioral research.

Some evidence exists on both sides. Many influential individuals, including the Vice-President of the United States, have deplored the creeping influence of the behavioral scientist and engineer into contemporary life. The loss of autonomy, resistance to a deterministic view of life, and the increasing predictability of behavior do not appeal to large segments of the lay public. In addition, the argument commonly used against B. F. Skinner's *Walden Two* concerns the totalitarian organization of the community, with behavioral engineers, instead of politicians, on the top of the social pyramid.

If the prescription for child mental health were written on the national level, it is somewhat doubtful whether a substantial number of parents would follow it. The real negative consequences of inadequate child rearing have little impact on the family until adolescence and adulthood. Moreover, a substantial number of parents are emotionally incapable of providing the nurturance necessary for the mental health of their children. They have too many of their own problems with which to contend.

Nevertheless, there is some reason for optimism. The strategy of childhood prevention made possible by increased research has the potential of largely eliminating our generally nonproductive psychiatric population. The logic is appealing, if not the actual dollar consequences.

Some signs point to the increasing social usage of research findings. The recent social legislation concerning the advertising of cigarettes and also the limiting of violence in children's cartoons demonstrate that some social action is already possible; both involved substantial policy changes based on the findings of scientific research, the former from medicine and the latter from psychology. Not all research results may be translated into governmental policy, however. It is likely that the results of empirical research will have little social impact when they conflict with social taboos such as the recent report of the Commission on Sex and Pornography. The scientific evidence unfortunately conflicted with society's prejudices.

The willingness of the public to support and use the information gathered by extensive and expensive research on child mental health has yet to be firmly established. Before larger sums of money are spent on research, the public must be consulted. After all, it is the taxpayer who must pay the price, while we professionals earn our keep. Under such circumstances, it is difficult to be objective. As a profession, we must guard against the tendency to impose our cherished values on the nonprofessional public. It may be hoped that the advocacy portion of the Commission's recommendations will effectively present the case for children's rights and mental health. Once these rights are accepted by society, then professionals can duly serve as the technical experts necessary to meet their implementation.

Research Policy and Administration

After the absolute amount of dollars is assigned to research concerned with human development, important questions arise concerning research policy and implementation. The overall question is which set of policies offers the most preventive payoff. On this point the Commission takes a strong, but disappointingly, traditional stand.

Except for the recommendation concerning the establishment of 10 child mental health research centers throughout the country, most of the Commission's recommendations support the research status quo. Briefly, both basic and applied research are necessary and should be increased; policy should maximize the "opportunities" and "productivity" of the individual researcher, especially as related to the university's role in basic research; appropriately designed applied research should be increased to assess social action programs; multidisciplinary collaboration is encouraged; and NIMH should sponsor, under its Clearinghouse function, more adequate information retrieval and dissemination to professionals. The last recommendation has been noted by others (Etzioni, 1972), and the Interagency Panel of Early Childhood Research, established in 1970, has begun to coordinate child development research of nine agencies under the Department of Health, Education, and Welfare. A similar interagency panel is being established for adolescence.

Below the level of policy, topical priority is given by the Commission to longitudinal, multivariate, life-span studies of human development, methodological research on child development and mental health, and specific studies dealing with early infantile autism, childhood schizophrenia, the natural

history of emotional disturbance, and the effectiveness of various intervention procedures on mental illness, institutionalization, and drug use. The latter studies are to be conducted in the 10 child mental health research centers.

Obviously, there is much that is right about the research policies and specific study areas recommended by the Commission. The specific study areas in particular are soundly conceived; they represent significant problem areas related to child mental health, and the methodological issues raised are premised on developmental research performed in the last several decades. At the principle level, however, the Commission does not go far enough in redirecting our national research policy.

The basic problem with current research administration is *accountability*. Increasingly, in recent years, I have become disenchanted with *individually* sponsored research within *university settings*. Several recent writers have noted the basic incompatibility between university settings and mission-oriented, interdisciplinary research (Harris, 1968; McElroy, 1971; Piel, 1969). While I have every reason not to be an iconoclast, speaking as a former graduate research assistant, predoctoral research fellow, and principal investigator myself, I feel compelled to kill the goose that laid the golden egg.

It is not that scholars should divest themselves of scholarship, but that governmental research institutes, either on or separate from university settings, might be scientifically more productive. The primary problem is that professors and students can be only part-time researchers. They both have other important duties. Professors have teaching, university administration, and community service to tend to, while maze-running, hurdle-jumping graduate students must give considerable (if not full) attention to their academic standing.

The counterargument is often posed that research activities complement and overlap professors' and students' other duties. This argument rings in the hallowed halls of academia today. The overlap, however, is imperfect, and no matter how talented the individuals, their nonresearch responsibilities can only subtract from the time available for the generation of new knowledge. It is still only in the providence of the renaissance man to make substantial contributions to all four areas: research, university administration, community service, and teaching.

The administratively wasteful policy of supporting part-time researchers is undoubtedly reflected in any cost–productivity analysis. To be sure, scientific research would be expensive under any set of policies. However, my experience indicates that the cost per published study is substantially increased by funding projects to individual, part-time researchers. Perhaps science cannot be conceived in terms of a cost–productivity analysis; indeed, some studies take considerable thought and time for execution and may significantly contribute to knowledge, but the business world should afford such extravagance! There are too many videotape recorders, slide projectors, research trailers, and voice keys accumulated by part-time researchers, which are used for few days or months a year—mine included.

Nothing short of drastic action is needed. In this regard, the advantages of centralization and full-time researchers recommend the European model of university- or non-university-related technical institutes. Certain of these institutes, like the 10 child mental health research centers recommended by the Commission, would deal with specific problem areas. Most would be mission oriented. All employees would be full time, but in recognition of the intimate relationship between higher education and research, a certain percentage would rotate between academia and the institutes. Instead of profit as a product, numbers of published articles, coupled with their scientific significance, would be given administrative consideration. Reinforcers such as salary, promotion, and administrative positions, would be contingent on the scientist's actual accomplishments. Finally, equipment usage would be recorded and policies of equipment sharing established among institutes, particularly regarding the most expensive pieces of apparatus.

In short, the present author would like to go beyond the recommendation of 10 child mental health research centers and substantially cut back on individually funded grants to mainly university personnel. The research dollar would likely be lengthened by the European technical institute concept together with accountable administrative procedures which reward individual scientists for productivity. In the end, however, an emerging body of empirical data concerning the efficacy of various research policies should buttress the administrative procedures of government agencies. What is needed is a science of science.

Research and the Professional Practitioner

As a former half-time exile from experimental child psychology, doing applied research for a social action agency within a university setting, I have lived in Cronbach's (1957) divided world of experimental and applied psychology. The internal dissonance produced by this schism might have produced some satisfying personal growth and interesting soliloquies, if the immediate emotional consequences of frustration and conflict could have been removed. My experiences have been totally politicizing.

Despite Cronbach's careful documentation, it is my belief that the schism in psychology exists today, but for a deeper reason. Cronbach's claim is that the research procedures used by experimentalists differ substantially from those employed by practitioners; the experimentalists stress the mean, group homogeneity, and analysis of variance, while the correlationists rely on the standard deviation, group heterogeneity, and factor analysis. Both, however, base the acquisition of new knowledge on scientific empiricism.

The conflict between pure and applied social science is simpler and more general: It is an epistemological conflict that profoundly affects both the professional practice and interpersonal relations of the applied scientist. At the expense of alienating my philosopher colleagues, in practice there are essentially two ways of knowing: *rationalism* and *empiricism*. Experience has taught me that mental health practitioners frequently rely on the former, while experimentalists rely on the latter method of knowing. In some cases, the applied practitioner's rational method may even lead to a reliance on intuition; the constraint of dissonance among internal elements is thereby removed. In short, the conflict between experimental and applied psychology is not so much between different research tools, but whether empiricism or rationalism is supreme as a way of knowing. Psychology is a science, while applied psychology is at best part science and part art. When the practitioner does rely on empiricism he adopts the research tools outlined by Cronbach.

There is a practical advantage to empiricism which totally transcends its epistemological nature: A reliance on empiricism minimizes the potential for individual and professional conflict in applied, particularly social action, settings. The reason for this is that it provides all professionals with an objective means of evaluating the effectiveness of various policies, individuals, or programs. Evaluation is thereby taken out of the personal–political realm, and the emphasis is placed on the subject.

The consequences of an antiresearch bias on the part of practitioners are often tragic. With no objective means available for evaluation, many decisions regarding policy, individuals, or programs polarize the professional group and shift attention away from the children, placing it on staff relations. Staff morale sinks, work anxiety increases, certain key individuals resign, and the intervention program is doomed to failure. I have seen the pattern countless times and watched our valuable tax money wasted. An empirical orientation on the part of practitioners would not be a complete panacea, but the level of interaction among different professionals would undoubtedly be raised. The other alternative is to all become politicians.

The frequent reliance on rationalism by practitioners largely represents a failure of our professional training institutions. Students undoubtedly learn values along with their education. Many professional schools have either dropped or lessened their research requirements. If students have had adequate research training, they soon forget their research knowledge and retain only the skills that are used day to day. We need to not only bolster the research component of graduate education, but provide continued in-service training after graduation. Ideally, as recommended by the Commission, the professional staff should be continually involved in applied research and program evaluation. At the present time, huge sums of money are spent on social action with little knowledge concerning the effectiveness of the dollars budgeted.

A final note might be made of the Commission's child advocacy system, composed of neighborhood child development councils, local child development authorities, state-level child development agencies, and a national-level advisory council on children. This new consumer–professional coalition brings power to the people, involves nonprofessionals in child health programs, and, in general, increases the public's awareness of child mental health problems. This last factor in particular may do much to overcome the indifference toward child mental health noted in the first section. However, the dollar cost of such persuasion would be either prohibitive or better spent on the other recommendations concerning manpower training and child services.

The Future

In the end, it is hoped that changes in certain administrative procedures will do much to make up for America's decreased investment in research. During the early 1960s, America's expenditure for basic science increased by an impressive 16.2% per year (McElroy, 1971). However, the most recent federal growth rate is not large enough to even offset inflation.

Change is underway today. A research task force of the National Institute of Mental Health is engaged in a massive one-year review of the Institute's science activities. Ten study groups, composed of the Institute's major research interests, have been established. It is hoped that they will be successful in one of their principal objectives, the development of optimal administrative and organizational arrangements. What is needed is a coordinated program of research like that recently proposed for the environment (see Bowers, Hohenberg, Likens, Lynn, Nelkin, & Nelkin, 1971). As noted by McElroy (1971) and here, research policy, in all likelihood, will become more centralized, mission oriented, interdisciplinary, and perhaps less university centered.

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Children and Public Policy

A Role for Psychologists

CRAIG T. RAMEY *University of North Carolina, Chapel Hill* [1]

In 1970 the Joint Commission on Mental Health of Children published a report entitled *Crisis in Child Mental Health: Challenge for the 1970's.* The document is a plea for this country to reaffirm its concern for its young through meaningful actions and programs. The report was the product of three years of labor and the advice of "more than five hundred of the country's leading authorities on early childhood, adolescence and the young adult [p. iv]." Thirteen national professional associations, including the American Psychological Association, were represented on the Commission's Board of Directors.

The scope of the report reflects the diverse disciplines which were represented. Legal, medical, and educational reforms are advocated in an attempt to diminish the all too frequent occurrences of inequality of justice, care, and opportunity.

The report is a potential landmark in the establishment of adequate services to the children of this nation. Because many of the recommendations in it are controversial, it is desirable that the American Psychological Association, as an official body, recognize and respond to the various positions set forth. Individual psychologists are frequently called on by government and other public institutions to provide advice and counsel in matters concerning public policy for children. Yet, it is only rarely that psychologists seek to influence the course of public policy through actions by their professional associations.

By making a cohesive and collective response to this report we could make one of the most powerful responses that psychologists have yet made concerning formulation of public policy for children's programs.

The Commission's report advocates major reforms in areas such as physical and mental health services, employment, and education. Its major strength lies in its unflinching confrontation with many of the basic ills of our society, including disintegrating family life, injustice to minority groups, and lack of serious commitment to our children who are emotionally disturbed or mentally ill.

The report also has a weakness, albeit a very understandable one. The weakness is that in this attempt at comprehensiveness, priorities are not ordered, nor in many cases are specific mechanisms advocated to discover and to remedy the many deplorable life circumstances which affect our young. Although the members of the Commission are to be commended highly for their serious and difficult labor, the report would have been even stronger if such steps had been carried through.

In this article, two different issues are pursued. First, issues raised by the report are discussed and recommendations for APA's response are made. Second, a model is proposed which could be used to deliver many of the services that are advocated.

Responses to the Commission's Report

1. In developing positions on matters of public policy for children, APA should limit itself to those areas in which it can legitimately claim professional expertise. A guideline for this recommendation is that APA's official position regarding any public policy or program be limited to domains in which significant numbers of psychologists have made or are making direct professional contributions.

2. Before assuming an official position, the APA should conduct or make use of existing cost analyses for specific programs it feels to be legitimately within the domain of psychology. These analyses should include a careful delineation of manpower and facilities needed for effective implementation

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[1] Requests for reprints should be sent to Craig Ramey, Frank Porter Graham Child Development Center, University of North Carolina, Chapel Hill, North Carolina 27514.

as well as our best estimates of the costs to society associated with not implementing the advocated program. This suggestion is not made casually. Rather, it is made with the full realization that such efforts are costly in both time and money. Nevertheless, if we are to advocate specific courses of action, then it is incumbent upon us to demonstrate the desirability and feasibility of particular programs. Such analyses will help to formulate policy and to avoid unnecessary and destructive criticism of being utopian in goals or naive to the process of implementation.

As a result of the information made available through the cost analyses and information on alternative programs, the APA national staff could begin to establish tentative priority statements. These statements could include expert opinion on the probability of funding during current administrations and other pertinent observations. This information might be made available to APA members by mail, and open forums could be held on these issues at national and regional meetings. Following these discussions, mailed ballots could be sent to the general membership to determine specific courses of action.

3. During the process of priority setting it serves no constructive purpose to make derogatory comparisons between funding in nonmental health sectors and mental health programs. For example, comparisons deprecating the cost associated with military programs, as is done in the Commission's report, are probably unwise and should be avoided. This course of action is recommended not because there is no legitimate debate over the government's overall funding priorities, but rather because mental health programs can be justified on their own terms. Further, official comparison with other agencies engenders a needless and, in all probability, unproductive competitive stance toward much more established and powerful lobbies.

4. After priorities have been set, resources should be made available to representatives of APA effectively to inform legislative leaders of our policy decisions. Specifically, APA should either choose lobbyists from its own ranks or retain competent individuals from outside the membership.

Responses to Specific Issues

PREVENTION OF PSYCHOPATHOLOGIES

The Commission's report was very clear in its recommendation that prevention of emotional and mental disturbance receive a high priority. This is a position with which, hopefully, few psychologists will take issue. Although psychology possesses many techniques to effect remediation of mental and emotional disturbances, the expense of remediation in terms of professional time, money, and wasted human potential is probably much greater than adequate prevention programs.

Given that few psychologists would quarrel with an emphasis on prevention, it becomes necessary for the profession to determine which detrimental behaviors should be attacked first. Although it is beyond the expertise or desire of the present writer to rank order the problem areas on a priority basis, such an endeavor seems timely. Perhaps a panel of experts in psychopathology could be assembled under the auspices of APA to begin this awesome task. Several potential questions related to priority setting might include the following:

1. What proportion of the population evidences symptoms?
2. How handicapping is the pathology?
3. Is the course of the pathology progressive?
4. Is there a known etiology?
5. Are specific measures available which are likely to prevent its occurrence?
6. What resources are necessary to accomplish prevention?

CHILD ADVOCACY

The report recommends that a child advocacy system be created to guarantee the mental health of children. Specifically, it urges a federal-, state-, and local-level arrangement for advocacy of children's welfare. At the national level, the report suggests a presidential advisory council which

> would be charged with the responsibility of studying and gathering information on the problems of children and youth in the United States and with doing long-range planning, policy making, and programming, both for services and for manpower. This advisory body would be concerned about how well federal agencies are working together, competing, or overlapping in providing services. It would advise the President and the Congress on the effectiveness of programs and would make recommendations for legislative and program changes and in the allocation of monies spent for children and families [Joint Commission, 1970, p. 8].

In the process of developing a child advocacy system, the consumers of services must be heard. When children are unable to speak effectively for themselves, their parents will have to serve as first-line advocates and determiners of policy. It is

interesting to note that government is sensitive to this issue. Paul and Neufeld (in press), for example, have recently pointed out that in 1970, Public Law 91-517 established councils at the state and federal levels for the developmentally disabled. That law requires each state council to submit statewide plans for developmentally disabled children. Further, it specifies that one-third of the council's members at both the state and federal levels consist of consumers.

UNIVERSALLY AVAILABLE PRESCHOOL PROGRAMS

Pressures continue to increase for the establishment of year-round preschool educational programs. The demand for this service comes both from those who seek to ensure equal career development opportunity for women and from those who see such a service as a means to eliminate cultural-familial mental retardation. It is the latter issue which is particularly relevant to the mental health "crisis" alluded to in the report.

Evidence has existed for some time indicating that cognitive differences between low- and middle-income children begin to appear under three years of age (Bayley, 1965; Knoblock & Pasamanick, 1953). More recently, Wachs (1967) has reported significant social class differences in sensorimotor development in infants under one year of age, using the Piagetian-based Uzgiris-Hunt Scales of Infant Development.

If Bloom's (1964) idea that an organism is most easily changed during periods of rapid transition is valid, ought not we as psychologists seriously advocate programs which begin during early infancy and aim to prevent the onset of developmental impairment?

An examination of results from research-oriented day care and preschool programs is encouragement for the universal availability of such programs. Results from both center-oriented and home-oriented preschool programs indicate that disadvantaged children who participate in such programs show superior intellectual performance relative to nonparticipating children (e.g., see recent reports by Gordon, 1972; Heber, 1971; Klaus & Gray, 1968; and Levenstein, 1970). These projects, and others which currently exist, are probably best regarded as pilot projects rather than as definitive evidence for the beneficial effects of early intervention. Nevertheless, the bulk of the evidence certainly seems to indicate that the opportunity to participate in such programs is advantageous for the disadvantaged child, at least while he participates in the program. Thus, in principle, the recommendation by the Commission to support high-quality preschool education, particularly for our disadvantaged children, seems sound and prudent.

INCREASED SUPPORT FOR RESEARCH

It is not unlikely that in an article of this type there should be a plea for expanded support for research. Yet, the recent trends in research funding at the federal level for basic and applied research seem to make such a request both timely and necessary. It is almost inconceivable that at a time when this country continues to grapple with major problems in education, desegregation, poverty, and alienation that there appears to be a reduced commitment to the investigation of these conditions. Had we as a nation succeeded in remedying these malevolent situations, then reduced support for research in these areas would be warranted. Such is not the case, however. The "crisis" which was acknowledged in the Commission's report in 1970 has not been passed. Indeed, a good case could be made that the intensity of the crisis has increased.

To combat successfully the forces which threaten the well-being, if not the very life, of our society, research should be fully supported in at least the following three areas.

First, nationwide epidemological studies should be undertaken to document precisely the extent to which the young of our nation are thwarted in the full realization of their developmental potential.

Second, more adequate assessment and evaluation of action-oriented social programs should be undertaken immediately. In the future, detailed evaluation strategies should be planned from the very beginning. It is naive to believe that all, or even most, pilot or demonstration projects will be successful in ameliorating the adverse effects of deplorable social conditions. However, it is only through the careful scrutiny of adequately collected data that those programs which are effective can be identified and their successful components disseminated.

Third, basic multidisciplinary research into the causative factors underlying unrealized potential should receive increased support. Realizing that a variety of levels of explanation are needed to understand fully social phenomena, encouragement and incentives should be provided for more collaborative multidisciplinary research efforts.

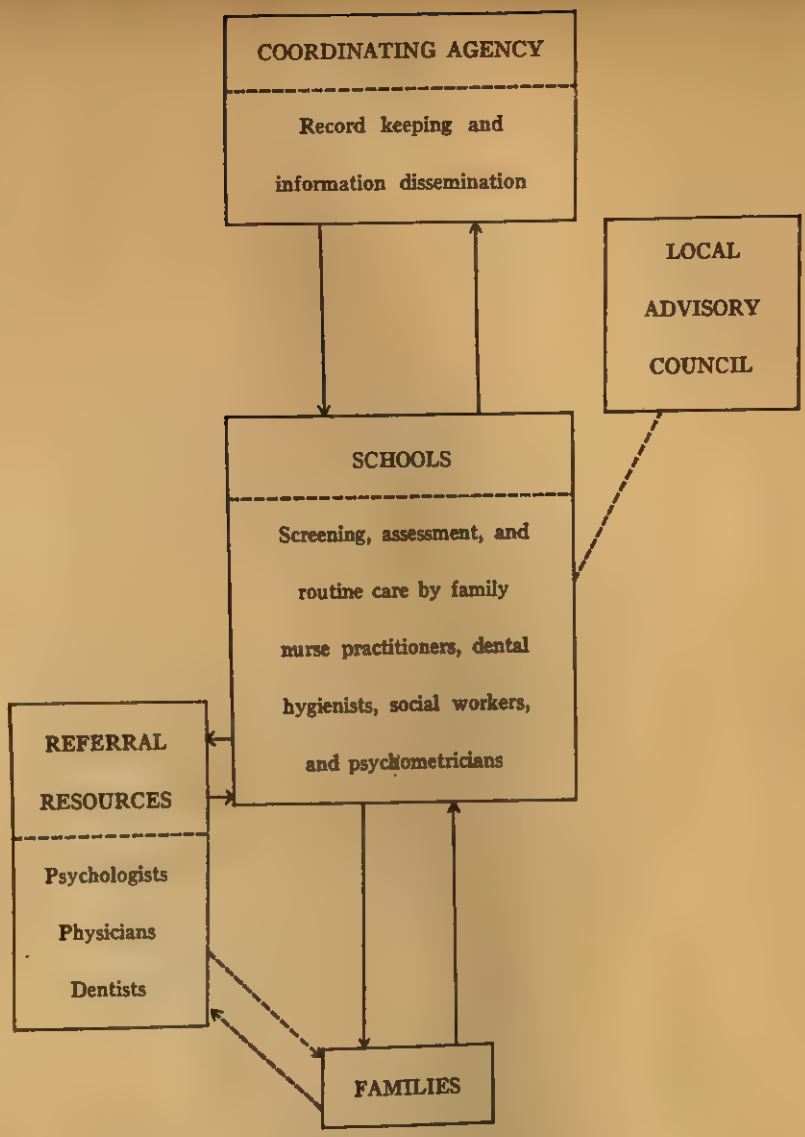

Figure 1. A delivery system for health services.

A General Model for Delivering Services to Children and Families

The main function of this system is to monitor the physical and mental health of this country's children and to provide necessary preventive and remedial services. A child would be enrolled in this system during his mother's pregnancy and would remain in it until adulthood.

The major premise on which the model to be presented rests is that a truly effective delivery system will have to be national in scope. Given that this nation's population is highly mobile geographically, it is mandatory that a system be devised which will be able to maintain contact with transient as well as permanently located families. Because geographical mobility shows no signs of abating, the ability to serve this wandering population in the future will probably be even more difficult to maintain.

The following are major problems which should be considered by any truly comprehensive delivery system for mental-health-related services for children:

1. Prevention of developmentally disabling social conditions.
2. Screening and assessment for early signs of dysfunction.
3. Ready availability of facilities for prompt treatment of minor maladies.
4. Fast referral for extensive treatment.

5. Local follow-up on outcomes of referral.
6. Maintenance of usable and transportable records.
7. Coordination of information flow.

Figure 1 contains a schematic outline of the proposed delivery system. This model has several structural features which need comment. It will be noticed that local schools serve as the primary point of intersection for families and relevant professions. Although many school systems have historically fulfilled much the same role as will be proposed here (particularly for the poor), there does not appear to be a plan for using this site to provide services on a coordinated nationwide basis.

The school has been chosen as the first-line screening, assessment, and treatment center for several reasons. First, schools are primarily neighborhood based and therefore easily accessible to the populations that need to be served. Second, they represent a sophisticated network of physical facilities, which could be used (with minor modifications) in lieu of the creation of new facilities. Third, schools are underused on a time basis.

Each participating school would have assigned to it a team of professionals to provide routine screening, assessment, and minor treatment. Such a team might include family nurse practitioners, dental hygienists, social workers, and psychometricians. Problems which required more refined diagnoses or treatment would be referred to a second-level professional group to include, among others, psychologists, physicians, and dentists.

Attached to each school there would be a local advisory council consisting of representatives from various health professions, teachers, and parents. The role of this group would be to monitor the system and to serve as a liaison between the system and the consumers of services.

The coordinating agency should be one that can maintain and disseminate information to relevant individuals on a national scale. Records need to be quickly and accurately transferable to new locations as families move, in order to insure continuity of care and treatment. A likely candidate for this function might be the Social Security Administration.

The main advantage of adopting a service delivery system for children based on this model is that an efficient consolidation of services would be possible. Our current fragmented delivery system, which misses many of the children who most need services, could be replaced by a comprehensive system which would guarantee high-quality care and treatment to all children. Further, much more efficient use of professional time would be possible.

The nationwide adoption of a system based on the model which has been sketched here would allow the continuous monitoring of trends in the mental and physical health of our nation's children. The information generated by periodic screening and assessment and by prompt treatment would serve as a data base for the modification of system characteristics based on the current needs of the population. Through a process such as the one which has been proposed this nation could reaffirm through meaningful action its concern about its young.

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Implications of Developmental Theory for Child Mental Health

JEROME B. DUSEK *Syracuse University*[1]

Following extensive study, the Joint Commission on Mental Health of Children (1970) recently published its findings and recommendations regarding a number of broad issues pertinent to the mental health of children and youth. The foundation of the report is the Commission's suggestion that rather than allow poor mental health to occur and then administer treatment, a system should be established to *prevent* the occurrence of poor mental health in children.

In order to obtain an atmosphere in which prevention of poor mental health is positively viewed and supported, the Commission argued for what is essentially a child's Bill of Rights, including,

> the right to be wanted, the right to be born healthy, the right to live in a healthy environment, the right to satisfaction of basic needs, the right to continuous loving care, the right to acquire the intellectual and emotional skills necessary to achieve individual aspirations and to cope effectively in our society, and the right to receive care and treatment through facilities which are appropriate to their needs and which keep them as closely as possible within their normal social setting [pp. 2–3].

It should be pointed out that although the Commission was clear in stating that these should be the rights of all children, the basic concern was with children from poorer socioeconomic backgrounds in which these kinds of assurances are not normally given. It should also be noted that many of the Commission's recommendations are relevant to development during adolescence and adult life as well as childhood.

In order to insure these rights to all children, the Commission offered a number of recommendations regarding implementation of environmental conditions conducive to these rights. The Commission recommended the establishment of a child advocacy system at the local, state, and national levels; community services and programs of a supportive, preventive, and remedial nature; research programs, both "basic" and "applied," with a positive national view toward research endeavors; and manpower and training programs to both insure jobs for those in need and to provide the manpower for community-based programs. The first two proposals, as detailed in the Commission's report, may be broadly conceived as a package of procedures and programs for guaranteeing delivery of preventive and remedial services to children. The latter two proposals provide the means to present the crucial information and manpower necessary to accomplish the former.

The purpose of the present article is to discuss several aspects of the Commission's report in an attempt to define areas in which professional psychologists may contribute to the realization of the goals established by the Commission. The article deals primarily with issues of a research nature and, as such, is addressed primarily to psychologists working in university or other research settings. Following a brief presentation of the developmental view of child psychology, several implications of this point of view for basic and applied research relevant to child mental health are examined.

The Developmental View

As a basis for its recommendations, the Commission took a broad definition of the concept of mental health founded on a developmental orientation. As a result, the Commission discussed child mental health within a cognitive capacity, both potential and achieved, as well as a physical health framework. As used in the Commission's report, it is difficult to define what is meant by *developmental view* or *developmental level*. Historically, these terms have been used in several different ways,

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Requests for reprints should be sent to Jerome B. Dusek, Department of Psychology, Syracuse University, Syracuse, New York 13210.

each having its implications for the study of child development and child mental health. One interpretation is the rather naive, simplistic notion referred to by Kessen (1962) in his description of the literary-evocative, or metaphorical, use of the term *stage*. As noted by Kessen, this usage is limited in its utility for describing and explaining behavior. A second interpretation of developmental view is a maturational one in which the primary cause for development rests in the genetic material of the organism (Kessen, 1965). This approach is perhaps best exemplified by the extensive work of Gesell (e.g., 1928) and is typically characterized by the use of chronological age as a major factor to explain development. Kessen (1962) has discussed the implications of using age as an explanatory variable. Finally, the currently prevalent view in much of the professional literature refers to a theoretical orientation based on the organismic model of man. The implications of this manner of viewing the developing organism have recently been discussed by several authors (e.g., Langer, 1969; Reese & Overton, 1970). Briefly, the organismic model holds that man is a living, organized system which is inherently and spontaneously active. As opposed to the mechanistic, or learning theory, model of man, which is not a developmental model (Kessen, 1962), qualitative changes in the structure of the organism are accepted as given. Emphasis is on the explication of the nature of the processes producing these qualitative changes. This view will be explicated in detail, since it is the writer's opinion that it has important implications for development.

Application of the organismic model of man to the description of the psychological development of the child entails several basic assumptions with respect to building theories of development. One such assumption is that development will proceed through an invariant sequence of stages, each stage defined by the cognitive structures of the organism and existing in a hierarchical relationship to preceding and future stages. These and other implications of stage theory have been discussed in detail by Flavell (1963) and Kessen (1962). A second basic tenet of cognitive theories of development is the distinction between a child's performance and competence (Flavell & Wohlwill, 1969). Performance is viewed as simply a measure of the child's behavior in a given situation at a given time and not as a measure of the full range of his capabilities with respect to that situation. An excellent example of this notion may be found in studies of language development. The language used by a child may not reflect all of his capabilities with respect to language (Chomsky, 1964). Ginsburg (1972) has extrapolated this issue with respect to the study of the psychological development of deprived children. A third basic assumption is that of internal motivation (Hunt, 1963, 1965). As noted above, the organism is viewed as inherently active. Within cognitive developmental theories, this motivational statement is the basis of development, which involves transformations of cognitive structures due to organization of systems of internal relations through interaction between the structures of the organism and structures of the environment. The direction of development is toward greater equilibrium in organism–environment interactions (Flavell, 1963; Kohlberg, 1969).

IMPLICATIONS FOR BASIC RESEARCH RELATED TO CHILD MENTAL HEALTH

Although a large volume of research based on this theoretical orientation to development has been conducted, there are a number of basic research issues in need of further investigation in order to clarify various aspects of the general developmental model. Although conceived within a basic research framework, these same issues may be crucial to a better understanding of child mental health. For example, research investigating the cognitive development of black children is a prerequisite to implementation of the Commission's recommendation for the development of diversified elementary school curricula responsive to the needs of children. What is currently known of cognitive development in black children is based on comparative research, that is, research in which the performance of black children has been compared to the performance of white children. The emphasis in much of this research has been on what competencies black children *do not* have rather than on competencies possessed by black children. The typical end result of this comparative approach is that black children do "less well" than white children and are, therefore, inferior in development. There are at least two serious implications of this type of research.

One implication is that relatively little is learned about the general nature of cognitive development in black children. Until such knowledge is gained, it will remain difficult, if not impossible, to determine whether previously shown ethnic group differ-

ences in development are due to a "lag" in development or are due to a qualitatively different nature of development. Of course, the same types of arguments may be made with respect to other ethnic groups or in regard to social class or other individual difference comparisons of development. The crucial point is not that comparative research of this nature should not be done, for that is up to the individual investigator. Rather, the value of noncomparative research strategies may go much further toward giving a more complete understanding of development than is possible with comparative strategies. In so doing, such an approach will also provide information relevant to program and curriculum development deemed desirable by the Commission. For example, rather than research aimed at demonstrating gain in ability as a function of intervention programs, what is needed is support for research focused on identifying competencies and programs developed to match and build on these competencies. The end result, then, is that relatively basic research provides information both for general development of the field and for programs devised for improving child mental health.

A second, and increasingly more important, implication of the comparative research strategy is its effect on special interest groups in the community. These groups are becoming more active in demanding prior approval of research to be conducted in schools attended by their children and are typically fearful of research comparing their children to children in the white community. It is noteworthy, in this context, that the Commission apparently felt compelled to preface its recommendations with the statement that "a high priority must be given to the establishment and preservation of a national research climate which optimizes the productivity and opportunities of the individual researcher [Joint Commisison, 1970, p. 17]." Clearly, greater community backing for research endeavors will be needed in the future. This backing is more likely to be obtained if the research is not of a comparative nature, but rather is aimed at providing answers to questions of concern to the community.

The above should not be considered a diatribe against comparative research, but rather as a reminder that there are meaningful questions about development which do not demand the comparative approach. Moreover, the application of findings from developmental research to child mental health may be facilitated by noncomparative research. Finally, community participation in research relevant to the advancement of theorizing about development may be enhanced if the research is not of a comparative nature. Noncomparative research will add to our ability to explain development, and thereby advance developmental theory, but will also provide information invaluable for the application of developmental theory to the everyday problem of guiding child development in the community.

A second, but related, area in need of considerable investigation is the relationship between development in one skill area (e.g., cognitive) to development in other skill areas (e.g., affective or language skills). Kohlberg (1969), for example, has argued that cognitive skills and affective skills (e.g., moral judgments) develop in parallel and independently. To the extent that this is the case, the child may be at different developmental levels on tasks representing different skills, making statements regarding general developmental level as dubiously useful as the traditional IQ concept (Guilford, 1967). What may be necessary is a model of cognitive development similar to Guilford's structure of intellect in which a profile of the child's current developmental levels (stages) is available. Future basic research in this area should clarify these matters and allow the determination of educational programs aimed at building the child's competence to new levels based on his current level of functioning.

Finally, in order to achieve a more complete understanding of development, it will be necessary to conduct large-scale life-span research. Although the Commission argued that further research was needed in the neglected age ranges of 1–3 years (toddlers) and 21–55 years, it must be argued that the most important information for a developmental view would come from studies focused on the development and change in competencies and over longer periods of the life-span at the infancy and childhood levels. In addition to the problems traditionally involved in longitudinal research, for example, sample attrition, cost, etc., at least three other problems must be solved: research design issues, interpretation of task behavior at various age levels, and the determination of tasks appropriate over a wide age range, particularly early to late childhood.

The problem of appropriate longitudinal research designs has recently been discussed by Schaie (1965, 1970) and Nesselroade (1970) and need not be elaborated here. It is sufficient to state that

research designs are available in which data from life-span longitudinal research may be evaluated without confounding age, cohort, or time of measurement effects. It should also be kept in mind that strong arguments for single-subject designs for the assessment of developmental change may also be made (e.g., Hoyer, 1972), thus eliminating some of the problems traditionally associated with longitudinal studies.

A considerably more complex and serious issue is that of the interpretation of behavior at various age or developmental levels. Within cognitive developmental theory, this issue relates to determining the rules of transition of the child's cognitive structures from a relatively lower to a relatively higher level of functioning. Kessen (1962) has discussed this problem and its theoretical implications for cognitive development. The opportunities and need for basic research in this area are obvious and multitudinous.

Perhaps the most difficult of the three problems is the development of tasks that are appropriate for children at different developmental levels of functioning and that allow the assessment of cognitive functioning, that is, the assessment of rules of functioning, at different developmental levels. Although several such tasks are available, for example, probability learning (Weir, 1964) and conservation (Flavell, 1963), this problem remains one of the most difficult yet most important for those interested in conducting developmental studies covering the infancy through adolescent years. This problem is especially important if the assessment of cognitive change from one level to another in development is to be assessed.

As research findings elucidate the concept of developmental level and as better techniques are developed for measuring the child's current level of functioning, appropriate methods must be developed in order to help the child attain the highest possible developmental level in various skill areas. Although it will undoubtedly be necessary to develop new teaching materials based on views consistent with the developmental model outlined above (for example, see Furth, 1970), it must be kept in mind that a vast amount of research involving behavioral technology based on the mechanistic model of man (Bijou & Baer, 1961, 1965; Reese & Overton, 1970) is available for altering children's behavior in various situations. Although these techniques are antithetical to the developmental model, they, nevertheless, may prove useful as tools to alter the child's behavior in such a way that he better attends to tasks aimed at increasing his cognitive competence in some skill area or for teaching certain basic skills prerequisite to more complex levels of functioning.

IMPLICATIONS FOR APPLIED RESEARCH RELATED TO CHILD MENTAL HEALTH

The Commission made a strong plea for applied research related to child mental health. Included among their recommendations were short-range applied projects planned on the basis of productivity at a given time in the nation's history, evaluation of various forms of therapeutic intervention programs, training in research manpower, comparative studies of various forms of intervention procedures, and research projects designed to evaluate and assess the action programs proposed by the Commission. Although these are basically applied research projects, a number of them are directly researchable from a developmental view and have direct relevance to this view.

One example might be to investigate the characteristics of children that relate to success in various day care programs as defined by the objectives of the program, that is, interact child characteristics with program characteristics. It should then be possible to develop techniques for determining the best program, for example, open environment versus more structured programs, for children with certain specified characteristics. To the extent that children's attitudes, characteristics, and cognitive competencies relate to the success of a program, this research, which would most likely have to be longitudinal, is crucial to program development. This approach would allow meaningful developmental research into children's cognitive competencies which is applied in nature, but not comparative, and which has obvious ties to basic developmental research as described above.

As a knowledge base concerning child and adult cognitive competency is built from basic research programs, this information may be incorporated into applied research programs aimed at training adults who work in child care settings. Various training methods for adults working in child care programs of different philosophical orientations may be developed. It should then be possible to interact adult characteristics with program characteristics in order to determine the best way to teach child care information to adults with various cogni-

tive or social competencies. Since the development of cognitive competency in children is related to the cognitive competency of the adults teaching children, it is obvious that this aspect of an applied research program is of critical value to the success of various day care and nursery school programs. Changes in children's behavior, as a function of the training of adults who work in various child care programs, should also be evaluated.

Obviously, a large number of applied projects of this sort are possible, each contributing to a more complete understanding of issues in child mental health as well as the remedial/preventive atmosphere suggested by the Commission. At the same time, however, these projects provide a testing ground for developmental theory as briefly described above and, as such, feed directly into basic research programs. As a result, the dichotomy between basic and applied research begins to fade as each feeds into the other. There remain two criticial issues. First, in order to do this, psychologists, and others, must become interested in the application of developmental theory to everyday child development situations. In other words, the difficult job of translating theory into practice must be tackled, either by the researcher-scholar or by people trained in a way that allows them to do this. Fortunately, some of this kind of work is being done. Second, and perhaps more crucial, is the fact that large-scale research projects of this sort, in which both "basic" and "applied" research are coordinated, require large sums of money for implementation. Current funding levels are not encouraging of the time and effort required to design and plan this type of project. If, as the Commission proposed, we are to make a concerted effort to improve the lives of our children, funds for large-scale programs of this nature will have to be made available.

Summary

Included in the Commission's report are a number of principles, suggestions, and recommendations which are certainly deserving of careful thought and consideration and which will no doubt stir considerable controversy, for example, child advocacy systems, the definition of a mentally healthy child, and a federally paid allowance to children of poverty backgrounds. The undertaking of the Commission was no doubt arduous and the recommendations were bold.

An attempt has been made here to spell out some of the ways in which a developmental view may contribute to a unique understanding of issues related to child mental health. This unique view is the result of theorizing and application based on a conception of the child as a developing organism undergoing qualitative changes with the emphasis on development and change. Although others favoring this view may have written more eloquently and expertly, and those favoring other views may disagree with certain of the arguments presented above, it is clear that this approach has something significant to offer vis-à-vis child mental health.

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Scientific Awards Announcement

The Committee on Scientific Awards is accepting nominations for its award program. The Committee selects up to three persons as recipients of the **Distinguished Scientific Contribution Award** who, in its opinion, have made the most distinguished theoretical or empirical contributions to scientific psychology in recent years.

A new award, the **Distinguished Contribution for Applications in Psychology,** has been authorized by the Board of Directors and will be given for the second time this year. This award will be presented to an individual who, in the Committee's opinion, has engaged in a program of research which is systematic and applied in character.

The awards are subject to the following limitations: (*a*) members of the Committee, former recipients of the awards, the President and the President-elect of the APA shall be ineligible. (*b*) The Committee shall seek diversity in selecting recipients, avoiding as far as possible the selection of more than one person representing a specialized topic, a specific material, a given method, or a particular application.

Names and appropriate information which will guide the Committee on Scientific Awards in conducting an intensive career review and evaluation should be forwarded to Office of Scientific Affairs, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. *Deadline for nominations is March 1, 1974.*

A New Tabulation of Statistical Procedures Used in APA Journals

EUGENE S. EDGINGTON *University of Calgary*[1]

In Edgington (1964), a tabulation was presented of statistical procedures used in APA journals from 1948 to 1962. The tabulation was intended to serve as a guide in the organization of statistics books and courses for psychology students inasmuch as frequently used statistical procedures are not only those that the student needs to understand in order to interpret psychological publications, but are also those that the student is likely to find useful for data analysis. This article brings the tabulation up to date for the same journals or their successors.

The journals examined for the current tabulation were the APA journals primarily concerned with original empirical research: *Journal of Abnormal Psychology, Journal of Applied Psychology, Journal of Comparative and Physiological Psychology, Journal of Consulting and Clinical Psychology, Journal of Educational Psychology, Journal of Experimental Psychology,* and *Journal of Personality and Social Psychology.* All articles in these journals for the even-numbered years from 1964 to 1972 (except the 1966 issues of the *Journal of Educational Psychology,* which were not available) were examined to determine what kinds of inferential statistics, if any, were used. The results are presented in Table 1 along with the previously published results for 1948–1962. Observe that the percentages in Table 1 are based on all articles involving statistical inference, *not* on all articles in the journals. (Ninety-one percent of the articles involved statistical inference.)

Table 1 shows that by 1972, 71% of the articles using statistical inference used analysis of variance.

[1] Requests for reprints should be sent to Eugene S. Edgington, Department of Psychology, University of Calgary, Calgary 44, Alberta, Canada.

TABLE 1

Number of Articles Using Types of Statistical Procedures Expressed as Percentages of Number of Articles Using Inferential Statistics

Year	No. statistical inference articles	Percentages					
		Analysis of variance	*t* test[a]	Correlation[b]	Chi-square[c]	Factor analysis	New nonparametric[d]
1948	204	11	51	42	10	1	3
1950	310	16	47	38	19	3	3
1952	458	24	44	32	16	4	7
1954	529	34	34	33	14	3	13
1956	587	38	29	32	16	3	16
1958	689	41	26	29	16	4	17
1960	593	42	23	28	12	5	20
1962	775	55	19	24	10	5	20
1964	674	58	18	20	17	4	19
1966	925	62	18	20	16	2	19
1968	1,128	69	13	21	16	2	17
1970	1,170	70	13	25	12	3	14
1972	1,195	71	12	25	15	4	12

[a] Tests involving the use of either *t* tables or normal-curve tables. Excludes tests in connection with analysis of variance.
[b] Parametric correlation techniques and Spearman's rank-difference method. Excludes correlation coefficients computed for factor analysis.
[c] Traditional chi-square procedures.
[d] All nonparametric techniques not in correlation and chi-square categories.

For the 1972 journals, a separate count was made of simple one-way analysis of variance and more complex analysis of variance techniques. It was found that although simple one-way analysis of variance was frequently used, 88% of the analysis of variance articles employed repeated-measures designs or factorial designs instead of, or in addition to, simple one-way analysis of variance. (Factorial designs were employed more frequently than one-way repeated-measures designs.) Repeated-measures and factorial analysis of variance techniques are not commonly included in introductory statistics books, and so it is likely that they also are not generally included in the introductory course. The extent to which these procedures are used, however, suggests that they should be taught. Time for teaching these procedures could be acquired by not teaching independent and correlated *t*-test techniques, which are redundant when both simple and repeated-measures analysis of variance are taught. Inasmuch as there is frequent reference to *t* tests in the journals, however, the function served by them should be explained in terms of the corresponding analysis of variance tests.

Correlation and traditional chi-square procedures are used frequently and certainly serve distinctive functions, which should assure them of a place in introductory statistics books and courses. These procedures are, of course, usually taught, and so the recommendation made here is to retain them. Similarly, factor analysis is *not* a standard technique for an introductory statistics course, and it should *not* be a part of the course because it is used so infrequently. The "new nonparametric techniques," which exclude traditional chi-square procedures and Spearman's rank correlation, are used in all of the journals to some extent and in some journals rather frequently. The overall extent of use merits a general discussion of nonparametric statistics in the introductory course, but not a detailed discussion of a number of techniques. Of the 798 articles for the years 1964–1972 that used new nonparametric techniques, half made use of the Mann-Whitney *U* test, so there may be reason to discuss this test, but no other new nonparametric test approached such widespread use.

Tables like Table 1 were constructed for each of the seven journals to permit comparisons across journals. Variation among the journals in the use of statistical procedures was found, but there was also considerable agreement. For example, every one of the journals showed an upward trend in the use of analysis of variance, and in every journal the use of complex analysis of variance techniques in 1972 far outnumbered the use of simple one-way analysis of variance.[2]

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[2] The author will send separate tables for the seven journals to readers requesting them (see address in Footnote 1).

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Edgington, E. S. A tabulation of inferential statistics used in psychology journals. *American Psychologist*, 1964, 19, 202–203.

Contributions to Education in Psychology Awards

The American Psychological Foundation invites nominations for the 1974 Contributions to Education in Psychology award. The annual award is in the amount of $1,000 to be given to the recipient for his achievements. It is hoped that the awardee's institution will contribute a matching sum, thus providing the recipient with a modest "grant" to be used by him as he wishes to "improve the teaching of psychology." Thus, the objective of the award program is to enhance the local instructional program, not just to recognize "master teachers." Because the awards are to be presented at the APA Annual Convention in September 1974, *nominations must be completed and sent to the Committee by March 31, 1974.* A nomination form with a statement of the guidelines and suggestions for documentation can be obtained by writing to Irwin J. Knopf, Chairman, APF Teaching Awards Committee, Department of Psychology, Emory University, Atlanta, Georgia 30322.

Distinguished Scientific Contribution Awards for 1973

The Awards for Distinguished Scientific Contribution are presented by the Association at the annual convention. The awardees for this year, along with those for the preceding years since the establishment of the custom, are:

1956
Wolfgang Köhler
Carl R. Rogers
Kenneth W. Spence

1957
Carl I. Hovland
Curt P. Richter
Edward C. Tolman

1958
Frank A. Beach
Paul E. Meehl
B. F. Skinner

1959
Leon Festinger
Donald B. Lindsley
Neal E. Miller

1960
Harry F. Harlow
Charles E. Osgood
S. Smith Stevens

1961
James J. Gibson
Donald O. Hebb
Henry A. Murray

1962
Jerome S. Bruner
William K. Estes
Harry Helson

1963
Roger G. Barker
George A. Miller
Carl Pfaffmann

1964
Gordon W. Allport
Wendell R. Garner
J. P. Guilford

1965
Floyd Allport
Fritz Heider
Paul Thomas Young

1966
Nancy Bayley
Clarence H. Graham
Richard L. Solomon

1967
Solomon E. Asch
Ernest R. Hilgard
James Olds

1968
James E. Birren
Eleanor J. Gibson
Muzafer Sherif

1969
Jean Piaget
Stanley Schachter
Herbert A. Simon

1970
Donald T. Campbell
David Krech
R. Duncan Luce

1971
Roger William Brown
Harold H. Kelley
Roger Wollcott Sperry

1972
Edwin E. Ghiselli
Dorothea Jameson
and Leo Hurvich
Patrick Suppes

1973
Lee J. Cronbach
Conrad Kraft
Brenda Milner
Benton J. Underwood

Cronbach, Kraft, Milner, and Underwood were each presented with an engrossed citation of his/her formal contributions to the development of scientific psychology and a check for one thousand dollars. These psychologists have agreed, in accordance with established custom, to present addresses on some phase of their scientific work at the 1974 Convention. The presentation of awards was made by R. Duncan Luce, Chair of the Committee on Scientific Awards. Other members of the Committee are Herschel Leibowitz, Brendan Maher, James Olds, Harold H. Kelley, and James J. Jenkins.

Lee J. Cronbach

CITATION

"For his singularly outstanding contributions to psychometric theory and application. His work on reliability theory, interpretations of test validity, and basic concepts in decision making based on measures of individual differences has influenced the foundations of our science. His analysis of the relationships among various disciplines of psychology has pointed to new problems for investigation. His exceptionally clear and well-organized textbooks on psychological testing and educational psychology are read throughout the world. His analysis of educational evaluation has strengthened the contribution of psychological research to education. As an individual, he is widely respected for his sustained devotion to the problems he addresses and for his ability to focus with insight on the relevant issues involved."

BIOGRAPHY

Lee J. Cronbach was born April 22, 1916, in Fresno, California. After graduating from Fresno High

Lee J. Cronbach

School he completed the AB degree at Fresno State College in 1934. He obtained a secondary teaching credential at the University of California, Berkeley, in 1935, and taught mathematics and chemistry from 1936 to 1938 at Fresno High School. He also received the master's degree from the University in 1937. His doctoral program under G. T. Buswell and R. W. Tyler in the Department of Education of the University of Chicago (1938–1940) provided training for educational research. He served for one year as research assistant in the Evaluation Staff of the Eight-Year Study, and received his doctorate in 1940.

Most of his education in general psychology came when, as half of the two-man psychology faculty of the State College of Washington, he found himself teaching many courses he had never taken. In this position (1940–1946) he also directed a reading clinic and assisted in student personnel programs. In 1944 and 1945 he was on leave to work under Adelbert Ford, in a personnel research and development office established by the National Defense Research Committee at the U.S. Navy Radio and Sound Laboratory. There he validated personnel tests, and designed and evaluated an instructional program for training sonar operators to discriminate underwater sounds.

Cronbach returned to the University of Chicago as assistant professor of education in 1946, and in 1948 moved to the University of Illinois as associate professor of education, and later professor of education and psychology. The Illinois appointment was in the Bureau of Educational Research, and provided freedom and encouragement to work on fundamental questions relevant to education. After 16 years at the University of Illinois he moved to Stanford University, where he is Vida Jacks Professor of Education with responsibilities in training for educational research.

Cronbach's research, being primarily methodological, has often taken its direction from the difficulties encountered in the research of colleagues and in projects to which he has been a consultant. At the University of Chicago, he was exposed to the personological studies flourishing around Robert Havighurst, Carl Rogers, and W. E. Henry. Their dynamic assessments of the person appeared to be ill-served by traditional methods of validating test interpretations. Cronbach produced papers suggesting new approaches to test validation and a textbook that emphasized general critical principles of test development and use. The work was carried further when Cronbach was asked to chair the Committee on Test Standards of the American Psychological Association. To develop a code for maintaining the quality of tests of ability and personality, it was necessary to reconsider the entire logic of validation. Paul Meehl, out of his study of the philosophy of science, developed a committee paper which contained the basic idea of construct validation. This idea was carried further by the committee, and ultimately was elaborated into the well-known Cronbach-Meehl (1955) exposition. (The toss of a coin determined the order of authorship.)

Consultation with the Kelly–Fiske project on assessment of clinical psychologists led Cronbach into a program of studies (1951–1958) on empathy and the perception of others, partly in collaboration with Fred E. Fiedler and N. L. Gage. In describing empathy, similarity between persons, and projection, and also in the application of tests in the clinic, the analysis of score profiles loomed large. To clarify the logical issues in profile analysis, Cronbach enlisted Goldine Gleser, then associated with clinical research at Washington University, as

co-worker, thus launching a 20-year collaboration.

While consulting with a Navy laboratory regarding sonar training, Cronbach was introduced to Shannon's information theory; he and Gleser set out to trace its possible use as a basic model for psychometrics. Information theory proved unsuitable, but led them ultimately to treat test interpretations as rules for decision making, applying the more elaborate model of statistical decision theory. Cronbach's next collaboration with Gleser turned into a nearly unending project. At the outset, it was their intent to put the ideas of measurement theory, new and traditional, into a straightforward but unified exposition. Almost at the outset, however, contradictions and puzzles turned up which were resolved only by the production, some 15 years later, of a monograph introducing a new "theory of generalizability." This theory embraces much of classical theory but offers more flexible and constructive analyses, and brings to light problems previously unrecognized. The 1972 work ended where the 1952 collaboration began, with suggestions for modifying the way in which profiles of test scores are used.

New problems also emerged from the earlier work on decision theory. As Cronbach pointed out in his 1957 APA Presidential Address, the decision analysis showed that neither the traditional methods of the experimenter nor those of differential psychology are adequate to investigate responses to treatments or to validate tests for purposes other than selection. The interactions to which he directed attention have increasingly become a concern in research on mental processes, on personality, and in several applied fields. Since 1965 Cronbach has been engaged with R. E. Snow and many students in substantive and methodological work on interactions, with particular reference to adapting instruction to the individual. A treatise on this "new discipline" of psychology is near completion. Cronbach's other recent writings reflect a concern for the social policy underlying the use of tests, which has led him into studies of the history of testing and its social influences. Cronbach is also engaged with R. R. Sears in a study of the career development and social contributions of the Terman "gifted" sample.

Cronbach's approach to curriculum and instruction, expressed particularly in his textbook on educational psychology, was primarily the outgrowth of his years at the University of Chicago. Teaching the undergraduate course there in 1946–1948, he followed Tyler in urging that classroom practice be aligned with psychological findings, particularly those on the generalization and transfer of learning. Cronbach followed Havighurst and Prescott in interpreting the pupil's response to schooling as a reflection of and a vehicle for his continuing development as a person. Except for wartime work on training materials and some unpublished local studies of instruction at the University of Illinois and Stanford University, Cronbach has done little direct educational evaluation; his ideas have developed mostly through his role as a consultant. He followed closely the applied work of the Unit on Evaluation at Illinois under J. T. Hastings, and later was consultant on measurement for national projects seeking to reform science and mathematics curricula and for the National Assessment of Educational Progress.

In 1955–1956 Cronbach served as scientific liaison officer in the Office of Naval Research, London, and in 1967–1968 he was Fulbright lecturer in the Faculty of Education of the University of Tokyo. From 1961 to 1966 he chaired the Committee on Learning and the Educational Process of the Social Science Research Council, which sought to make psychological research on learning more fruitful for education. For this committee he directed postdoctoral training institutes at Stanford in 1965 and 1966 which have been the model for subsequent international institutes in Sweden, France, Germany, and Southeast Asia.

Cronbach has served as President of APA and its Division on Evaluation and Measurement, the Psychometric Society, and the American Educational Research Association. In 1960–1961 he was a member of the Institute for Advanced Study and in 1963–1964 a Fellow of the Center for Advanced Study in the Behavioral Sciences; in 1971–1972 he was a Guggenheim Fellow. He is a member of the National Academy of Education, the American Philosophical Society, and the American Academy of Arts and Sciences. He received an honorary Doctor of Humanities degree from Yeshiva University in 1967, and the award for Distinguished Service to Educational Measurement from the Educational Testing Service in 1971.

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Conrad Kraft

CITATION

"For diligence, skill, and effectiveness in applying psychological principles and knowledge to improve the efficiency and safety of man's interaction with his environment and in complex man–machine systems. His experiments reflect both a sensitivity to problems stemming from the technological nature of our society and an appreciation for the potential value of the data and methods of experimental psychology. His ability simultaneously to encompass both points of view has produced a series of unique and important contributions. In particular, his development of radar room lighting systems and his identification of a visual illusion during night visual landing approaches as a major contributing factor to a series of fatal jet aircraft accidents represent outstanding examples of his and psychology's contribution to our society."

BIOGRAPHY

Conrad L. Kraft was born in Birmingham, Michigan, on April 25, 1916, but he grew up in the western town of Rawlins, Wyoming. His undergraduate work was done at the University of Wyoming, located in the more famous city of Laramie. He began his undergraduate work in engineering, but the personality and teaching skill of Robert Hall Bruce drew him into the field of general-experimental psychology. By the time he graduated in 1939, his interest in behavioral mea-

surement was well-established, but jobs were scarce. He was first employed to make a photographic survey of the Big Horn Cattle and Sheep Company, which added to his relatively new skills in photography. This skill has since been a basic tool in much of his research.

A few months after graduation, his first opportunity to use his interest in psychophysics came with the opportunity to work with E. H. Scofield in the application of these techniques in measuring odor and taste. Scofield had just organized the Quality Research and Control Laboratory of Joseph E. Seagram and Sons at Louisville, Kentucky. The initial work of this laboratory was basic research in methodology designed to obtain the necessary reliability and validity to improve and control the quality of alcoholic beverages. The location of this laboratory in a distillery gave Kraft's father, an Episcopalian minister, the opportunity to quip, "My son is chief taster for Seagram's." Working in the laboratory were other young men who found the methodological problems most stimulating. Dean B. Foster took his interest in olfaction to Cornell University and developed the room-sized olfactometer under Karl Dallenbach. David R. Peryam extended the tasting methods to food with the Army Quartermaster Corps at Chicago in later years. Kraft, while with Seagram's, was asked to organize and develop branch laboratories in Indiana, Maryland, and Ontario in Canada. This assignment gave him experience in introducing psychological measurement into industrial organizations and in managing these laboratories.

In January of 1944 Kraft joined the Navy and its program of personnel classification and selection. This led to a short course in visual skill measurement with the Ortho-Rater from J. Tiffen and S. E. Wirt at Purdue University. Most of Kraft's wartime duty was on "the islands," Treasure and Goat, in San Francisco Bay. Building new crews for new construction destroyers and personnel transports was the major work of the classification and selection people on Treasure Island. Kraft, however, enjoyed most the chance to do some research with the Ortho-Rater, obtaining improved instructional aids, test–retest reliability, validation studies with the performance information on Combat Information Center personnel and shipboard lookouts recently returned from the Okinawa "picket line."

As 1946 brought the end of the war, Kraft returned to the University of Wyoming for his

Conrad Kraft

master's and more skills in psychology and statistics. This work under R. H. Bruce expanded his interest in vision, and in the fall of 1947 a Lumley Memorial Fellowship took him to Ohio State University to continue in this field with Sam Renshaw.

The work under Renshaw at Ohio State was to cover a number of years in a very active laboratory. The 1947–1952 period found a large number of graduate students returning from military service with a heightened interest in experimental psychology. Adams, Alluisi, Briggs, Chenoweth, G. Rowland, DeGroot, Hartman, Kelley, Pheiffer, and F. A. Young made for a lively series of debates on any subject, theory, school, or method. Renshaw was active in bringing many of the outstanding people in the field of vision to Ohio State once a year with the vision conferences. These conferences and the workshops in behavioral optics for optometrists gave the graduate students a broad exposure to clinical and theoretical aspects of vision.

Paul Fitts had come to Ohio State in 1949, and his Laboratory of Aviation Psychology was soon to begin nine years of research in human engineering aspects of radar air traffic control. Kraft's combination of interest in engineering, aviation, vision, and methodology found a working environment much to his liking in this laboratory. Fitts' direction was stimulating, and members of the laboratory such as Schipper, Noble, McGuire, Leonard, and the Air Force monitor, Queal brought fresh ideas and a variety of backgrounds to attack the problems of a large system.

Kraft served with Jim McGuire as the technical liaison between the Laboratory of Aviation Psychology at Ohio State and Wright Patterson Air Force Base, Dayton, Ohio. This brought Kraft into contact with operational personnel and the Engineering Psychology Division of the Aero Medical Laboratory. The operational organizations provided many problems, and Kraft's interest in applying behavioral knowledge and techniques was aroused. Christensen, Warrick, and Grether at the Aerospace Medical Research Laboratory provided very sound guidance as to the workability and applicability of Kraft's endeavors to improve lighting, communications, data collection, methods of encoding information, and simulation. The juxtaposition of Fry and Enoch's laboratory to that of Paul Fitts exposed Kraft to physiological optics and the problems of the photointerpreter.

These individuals and their laboratories had marked influence on Kraft, and it is not surprising that after completing his PhD under the joint sponsorship of Renshaw and Fitts that a move to Boeing in 1959 found Kraft continuing his work in engineering psychology and vision. Boeing has provided the opportunity for Kraft to do research in these two areas, and to apply his work and that of others to the problems of man in space, the airborne observer, the photointerpreter, the industrial worker, the automobile driver, everyday safety, and the object of his special interest, the pilot. Most recently his work in vision has been in the fields of accommodation, color perception, and stereoscopic skill.

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Brenda Milner

CITATION

"For outstanding psychological study of the human brain. She and her students have elucidated the role of the prefrontal lobes, previously resistant to analysis; have provided definitive data on the relation of speech localization to handedness; and have demonstrated the importance of the right temporal lobe in pattern perception (visual, auditory, tactual). Of great significance is Milner's study of the function of the hippocampus in the consolidation of memory and the devastating effect of the bilateral loss of the hippocampus on the medial aspect of the temporal lobe. The result is an inability to form new memories and an astonishing helplessness even though earlier established memories are not impaired: a dramatic demonstration of the importance of that ability we take so lightly for granted, the ability to remember what happened 10 minutes (or more) ago."

Brenda Milner

BIOGRAPHY

Brenda Milner (formerly Langford) was born on July 15, 1918, in Manchester, England, where she attended Withington Girls' School. From there she went to Newnham College, Cambridge, to read mathematics, but after completing Part I of the Mathematical Tripos in 1937 she changed fields, graduating in 1939 with a BA degree in experimental psychology (then classed with the moral sciences). Her supervisor at Cambridge was Oliver Zangwill and to him she owes her first interest in human brain function.

After graduating, Milner was awarded a Sarah Smithson Research Studentship by Newnham College, so that she was able to stay on in Cambridge for two more years. The award coincided, however, with the outbreak of World War II, when the work of the Cambridge Psychological Laboratory (under Bartlett's leadership) was diverted almost overnight to applied research. For Milner this meant devising perceptual tasks that could be used in the selection of aircrew. Later in the war, from 1941 to 1944, she worked in Malvern as an Experimental Officer for the Ministry of Supply, investigating different methods of display and control to be used by radar operators. In 1944 she married Peter Milner and left England for Canada, supposedly for one year.

Since childhood, Milner had had a great love for the French language and had even hoped that she might one day live and work in France. When she found herself in Montreal, her knowledge of French was an immediate asset, enabling her to obtain a position at the Université de Montréal, in the newly formed Institut de Psychologie, where she set up a laboratory and taught comparative and experimental psychology from 1944 to 1952. A turning point came with the arrival of D. O. Hebb at McGill in 1947. His book *The Organization of Behavior* was still in manuscript form, and each chapter was being vigorously debated in the weekly seminar, which Milner attended. She decided to register in the PhD program, while still maintaining her teaching job at the Université de Montréal. In 1950, Hebb gave her the opportunity to study Penfield's patients at the Montreal Neurological Institute and she has remained there ever since. She obtained the PhD

degree from McGill University in 1952 and the ScD from the University of Cambridge in 1972. At present she holds the position of professor of psychology in the Department of Neurology and Neurosurgery at McGill, and since 1964 her work has been supported by the Medical Research Council of Canada through the award of a research associateship.

Milner's early work on the temporal lobes was influenced by the results of ablation work with lower primates, and particularly by Mishkin and Pribram's discovery of the role of the inferotemporal neocortex in visual discrimination learning. The visual deficits seen in patients after right temporal lobectomy indicated a convergence between the findings for monkey and man. A new departure came with the observation of global memory loss in two patients with bilateral damage to the hippocampal region, a finding which led Milner to focus much of her subsequent research on the analysis of memory disorders. In recent years she has extended her research to the frontal and parietal lobes, and has also been studying the effects of early unilateral brain lesions on the pattern of cerebral organization at maturity. Much of this work has been carried out with Laughlin Taylor, Milner's close associate for over 15 years.

Milner's work owes much to the encouragement and participation of her colleagues in neurosurgery, especially Theodore Rasmussen, as well as to the contributions of such graduate students as Doreen Kimura, Lilli Prisko, Suzanne Corkin, and Philip Corsi. She also values her collaboration with H.-L. Teuber, R. W. Sperry, and A. M. Liberman.

Milner is a fellow of APA and currently holds the office of President of the Division of Physiological and Comparative Psychology. She is also a member of several other professional societies. In 1971–1972 she returned to Cambridge University for a sabbatical year, during which time she held a Clothworkers' Fellowship at Girton College. In 1971 she received the Kathleen Stott Prize for Medical Research from Newnham College.

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Benton J. Underwood

CITATION

"For his massive contributions to the experimental and theoretical analysis of verbal learning and memory. A master of experimental design, he has been a recognized leader in the development of a modern and sophisticated methodology in his field of research. For more than a quarter of a century his wide-ranging investigations have focused on

the fundamental processes of acquisition and retention. The substantive areas in which his work has yielded new theoretical insights and basic empirical findings are too numerous to list. Among the highlights are his systematic explorations of the effects of distribution of practice, the principles of transfer, the mechanisms of interference in retention, and the role of discriminative processes in recognition. While firmly rooted in the traditions of his discipline, he has throughout his career been an innovator and a pacesetter in a rapidly growing and changing domain of research."

Benton J. Underwood

BIOGRAPHY

Benton J. Underwood was born February 28, 1915, in Center Point, Iowa. He received his primary and secondary schooling in Albion, Iowa, and his undergraduate degree at Cornell College, Mount Vernon, Iowa, in 1936. His goal in attending college was to qualify as a high school athletic coach. He served in this capacity for three years (1936–1939), during which period he learned that dealing with the problems of the classroom was far more rewarding than dealing with those that arose in the locker room.

He began his graduate work in psychology at the University of Oregon in the summer of 1939, but an offer of financial aid from the University of Missouri for that fall led him to Columbia. The stimulation produced by the small department at Missouri, under the chairmanship of Arthur W. Melton, led to an intellectual awakening and to a permanent imprinting of an interest in the study of human learning and memory. After the master's, he was appointed a research assistant to John A. McGeoch at the University of Iowa. The PhD was granted in December 1942. The ferment at Iowa during these years was at a peak. Spence and Bergmann were hammering out positions in a philosophy of science appropriate to psychology; the theoretical controversy between Hull and Tolman was the impetus for the activity in the animal laboratory; and the newly postulated idea of *unlearning* of associations (Melton and Irwin) guided much of the research in verbal learning. Over it all, McGeoch as chairman presided with a balancing influence which supported sound research on any topic, theoretically pointed or not.

Underwood was commissioned in the Naval Reserve in January 1943 and was assigned to the Naval Aviation Psychology Branch, headed by John J. Jenkins. Immediately after his discharge in January 1946, he joined the faculty at Northwestern University, this appointment being recommended by William A. Hunt who, along with Donald B. Lindsley, also became a member of the faculty at Northwestern at that time. He found the faculty of the department and the general atmosphere of the University to his permanent liking. Both his long-term colleagues (Hunt, Duncan, Campbell) and the many who came and went provided the department with the critical research orientation needed for a first-rate department. Through the Office of Naval Research, continuity in experimentation has been possible, and there always seemed to have been two or three bright and energetic graduate students who were willing to cast their lots with a memory drum. During the 23 years of support by ONR, much of the day-to-day responsibility for Underwood's research rested on his research assistants, who were Ross L. Morgan, E. James Archer, Jack Richardson, R. W. Schulz, Geoffrey Keppel, Bruch Ekstrand, Joel Freund, and Joel Zimmerman.

Underwood has been President of Divisions 1 and 3 of the American Psychological Association, and of the Midwestern Psychological Association. In 1964 he was awarded the Warren Medal by the Society of Experimental Psychologists, and in 1970 he was elected to the National Academy of Sciences. He has served on a number of government and quasi-government boards and panels, as a consulting editor for several journals, and he is cur-

rently an Editor of the *American Journal of Psychology*. Cornell College awarded him an honorary ScD in 1966. Such events continue to amaze the former high school athletic coach.

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Distinguished Professional Contribution Award for 1973

The second Award for Distinguished Professional Contribution was presented to David Wechsler at the Annual Convention of the American Psychological Association in Montreal on August 28, 1973. Brent N. Baxter, Chair of the Committee on Professional Awards, made the presentation. Other members of the Committee are Theodore Blau, Edward Loveland, Alfred Marrow, and Edward S. Bordin.

David Wechsler

CITATION

"His contributions to the theory of cognitive growth and his concurrent development in 1939 of the first clinical test of adult intelligence and the forms for children and preschoolers which followed it have changed the course of professional psychology, broadening its areas of application and its impact upon other professions and the public at large. These contributions used by psychologists working in hospitals, clinics, schools, colleges, and industrial settings have played a critical role in the lives and careers of millions of individuals throughout the world. As one of the first presidents of the Division of Clinical Psychology and also one of the charter members of the American Board of Professional Psychology, he helped give impetus to the postwar accelerated growth of professional psychology. Kindly, thoughtful, patient, and wise, David Wechsler as scholar and clinician has ennobled professional psychology."

David Wechsler

BIOGRAPHY

David Wechsler was born in 1896. He received his AB and MA at about the time that World War I was claiming center stage in the United States. The young scholar, as Private Wechsler, found himself involved in the large-scale intelligence testing program conducted by the Army. Thus began a fruitful interest in the measurement of intelligence, which has led to major contributions to theory and practice. After the war ended, Wechsler studied in London with Spearman and Pearson and in Paris with Henri Pieron and Louis Lapique. It was in Paris that he became interested in the psychogalvanic reflex. Later, he studied this reflex in depth in his doctoral dissertation "The Measurement of Emotional Reactions." He was awarded the PhD by Columbia University in 1925.

By 1932, Wechsler had become chief psychologist at Bellevue Psychiatric Hospital. He had already published papers in a variety of areas: studies in chronaxia, tests for taxicab drivers, the influence of education on intelligence, galvanic responses in preschool children, and others. An important article entitled "The Range of Human Capacities" was subsequently expanded and published as a book in 1935. This volume revealed that "the differences which separate the mass of mankind from one an-

other, with respect to any one or all their abilities, are small . . . ." It also noted the decline of ability with age, a matter of special significance in Wechsler's later work with intelligence scales.

Starting in 1934 and continuing to the present day, he has devoted some of his enormous energy and talent to the development of the intelligence scales which are internationally known: the Wechsler-Bellevue I (1939), the Wechsler-Bellevue II or Army Wechsler (1942), the Wechsler Intelligence Scale for Children (1949), the Wechsler Adult Intelligence Scale (1955), the Wechsler Preschool and Primary Scale of Intelligence (1967), and the Revision of the Wechsler Intelligence Scale for Children (1974).

When Wechsler undertook to develop the first of his adult scales, the Wechsler-Bellevue I, he was responding to a need for an instrument that would measure adult intelligence in adult terms. He insisted that "adult intelligence cannot be evaluated in the same terms as those generally employed in defining juvenile intelligence," and that the concept of mental age for adults was misleading. He wrote:

> To calculate an I.Q. for a man of 60 by dividing his M.A. score by 15, is as incorrect as to obtain an I.Q. for a boy of 12 by dividing his M.A. score by 15 . . . . for a valid evaluation of an individual's brightness, one must compare his mental ability to that of the average individual of his own age.

He produced his IQ tables by taking the age of the subjects into consideration, and thus the deviation IQ was offered for the first time in a major individual test of intelligence for adults.

During this period in the 1930s, Wechsler was crystalizing his thoughts on the measurement of intelligence. Especially noteworthy was the apparently simple but very insightful definition of intelligence that emerged in 1939: "Intelligence is the aggregate or global capacity of the individual to act purposefully, to think rationally and to deal effectively with his environment." This has been the guiding principle in Wechsler's conception of intelligence. The definition conceives of intelligence as a global entity, multidetermined and multifaceted, rather than as an independent trait. Wechsler wished to avoid designating any single ability as crucial. In particular, he wished to avoid equating general intelligence with intellectual ability. He has consistently adhered to this view. In the Manual (1974) for his Revision of the Wechsler Intelligence Scale for Children, he wrote:

> To the extent that tests are particular modes of communication, they may be regarded as different *languages*. These languages may be easier or harder for different subjects but it cannot be assumed that one language is necessarily more valid than another . . . . an intelligence scale, to be effective as well as fair, must utilize as many different languages (tests) as possible.

He pointed out the role of nonintellective factors at all levels of intelligence and inserted this note of caution: "Nonintellective factors are necessary ingredients of intelligent behavior; they are not, however, substitutes . . . for other basic abilities."

It should be noted that, over the years, Wechsler also found time to serve as Clinical Professor at the New York University College of Medicine, a Trustee of the American Board of Examiners of Professional Psychology, President of Division 12 (APA), and President of the American Psychopathological Association. He has had more than 60 articles published in scientific journals and is the author of *The Measurement of Adult Intelligence*. This book was first published in 1939. It went through three editions and, in its fourth edition in 1958, it became *The Measurement and Appraisal of Adult Intelligence*. A fifth and enlarged edition of this book, by Joseph D. Matarazzo, was published in 1972.

Although he retired from his position as chief psychologist at Bellevue Hospital in 1967, it is obvious to all who know him that he has simply "changed jobs." With writing, lecturing, traveling, developing new test ideas, and researching new areas, he has been busier than before. Among topics that have been of interest to him are artificial intelligence, the problem of memory storage, and collective intelligence. His views on the last are in his paper, "The Concept of Collective Intelligence," published in the *American Psychologist*, October 1971.

David Wechsler has made major contributions to psychology for more than a half-century. Hopefully, he will continue to do so for many years to come.

SCIENTIFIC PUBLICATIONS

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Wechsler Intelligence Scale for Children. (Revised) New York: Psychological Corporation.

Selected papers of David Wechsler. New York: Academic Press.

Gold Medal Award

The Trustees of the American Psychological Foundation invite members of the American Psychological Association to nominate candidates for the Gold Medal Award. The Award is given to an American psychologist in recognition of a distinguished and long-continued record of scientific and scholarly accomplishments. The Award is limited to psychologists 65 years of age or older and is also limited to those residing in North America. Nominations should be accompanied by a statement highlighting the accomplishments of the nominee. They should be addressed to: Secretary, American Psychological Foundation, 1200 Seventeenth Street, N.W., Washington, D.C. 20036, to be received no later than March 15, 1974. Decisions will be made by the Trustees of the Foundation.

American Psychological Foundation Awards for 1973

Gold Medal, Distinguished Contributions to Education in Psychology, and the National Media

On August 28, 1973, at the annual meeting of the APA in Montreal, the American Psychological Foundation announced the recipients of the Gold Medal Award, the Distinguished Contributions to Education in Psychology Awards, and the National Media Awards. The presentations were made by Henry W. Riecken, President of the APF. Harry F. Harlow received the Gold Medal Award; James B. Maas and Frank J. McGuigan were given the Teaching Awards; and Gerald Jonas, ABC News and Jules Power Productions, the Great Atlantic Radio Conspiracy, Christine Darg, and Elliott Aronson were the recipients of the National Media Awards. Other members of the APF are Nicholas Hobbs, Gardner Lindzey (Vice-President), Boris E. Cherney (Treasurer), George Miller, George Albee, William Bevan, Kenneth B. Little (Secretary), Kenneth B. Clark, and Anne Anastasi.

The American Psychological Foundation was established in 1953 to receive gifts from psychologists and others who wish to contribute to the advancement of psychology as a science and a profession in the service of knowledge and the welfare of man.

Harry F. Harlow

Gold Medal Award

Each year at the annual convention of the American Psychological Association, the Foundation presents its Gold Medal Award to a senior American psychologist in recognition of a distinguished and long-continued record of scientific and scholarly accomplishment. With the Medal goes a cash award of $1,000. Past recipients of the Gold Medal Award are Robert Sessions Woodworth, Edwin R. Guthrie, Edwin G. Boring, John F. Dashiell, Walter R. Miles, Gordon W. Allport, Heinrich Klüver, Karl M. Dallenbach, Floyd H. Allport, Henry A. Murray, Sidney L. Pressey, B. F. Skinner, and Gardner Murphy.

HARRY F. HARLOW

This year's recipient of the Gold Medal Award has built a distinguished scientific career upon the study of basic psychological phenomena that are simultaneously of great scientific interest and great social importance. It is fair to say that, while he has been concerned with learning and behavior changes accompanying maturation from infancy to adulthood, and while he has shown some interest in therapeutic communities, a larger share of his attention has been given to affective behavior—especially to the affective relations between parents and offspring and to the pathologies of these relations. Although he would surely agree that "love is not enough," it

is clear that he believes love is in first place, and that whatever is second is quite a few furlongs back. He has lectured and written extensively on the insufficiency of love and is probably best known to the general public for his views on this topic. Indeed, numbers of American mothers may have been influenced in how they behaved toward their children by hearing or reading his views.

On the other hand, it is not true that he contributes a regular column to the *Ladies Home Journal*, although that is one of the few journals in which his work has not appeared. He generally reports his work in prose but has occasionally resorted to verse, perhaps because he thinks it helps him to make better contact with his subject. Finally, he does not work with human subjects, but only with their close relative, the rhesus monkey.

The facts of Harry F. Harlow's academic history are simple—almost defiantly simple for the purpose of constructing an exciting story. There are not a lot of comings and goings, changes of scene, or of fortune, but rather a record (rare among psychologists) of attachment to a single university that parallels his steadfast interest in the monkey. Harry Harlow was born in Fairfield, Iowa, long enough ago to make him eligible for the Gold Medal Award. After a year at Reed College he completed his university education at Stanford, where he received the BA and, three years later, the PhD in 1930. He immediately joined the psychology faculty at the University of Wisconsin and has remained a member of it ever since. He spent the 1939–1940 year as a Carnegie fellow in anthropology at Columbia University and spent two years as chief of human resources research for the Army in the early 1950s. Since 1956 he has held the George Cary Comstock Research Professorship in Psychology at Wisconsin.

But if his residential history is relatively tranquil, his scientific career is full of innovation, discovery, and invention. It is well known that he established at Wisconsin one of the earliest primate laboratories in the country. What is not so well known is that the establishment of that laboratory resulted from one of many fortunate accidents that Harry Harlow has known how to exploit to scientific advantage. Harlow had completed his doctoral dissertation at Stanford under Calvin P. Stone, running rats on elevated mazes, and he expected to continue this line of work. Upon his arrival in Madison he inquired the location of the animal laboratory, only to be told that it had been torn down the previous summer. The wife of one of his colleagues, noting his dismay at this news, suggested helpfully that he go out to the Vilas Park Zoo and conduct his research on the monkeys there. He had never worked with monkeys or even seen one at close hand, but implausible as such a plan must have seemed to him, the young investigator explored it. The experience converted him because, as he later remarked, "monkeys were so much more broadly able than rats." His delayed-reaction-time experiments at Vilas Park Zoo were a success, and he set out to build his own facility.

In that early primate laboratory, Harlow began his 40-year scientific journey through the intricacies of rhesus psychology. He first undertook to investigate the learning ability of monkeys by studying the effects of lesions in specific cortical regions. One thing led to another. The learning research program required a large number of monkeys, so a breeding program was started. In order to reduce the incidence of contagious disease and to standardize the early experience of the subjects, baby monkeys were separated from their mothers a few hours after birth and reared in individual cages. The effect of this separation from mother began to be appreciated as Harlow and his colleagues noticed the strong attachment which infants developed for their cheesecloth baby blankets. A flash of insight led to the invention of the surrogate mother in 1957, and to the series of experiments on contact comfort. A marvelous bevy of surrogate mothers were constructed: mothers of terry cloth, hardware cloth, satin and silk; lactating mothers and dry ones; mothers with painted faces or merely bald knobs; heated and refrigerated mothers; mothers that rocked and stationary mothers. The trauma of tactile deprivation and the consolation of clinging led Harlow to an infatuation with skin. In his presidential address to this Association, he praised cutaneous contact throughout the mammalian world —in verse. Regarding the hippopotamus, he declaimed:

> This is the skin some babies feel
> Replete with hippo love appeal.
> Each contact, cuddle, push and shove
> Elicits tons of baby love.

The research on social attachment in relation to security and exploratory behavior led back again to learning, especially the infant's learning how to communicate and to play. Infant play, Harlow observed, is "the primary activity leading to effective age-mate love."

The varieties of love—maternal, infant, peer, heterosexual, and paternal—were a major theme in

his research for a decade after the construction of the first surrogate mother, although he never lost interest in the ability of rhesus to solve problems as well as to learn appropriate social roles. The significance of rearing in isolation became dramatically clear when "motherless" monkeys (i.e., females reared in isolation) themselves became mothers and displayed an appalling disregard for their offspring, ignoring them or even abusing them violently. Harlow also observed, in monkeys reared in isolation, the occurrence of something very like the anaclitic depression reported by Spitz and Bowlby on the part of human infants when separated from their mothers. Following this lead in a series of experiments took Harlow to the systematic study of depression in monkeys and, eventually, to ingenious attempts to relieve that condition, using monkey psychotherapists as well as heated surrogate mothers.

This summary is an altogether too superficial sketch of the adventures of a scientific argonaut, who has not yet found the golden fleece but has discovered that the skin you love to touch is mother's. The full account of the journey is available in the *American Scientist* for 1972, lucidly and modestly written and sprinkled with Harlowian aphorisms: "There is more than milk to human kindness"; "A mother's face that would stop a clock will not stop an infant." "There is only one social affliction worse than an ice-cold wife and that is an ice-cold mother"; and "Female beagles have warm and wondrous love affairs despite the fact that they are basically bitches." The story of his research is the story of his professional life—truly a quest for knowledge—knowledge which, as he says, "is forever changing" and hence "the search for knowledge never ends." It is a story plainly told by a plain-spoken man who takes his task with the utmost seriousness but takes himself with a measure of humor and is utterly without pretension. He knows and acknowledges the role chance plays in scientific discovery and we recognize, as we follow his progress, the role played by a prepared mind.

Harry Harlow's scientific bibliography is appropriately lengthy, as is his list of distinguished students. He has served psychology in other ways as well. Besides the presidency of APA in 1958, he has presided over the Midwestern Psychological Association and the Division of Experimental Psychology. He edited the *Journal of Comparative and Physiological Psychology* from 1951 to 1963, although he claims that the real work was done by the late Margaret Kuenne Harlow.

This year's recipient of the Gold Medal has been honored before—many many times. He was given the Distinguished Scientific Contribution award by this Association in 1960. He won the Warren Medal in 1956, and received the National Medal of Science from Lyndon Johnson in 1967. He is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society. To this man so often honored, with honors richly deserved, the American Psychological Foundation is proud to award its Gold Medal, together with a check for $1,000 and a scroll whose citation reads: "To Harry F. Harlow—a creative scientist and dedicated investigator, he has enlarged the science of man through artful experimentation with monkey."

Distinguished Contributions to Education in Psychology Awards

The American Psychological Foundation has also been accustomed to recognize and reward distinguished contributions to education in psychology. These awards are given to individuals who have made unusual contributions to instruction in psychology either through their own teaching or through other instructional functions such as developing new curricula. The award is a check for $1,000 to be used for the improvement of education in psychology in whatever manner the awardee may decide, and a citation indicating the nature of the contribution. Two awards are being made this year on the basis of recommendations by the APF Committee on Teaching Awards which was chaired this year by Jay Knopf.

JAMES B. MAAS

James Maas received his bachelor's degree from Williams College and his master's and doctorate degrees from Cornell University. He became an assistant professor of psychology at Cornell in 1965, where he has continued. He has recently accepted the position of Director of the Center for Improvement of Undergraduate Education at Cornell.

In 1972 he received the Clark Distinguished Teacher Award at Cornell University. He is singled out as a recipient of this APF award because

James B. Maas

of his total and outstanding commitment to undergraduate teaching and for the innovative development of methods, procedures, and materials for the teaching of large numbers of students.

At the present time, Maas is teaching in the introductory psychology program that has a classroom enrollment of 1,300 students per term. Student ratings of this program place it at the highest rank in the University. A major feature of this course is the creative utilization of multimedia presentations which have been in large part developed by Professor Maas. He devised a slide program for general psychology which has been adopted by more than 1,200 colleges and universities, and a series of other formats which have been in great demand and use. He developed the Cornell Inventory for Student Appraisal of Teaching which is being used at over 300 universities. He has imaginatively used "candid camera" episodes in the teaching process. He introduced at Cornell a graduate seminar in the teaching of psychology. These innovations demonstrate his commitment to excellence in teaching and extend his influence on the development of future teachers.

James Maas delivered at this Convention his presidential address to Division 2 on the subject of educational innovation—an interest that is reflected in the citation which accompanies this award: "To James Beryl Maas—whose style and ingenuity in teaching have suggested imaginative ways in which media technology can be introduced effectively. He has amply demonstrated significant educational effects in traditional classroom settings."

FRANK JOSEPH MCGUIGAN

Frank McGuigan received both his bachelor's and master's degrees from the University of California, Los Angeles, and his doctorate from the University of Southern California in 1950. His academic career began at the University of Nevada from which he moved to become a research scientist for five years at the Human Resources Research Office. He took up teaching again at Hollins College, Hollins, Virginia, in 1955 as professor and chairman of the Department of Psychology, where he has been ever since. His selection as recipient of this award is in recognition of his long-standing dedication to scholarship, to the promotion of teaching excellence in a small liberal arts college, and to the implementation

Frank J. McGuigan

of quality education in psychology through his own example.

Under his leadership, the Psychology Department of Hollins College became recognized for producing students who have gone on to take advanced degrees in psychology. Professor McGuigan has written several effective undergraduate texts. He has been active in the evaluation of teaching in a number of educational settings ranging from grammar school through the university level, as well as adult education programs. At Hollins College, he began early to encourage the involvement of qualified undergraduates in collaborative research with faculty members. He proposed and developed a master's program at Hollins, concentrating on preparatory work for the PhD. Since its inauguration in 1958, 89 out of 134 students have entered into PhD programs around the country. Letters from former students uniformly attest to his outstanding classroom performance as "challenging and meaningful." He consistently has been able to attract faculty of the highest quality in both research activities and teaching excellence.

His citation reads: "To Frank Joseph McGuigan —A successful teacher and scholar, because he is, first, a genuinely zealous student of psychology and a powerful source of inspiration in deed and word to those who have worked with him."

National Media Awards

These awards are offered as a means of recognizing and encouraging the communication of psychology to the general public by reporters, magazine and free-lance writers, television, radio, films, and books. Awards are presented in six categories (only five awards were made this year, however, and next year the television and film categories will be combined). In addition, a Grand Prix winner is selected from the winners of the five categories. Each winner received three days' expenses to the APA's Annual Convention in Montreal and a citation. A record total of 162 entries were received in the six categories this year.

The first winner of the Grand Prix award was Gerald Jonas for his two-part series on the work of Neal Miller which appeared in the August 19 and 26, 1972, issues of the *New Yorker* magazine. Jonas also received the award in the magazine category.

Other winners included: *Television*—"The Masks We Wear," produced by ABC News and Jules Power Productions, narrated by Harry Reasoner; *Radio*—"The IQ Fallacy," produced by the Great Atlantic Radio Conspiracy and aired on WBJC-FM in Baltimore, Maryland; *Newspaper reporting*—Christine Darg of the Richmond *Times-Dispatch* for a series of articles on the human potential movement; and *Books/monographs*—Elliott Aronson for the book *The Social Animal* published by W. H. Freeman and Company.

In addition to the five winners, honorable mentions and letters of special commendation were awarded to a number of entries in all six categories.

1. In the newspaper reporting category, honorable mentions were awarded to Margaret Deats Bott for a series of articles in the *Galveston* (Texas) *Daily News* on mental health; Tom Darcy, a political cartoonist for *Newsday* (Long Island) who wrote an autobiographical account of his battle against mental illness entitled "After the Recovery, Years of Judgement"; Duston Harvey, a staff writer for *United Press International*, for an article entitled "Controversy Over Black Vs. White Intelligence Rages On"; Barbara Katz, a reporter for the *National Observer*, for the article "Finding Psychiatric Help Can Be Traumatic Itself"; and Paul Kidd, for a series of articles in *The Spectator* (Hamilton, Ontario, Canada) on "Mental Health in Hamilton." Tomi Knaefler, a reporter for the *Honolulu Star Bulletin*, received a letter of commendation for her coverage of the APA Annual Convention in Honolulu last year.

2. In the books/monographs category, honorable mentions were awarded to Robert E. Ornstein for *The Psychology of Consciousness* published by The Viking Press (New York City); and to James Leslie McCary for *Human Sexuality* published by Van Nostrand Reinhold Company (New York City). Hilde S. Schlesinger and Kathryn P. Meadow received a letter of special commendation for *Sound and Sign: Childhood Deafness and Mental Health* published by the University of California Press (Berkeley).

3. In the magazine writing category, honorable mentions were awarded to Kenneth Goodall for "Shapers at Work" which appeared in *Psychology Today* magazine and to Philip Zimbardo for "Vandalism: An Act in Search of a Cause" which appeared in *Bell Telephone* magazine.

4. In the television category, an honorable mention was awarded to "The Lee Phillip Show" which appears on WBBM-TV in Chicago. A letter of special commendation was sent to Matt Duncan for

a special introductory course devised for educational television in Southern California entitled "As Man Behaves."

5. In the radio category, honorable mentions were awarded to Connie Goldman of KSJN radio in Minneapolis, Minnesota, for "Margie Talks About Death and Dying" and to Carol Kadushin of National Public Radio for a series of shows entitled "It's All in Your Mind."

6. No film winner was selected, but a letter of special commendation was sent to Richard I. Evans of the University of Houston for the film *Psychology of Creativity*.

The National Media Awards program was established by the American Psychological Foundation in 1956 to honor outstanding reporting on psychology in the national media. The 1973 winning entries were produced sometime between May 1, 1972, and May 1, 1973.

Announcement by Henry Riecken of the Rosen Bequest

Finally, I would like to take advantage of this occasion to make a special announcement. It is the sort of announcement that gives the President of the American Psychological Foundation a special pleasure, for it concerns an opportunity to expand and deepen the Foundation's program in psychology.

At the meeting this morning, the Trustees accepted with gratitude the bequest of Esther Katz Rosen. In her will, Dr. Rosen made the American Psychological Foundation the residuary beneficiary of her estate, to be used "for the advancement and application of knowledge about gifted children," giving the Trustees fairly broad discretion in interpreting those terms. The size of the Esther Katz Rosen fund is not yet precisely known since the estate is still being settled, but the bequest is a substantial as well as a generous one.

Esther Katz Rosen, who died at the age of 77 last April 13, was a practicing clinical psychologist as well as a civic leader in Philadelphia for more than 35 years, specializing in work with children. She was a former member of the Board of Examiners of the Pennsylvania Psychological Association and a former secretary of that state association as well as a fellow of APA. Dr. Rosen was educated at Goucher College and received both an MA and PhD from Teachers College of Columbia University, where she was a Runyan Scholar as well as an assistant to Edward L. Thorndike at the Institute of Educational Research. She was also a diplomate in Clinical Psychology of the American Board of Professional Psychology. A consulting psychologist in private practice in Philadelphia, she specialized in diagnostic evaluation and guidance with children and youths as well as offering advisory services to parents. She was, at the same time, active in Philadelphia civic affairs, serving on the Philadelphia County Board of Assistance, the Association for Jewish Children, the Crime Prevention Association, and various committees of the Health and Welfare Council of the City. She was an ardent believer in civil rights and a worker for equal rights for women. Those who knew her personally speak with great warmth and affection of her lively curiosity, her sense of humor, and her vibrant personality. In writing about Dr. Rosen, one of her nieces concluded with this remark: "Her love and understanding of children was very deep and she left her bequest to the American Psychological Foundation because she had confidence that it would use the funds wisely in accomplishing her objectives for gifted children."

The Trustees are very much aware of their responsibility to use these funds wisely, and have begun a deliberative process that will probably extend over some period of time before a conclusion is reached as to the best way of using the Esther Katz Rosen Fund for its founder's purposes. The Trustees would like to draw on the wisdom and the advice of members of the APA and have asked me to make known their willingness to receive your suggestions about the best ways of advancing and applying knowledge about gifted children. I hope that if you have any thoughts along this line that you will write to the President of the American Psychological Foundation in care of the American Psychological Association in Washington, D.C.

Comment

Some Comments on Hersch

As this comment is essentially a criticism of "Social History, Mental Health, and Community Control" (Hersch, August 1972), it is appropriate to review briefly the article. Hersch sees the mental health movement as being in a "kinship relationship [p. 749]" to the other movements of the 1960s, with the mental health movement reflecting an environmental reform point of view. He discussed shifts in ideology that he thinks have occurred: (*a*) from a traditional, clinical frame of reference to a public health frame of reference, and (*b*) the addition of social action ideology to the preceding frames of reference. The social action ideology resulted in the involvement of mental health professionals in community control. Unforeseen problems of community control resulted in professional and personal difficulties for those mental health workers who tried to function within the community control framework. After his analysis of these difficulties, Hersch concluded that "it would be unfortunate if we should prematurely reject the community control principle because we lacked the facility and skills for working within it [p. 754]."

Hersch's analysis was based on the assumption that the growth of the mental health movement and the subsequent involvement of mental health professionals developed from a desire to implement democratic and humanistic principles. This assumption is fundamentally in error, with the result that his analysis contained a series of errors that developed as a consequence of his initial assumption. It is a fundamental error because it ignores the social unrest and developing power struggle of the 1960s. Ignoring these social and political realities in an attempt to analyze the development and difficulties of community control, a social phenomena whose very development is a part of these same realities, leads to conceptualization and analysis that does not deal with critical issues. Only ignoring the social and political realities of the 1960s could result in the error to be discussed in the next paragraph.

Writing in 1972, after Watts, Detroit, and Newark, Hersch discussed the anger encountered by mental health professionals working within community control. This anger, coming from the local residents, directed toward the professionals, was described in this way, "it is as if there is the emergence of a deep-seated and long-standing anticolonialist rage [p. 753]." To describe this rage on an as-if basis reflects a refusal to deal with the full reality of the lives of the oppressed and the disadvantaged. One might as well try to analyze Watts, Detroit, and Newark with "as if"!

As a basis of discussing the incorporation of a social action ideology into the community mental health movement, Hersch defined social action. His definition derived from his original position of democratic and humanistic principles with the thrust of social action being for "the well-being of the population or community as a whole [p. 750]." In his definition he omitted the following crucial characteristics of social action: (*a*) Social action is contingent upon the existence of pressure groups desiring a change in their organizational and/or political environment; (*b*) pressure groups can only effect change as a function of the power they exert; (*c*) desired change may range from modifying and limiting to destroying the particular power structure involved; (*d*) the results of a social action approach may not benefit the larger society; and (*e*) social action has a potential for destruction. It follows logically from these characteristics that the realities of the social action arena are contest, bargaining, conflict, confrontation, threat, and violence. The professional mental health workers did not understand social action in these, or similar, terms and encountered unexpected difficulties. Those who understand social action in these terms would encounter difficulties but not those developing from unanticipated or unrecognized realities. It is obvious from the problems with which they struggled that the mental health professionals were not knowledgeable of, let alone skilled in, social action. Hersch recognized that the professionals were not prepared for the problems that developed in community control situations, and he offered an explanation. His explanation, however, did not provide a basis for avoiding future errors. It could very well be considered a sophisticated rationalization:

> Perhaps out of a reawakened awareness of democratic principles, perhaps out of frustration for the failures of the past, perhaps out of a sense of collective guilt—for whatever mix of reasons, there was a rush to community control to provide the answers that were not available before [p. 752].

Using an inadequate definition, Hersch proceeded to discuss the incorporation of the social action ideology into community mental health. He sees a shift from providing helping services to "consumer groups" to programs for promoting the political power of these groups. Consistent with his basic assumptions and previous errors, he used the concept "consumer group" for the disadvantaged. This is, again, an error that derives from the lack of recognition of the social and political realities. The crucial characteristic of a "consumer group" is that it has purchasing power—the ability to decide what, when, and where it will purchase. A recipient group has the needs for services but lacks the purchasing power. The disadvantaged have been recipient groups, not con-

sumer groups. To describe them as consumer groups is to ignore their essential powerlessness. If there is no recognition of the fact of powerlessness and the conditions that make for powerlessness, there can be no anticipation of the problems that will occur when a group attempts to change from powerlessness to holding some degree of power. This was one of the sources of difficulty for the mental health professionals involved in community control.

Hersch's analysis of three problems in community control was consistent with his preceding analysis in that he continued to ignore social and political realities. The problem of the undemocratically selected, pathologically motivated leader is discussed in terms of personal dynamics. Having labeled these leaders diagnostically in terms of their pathology, he has implicitly laid the basis for either emotionally dismissing them as leaders, or rationalizing anger toward them. He did not include in his analysis the fact that pathological or not, undemocratically selected or not, these leaders must be dealt with in the context of their role in the community.

The second problem of community control that concerned Hersch was that power is "often not wielded for the sake of the rational development of the program [p. 753]." At the risk of sounding cavalier, our own personal reaction to this was "so what else is new?" Middle-class professionals experienced in the use of such power have been known to misuse it. Hersch acknowledged this in his statement,

> Now it is true that the professional community had frequently failed to assume its responsibilities where the disadvantaged were concerned. Where it did take responsibility, it often did so on its own terms, and therefore inappropriately, if not destructively [p. 752].

(Having clearly diagnostically labeled the pathological leader, we cannot understand why Hersch did not analyze the behavior of the professional community in the diagnostic terms of *character disorder*.)

The third problem dealt with by Hersch was that of the anger of the local residents toward the professionals. He correctly described the ways in which the rage is expressed and the effects of these expressions of rage. Inasmuch as he dealt with this rage as previously indicated, on an as-if basis, he denied the valid basis for this rage. He cannot, therefore, recognize that the middle-class mental health professionals are the representatives of the larger society responsible for the conditions engendering the rage. Not too surprisingly, he continued by acknowledging that the aforementioned problems are not exclusive to low-income community control boards (excluding the rage problem), but stated that they are more frequent and vivid with low-income boards. The acknowledgment does not, however, proceed to ask the most important question: If these problems do occur with middle-class boards, and professionals function on and with these boards, why did the occurrence of this problem behavior by lower-class board members take the professionals by surprise?

In referring to the people who encountered all of these difficulties and reacted with a "kind of professional backlash [p. 752]," Hersch was willing to say, "The fault was their own . . . . It was folly, of course, to expect that greater nobility, deeper wisdom, and less human frailty would be found among the poor than among the well-to-do [p. 754]." He offered as an explanation for this mistaken behavior "their idealism and zest for the new . . . [p. 754]." Idealism and a desire for novelty are hardly an adequate explanation for inadequate behavior by a group of professed experts. The professionals went into community control activity, ignoring the social and political realities, ignoring the history that preceded them, and ignoring the fact that they are part of a larger society which is primarily responsible for this very history. They were unaware of the fact that they were equipped with only their good intentions and became angry when their good intentions were spurned. This is the behavior of elitism, benevolent in intention, but nonetheless elitist, and Hersch, again, did not indicate this as part of his analysis.

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George Siskind
Larue D. Carter Memorial Hospital
Indianapolis, Indiana

Was This Article Necessary?

It is unfortunate that the *American Psychologist* chose to give the unopposed impact of primacy and prominent location to a sweeping, unqualified negation of an entire area of behavioral science, namely, intelligence testing (McClelland, 1973). No doubt those who seek any available support for their antagonism to testing will gladly accept the apparent conclusion that nothing about testing is positive and that it remained for the January 1973 issue of the *American Psychologist* to make this known.

Among the observations that could be made about the article are these:

Logically, the first and most interesting result of the McClelland contribution should be a skeptical review of the author's own distinguished studies in the field of achievement. Portions of them are based on techniques similar to those involved in the standardization of intelligence tests. They depend, in fact, partly on projective techniques, whose validity has been questioned even more than that of intelligence tests.

The large number of employees of the Educational Testing Service as well as the estimable nature of its earnings are referred to as if these were comments on the correctness of the organization's work. The effect of their tests, "which have tremendous power over the lives of young

people by stamping them 'qualified' and 'less qualified' for college work [p. 1]" would suggest the evil intent of some Boris Karloff-like character in charge. Are these allegations germane to the adequacy of tests?

"Tests predict grades in school," it is stated, "because the games people are required to play on aptitude tests are similar to the games teachers require in the classroom [p. 1]." Far from being a detraction from such tests, there could hardly be a greater compliment. The question here is society's current approval of classroom "games" as a proper criterion. If that criterion is judged unacceptable, it should be changed, after which the predictors would require adjustment. Clearly, the game playing referred to and so strongly condemned by the author is a prime example of criterion sampling. A remarkable contradiction appears when a later passage stresses precisely the point that "the best testing is criterion sampling [p. 71]." In any event, the study of job behavior as a source of test items has been described in the literature for years. It is, and has been for over a generation of students, an elementary part of their education in test techniques.

Further reference to the criterion issue is offered in the statement that

> even a little criterion analysis would show that there are almost no occupations or life situations that require a person to do word analogies, choose the most correct of four alternative meanings of a word, etc. [p. 7].

Here, the well-known fact that test items often relate indirectly to the criterion is overlooked. They need not have face validity.

Selective emphasis of data is apparent in the presentation of Thorndike and Hagen as "good guys" because they did not find encouraging correlations between scores and later success. On the other hand, the statement that "most psychologists think so," referring to general confidence in this type of prediction, is promptly minimized (p. 3). It should be added that although the Thorndike and Hagen predictions were by no means positive in all respects, they did uncover correspondence between the type of work in which the subjects were employed and tests taken at an earlier time.

In one portion of the article the reader is warned that "We know that correlation does not equal causation, but we keep forgetting it [p. 3]." Is there a suitable text or qualified teacher who has not included this caution in an early stage of instruction?

Is it really necessary at this late date to become intensely concerned about the theoretical aspects of the "role of power in controlling life-outcome criteria [p. 4]"? The part played by culture in behavior of all kinds, including adaptable behavior, has caused spasms of breast beating and abject cries of confession by so many psychologists and for so many years that surely we would administer the death blow of boredom to students by offering the thought as a scoop in 1973. Is it really a fact that large numbers of psychologists in the test field spend their time in "their offices where they play endless word and paper-and-pencil games," when they should "go into the fields where they actually analyze performance into its components [p. 7]"? If there is actually one psychologist who does fit this description, he exemplifies the uninformed person's stereotype of the armchair psychologist who arbitrarily makes up test items without validating them. Apparently, McClelland does not understand standardization, the basic factor in test construction.

Finally, the view that intelligence tests directly measure innate qualities has been of historical interest alone for decades. Nevertheless, it was thought necessary to state that "stability is supposed to mean that the score reflects an innate aptitude that is unmodified by experience . . . [p. 8]." It would be a challenge to locate more than an isolated few who take this stand today.

The old slogan is reversed: Emphasize the negative and forget the positive.

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Robert Tyson
Herbert H. Lehman College of the City University of New York

McClelland on the Terman Data

McClelland (1973) argued that psychometricians have overemphasized intelligence, as measured by IQ tests, and that they should devote more effort to the development of tests allowing the prediction of competence in specific vocational categories. Most of his major points seem well taken. However, one can argue with his interpretation of the data from Terman's studies of genius. The major thrust of a large portion of McClelland's presentation came from his analysis of the Terman data. Further, McClelland's comments concerning the relations between parental socioeconomic advantage and children's success in the Terman studies of genius have relevance to the recent spate of papers speaking to the same general issue. Therefore, it appears useful to examine the Terman data, especially those reported in the first (1925) and fifth (1959) volumes devoted to Terman's study of genius, in order to determine the degree to which a close examination of these data supports McClelland's interpretation of them.

McClelland (1973) wished

> to state, as emphatically as possible, that Terman's studies do *not* demonstrate unequivocally that it is the kind of ability measured by intelligence tests that is responsible for (i.e., causes) the greater success of the high-IQ children. Terman's studies *may* show only that the rich and powerful have more opportunities, and therefore do better in life . . . . By current methodological

standards, Terman's studies (and others like them) were naive. No attempt was made to *equate* for *opportunity* to be successful. His gifted people clearly came from superior socioeconomic backgrounds to those he compared them with (at one point all men in California, including day laborers). He had no unequivocal evidence that it was "giftedness" (as reflected in his test scores) that was responsible for the superior performance of his group. It would be *as* legitimate (though also not proven) to conclude that sons of the rich, powerful, and educated were apt to be more successful occupationally, maritally, and socially because they had more material advantages [p. 5].

Terman's gifted group did come from an economically advantaged group. Possibly, in a moderately open society, one would expect those parents who have genes disposing themselves and their offspring toward brightness, as measured by IQ, to have done better economically than the population at large. Presumably, to "equate for opportunity," one would find parents similar to the parents of the gifted in education, stability, initiative, etc., who were day laborers. This might be a hard group to find, assuming that social mobility does occur and is, in part, a result of attributes of the individual. How advantaged were the parents of Terman's gifted sample? If scoring well on IQ measures is a consequence of environmental advantages resulting from superior socioeconomic status (McClelland, 1973, p. 6), then we can compare the offspring of the gifted group with the gifted group itself. The proposed examination will tell us a bit about how much effect environmental advantage has on such attributes as IQ; about the degree to which *pull* (McClelland, 1973, p. 4) is involved in vocational success.

The mean IQ of Terman's gifted group was 151 (Terman, 1925, p. 45). Various ways were used to categorize the parents of the gifted. One categorization (p. 62) gave the following information concerning the occupations of the parents of the gifted: professional, 29.1%; commercial, 46.2%; industrial, 20.2%; and public service, 4.5%.

Another measure of the occupational status of the parents of the gifted was obtained through the use of the Taussig Scale, which more closely resembles the Minnesota Occupational Scale, which was used to measure the occupational status of the gifted group themselves. This scale provides the following information: professional (similar to Group I, Minnesota Occupational Scale), 31.4%; semiprofessional, higher group (Group II, Minnesota Occupational Scale), 31.2%; lower group (Group III), 18.8%; skilled labor (Group V),[1] 11.8%; semiskilled labor (Group VI), 6.6%; and unskilled (Group VII), .13% (Terman, 1925, p. 64). A third analysis led Terman (1925) to state that "the mean of 12.77 for total fathers of gifted corresponds to the Barr[2] rating of a stenographer, librarian in a small city, or primary teacher [p. 71]." Of the parents of the gifted sample, 35.3% had family incomes below that of the average of skilled laborers in California at that time (p. 72). All in all, there seems to be no great amount of *pull* on the part of the parents of the gifted. They are not as "rich and powerful" as McClelland suggested.

The gifted offspring themselves were studied last in 1955 (Terman & Oden, 1959). Data concerning IQ (but not occupation) were obtained from the offspring of the gifted sample. It is clear that the offspring of the gifted grew up in better socioeconomic circumstances than did the gifted group themselves. Among the gifted males, 86.3% were in Classes I or II of the Minnesota Occupational Scale (p. 74). Of the employed gifted women, 65.1% were categorized as professional persons (p. 87). Of the husbands of the gifted women, 73.9% were in Categories I and II of the Minnesota Occupational Scale (p. 138). Only 15.5% of the wives of gifted men were employed. Of those employed, 56% were categorized as professional persons. The mean family income of the gifted was $10,866, at a time when the mean family income of the urban white population (itself a higher than average income group) was $5,069. Only 6% of the gifted had incomes below $5,000 (pp. 100–101).

The gifted were more successful than their parents. If "getting a high IQ" results from socioeconomic advantages, the children of the gifted should score higher than their gifted parents. They did not: The mean IQ for both male and female offspring is 132.7. This is well over one standard deviation lower than the scores of the gifted parents. The spouses of the gifted parents were tested with the Concept Mastery Test, not the Stanford-Binet, and no IQs are reported. However, the occupational levels of the spouses of the gifted are similar to the occupational level of the gifted of their respective sexes. The Concept Mastery Test scores of spouses (Terman & Oden, 1959, pp. 56–60) suggest that the mean IQ of spouses, had they been tested with a test yielding an IQ, probably would have been above 120 at the least, since the spouses did a bit better than a comparison group of electronic engineers and scientists (p. 60). The midparent IQ of offspring of the gifted probably is between 135 and 140; the occupational level of the gifted is high, as compared with that of the parents of the gifted. Yet, the IQ of the children of the gifted is lower than that of the gifted themselves or of the mean of gifted and spouses of gifted. This result would be expected from a genetic interpretation of giftedness—in fact, this is what Galton suggested long ago (e.g., Galton, 1892, p. 74) and might be expected in any situation involving polygenic inheritance. The regression toward the mean in IQ (Galton's "Law of Filial Regression") cannot easily be explained in terms of environmental advantage leading to skill at taking IQ tests, because the gifted provided a sub-

[1] Group IV, in the Minnesota Occupational Scale, is an agricultural group.

[2] Another system of occupational rating.

stantially more superior environment for their offspring than that which they themselves received. We have no data concerning the degree to which parental "clout" influences the occupational outcomes of the offspring of gifted parents. The fact that parental environment input did not lead to an increase in tested ability (although it may have reduced the expected regression toward the mean) suggests that the effects of parent education and parent income and of *pull* on performance are not as great as McClelland posited. I suspect that all of us will agree that environmental advantage and *pull* are helpful. The question is, Does advantage account for the preponderance of the variance in IQ and *pull* for much of the variance in occupational success, as McClelland suggested as an alternative explanation to that of Terman? It seems doubtful that such is the case.

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RONALD C. JOHNSON
University of Hawaii

On McClelland's "Testing for Competence . . ."

Some statements in McClelland's (1973) article need to be drawn from his polemic and examined with care, otherwise the reader may get the impression that he really does not mean his disclaimers, such as "I do not want . . . to dismiss intelligence tests as irrelevant or unimportant . . . [p. 5]." For example, note the statement that

> while grade level attained seemed related to future measures of success in life, performance within grade was related only slightly. In other words, being a high school or college graduate gave one a credential that opened up certain higher level jobs, but the poorer students in high school or college did as well in life as the top students [p. 2].

It is reasonable to ask why McClelland neglected to point out that grades might have something to do with the achievement of the credentials? While many things may enter into school completion patterns, including all of those social factors we are concerned about, did McClelland want us to believe that grades (which correlate with IQ and achievement tests) do not correlate with the amount of schooling one gets? In his example that followed, to show that grades do not predict success in later life, McClelland completely neglected to point out that the groups examined may have been highly selected already. By whatever selection procedures were operating, are we to believe that Wesleyan University students were not selected into that University because they had "potential" as demonstrated by some prior performances? This might make likely the surprisingly high success of his group of students with averages of C— or less. But, *surprising* is not a strong enough word for his finding that the low group had lawyers, doctors, research scientists, and college and high school teachers (in a sample of eight); *incredible* would be more appropriate, as I find it hard to believe, retrospectively, that in those days Wesleyan University had such magic that students with C— or less would get accepted to medical, law, and graduate schools. In those days, you were not even granted a degree with less than a C— average in most places. But possibly my memory of the time is poor, which is not the point. The point is that somehow McClelland has conveniently neglected to mention that some selective processes have already occurred by the time people reach a given level, such as college graduate, and the majority of these survivors may well be able to learn more and be successful in a wide range of occupations. Doesn't this have something to do with the difficulty in relating grades with his criterion of subsequent success?

Then, his rhetoric may bother one in its implications. "One would think that the purpose of education is precisely to improve the performance of those who are not doing very well [p. 2]." This surely can be one purpose, but please, Is it okay if we also suggest some attention for those who are doing reasonably well and even for those who are doing very well as part of the purpose?

There is also the element of the implicit cultural conspiracy of which we are all obviously a part. "For the tests are clearly discriminatory against those who have not been exposed to the culture, entrance to which is guarded by the tests [p. 7]." "Until recent 'expectations' were made (over the protest of some), the tests have served as a very efficient device for screening out black, Spanish-speaking, and other minority applicants to colleges [p. 1]." But it seems that some "groups" do well with the tests and grading system. Possibly we have to suspect that all of the time, Jews, Japanese, Chinese, maybe Americans, and a few others have been the dominant element of the power elite bent on maintaining itself in this society.

If we are concerned with the problems of education, are we automatically supposed to respond favorably to such nonsense? There are some interesting and potentially useful suggestions for improving the system, including some that McClelland mentioned in the course of his article. The kind of nonsense he advanced with his presentation distracts from our attention to these.

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EDGAR F. BORGATTA
Queens College of the City University of New York

McClelland Replies:

In writing my article (McClelland, 1973), I consciously chose to write in a challenging, emphatic style, for the very simple reason that I had said many of the same things in *Talent and Society* over 15 years earlier in a sober and cautious way, and as far as I could determine no one paid the slightest attention (McClelland et al., 1958). For that matter, others like Cronbach in his excellent text on testing have also said many of these things in a less provocative way, and still the testing movement has marched on totally unmoved by logic or research evidence. Yes, Dr. Tyson (1974), textbooks regularly state that correlation does not equal causation, but that does not stop the testing profession from arguing that because the average IQ score for stockbrokers is higher than for carpenters, it takes more intelligence to be a stockbroker than a carpenter. I thought it might be at least worth trying to stir up things a bit and get people thinking a little harder about what they were doing. In so doing, I tried to be careful even though provocative, but obviously my commentators were so provoked that they couldn't notice the cautionary remarks or felt they were obviously inconsistent with what I "really" thought. I see little point in extending the discussion of what I did or didn't say, did or didn't mean. I'd rather put the energy into developing the new kinds of tests I think we need, and I hope others will do likewise.

But I certainly was pleased that someone was stimulated to start looking at Terman's data from the point of view I was urging. That was precisely the kind of outcome I hoped for from my article. Yet Johnson (1974) simply could not believe that I meant what I said in stating that "it would be *as* legitimate (though also not proven) . . . [p. 5]" to argue that pull accounted for the success of Terman's gifted group. He felt I must have meant that pull accounted for more, or perhaps all, of the results. The fact is I have no prior conception of how the results would come out, except that I would be very surprised if both heredity and environment didn't contribute something to success. I was merely pointing out that Terman, and those who cite him, regularly fails to mention pull even as a *possible* source of variance in success.

So any attempt to begin to shed light on this problem is to the good, though I found Johnson's analysis rather incomplete. What would seem to be called for is (*a*) a comparison of the success of persons with high IQ from families with high and low socioeconomic status and (*b*) eventually something like a stepwise regression for predicting success based on such variables as status of parents, IQ, etc. In such a design it is possible to estimate the contribution of each factor to the outcome. If Johnson or anyone else has access to the original Terman data, I should think it would be very valuable to carry out such a reanalysis of the data.

Only one other factual point seems at issue. Borgatta (1974) simply doesn't believe the data I reported on students at Wesleyan University. I would be glad to send him the names of the people, their grade averages at Wesleyan, the names of the medical and law schools they got into, etc. What he perhaps forgets is that such students with poor averages could still, in those days, get into southern medical and law schools—fortunately for society, since many of them turned out to be quite distinguished—particularly if they served in World War II. His point about college graduates being selected already is well taken, even though their grades are terrible, but (*a*) that doesn't deal directly with the fact that we improperly use grades among this selected group to decide who should get into the professional schools and (*b*) it would be a more impressive point, if it didn't hold also for grades in high school, when the degree of selectivity is much less. Grades in college or in medical school have very little connection with success as a medical specialist, educator, or general practitioner (see Taylor et al., 1965). Grades in high school have very little value for predicting various measures of life success (see McClelland et al., 1958). It really is worth reading the literature in this area; you would be surprised, Dr. Borgatta; I was, when I first read it.

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David C. McClelland
Harvard University

A Voice from the Field[1]

Harris (March 1973) cited studies and made observations concerning Peace Corps problems of selection, self-assessment, attrition due to nonselection factors, the need for conditions of moderate stress during training, and the need for both volunteer and staff evaluation in the field, which are in agreement with the author's own personal views, derived from field experiences and relevant research. Harris is to be commended for the emphasis of his

[1] The substantive content of this comment is based on the author's personal experiences and participant observations in the field as a Peace Corps

studies in this area and his plea for building the science of selection on a strong foundation of field-oriented research. However, at the same time, it seems to the author that he implied that if this type of research were not conducted within a Peace Corps context, then it could not be validly generalized to specific Peace Corps problems nor be used to support the psychologist's role when challenged by a new administration.

This may not have been Harris's intent, but his failure to cite literature which the author finds relevant to the general problem of selecting young civilian adults for service in a remote environment to which they have not previously been exposed supports this conclusion. Furthermore, in view of the recent comments on commitments to field research discussed by Fried, Gumpper, and Allen (1973), Harris's apparent neglect of related studies appears to be at odds with his thesis advocating field-oriented research. It should be noted that the Peace Corps and its successor, ACTION, are not unique as volunteer organizations but do hold the distinction of being the largest and most widely publicized. Numerous volunteer programs exist and often duplicate Peace Corps activities. The U.S. International Volunteer Service (IVS) program, the British Volunteer Service Overseas (VSO) program, the New Zealand Volunteer Service Abroad (VSA) program, and other similar programs offer examples. All have some form of selection procedure, although they may not have been studied and documented. Nevertheless, the International Secretariat for Volunteer Service (ISVS) has published reports in this area (e.g., Denny, 1966; Quarmby & Quarmby, 1969; and Taylor, 1969c). In another area, a number of evaluative and research reports are available which address the problems of both civilian and military volunteer selection for service in the Antarctic as well as the adaptation and performance of the volunteer in the field (a representative partial listing includes: Doll, Gunderson, & Ryman, 1969; Gunderson, 1965, 1968; Gunderson & Kapfer, 1966; Gunderson & Nelson, 1963, 1964, 1965a, 1965b, 1966; Gunderson & Ryman, 1971; Law, 1960, 1963; Mullin, 1960; Nelson, 1963; Palmai, 1963a, 1963b; Seymour & Gunderson, 1971; Smith, 1961, 1966; Smith & Jones, 1962; Taylor, 1969a, 1969b; and Taylor & Shurley, 1971).

After having been selected for, and served, in both the Peace Corps and Antarctic programs, the author feels that there is a considerable overlap in the following areas: (*a*) selecting civilians for these programs, (*b*) the problems the volunteers will face in the (remote) environment, and (*c*) the type of individuals who volunteer. Antarctic volunteers are often young technicians, recent college graduates, or graduate students who apply for a year-long winter assignment. At least one psychologist (A. J. W. Taylor) has been active in selecting both New Zealand VSA and Antarctic volunteers. The U.S. Antarctic Research Program (USARP) is of particular interest in this area because the final word on civilian selection remains with the university department or government agency sponsoring the individual. Thus, it is often possible to obtain field performance data on individuals who may not have been entirely acceptable to one of the psychologist-psychiatrist teams that screen all USARP winter-over volunteers.

Harris chose to focus on the Peace Corps psychologist's role as selection officer and the often neglected role of researcher. He lamented the passing of both of these roles, but in so doing appeared to neglect a major argument for the retention of the Peace Corps psychologist. With an overall attrition rate exceeding 50%, one has cause for concern about the fate of these "failures." The Peace Corps may be viewed as one of the great experiments of this century in the application of humanitarian principles and idealistic efforts to the problems of underdevelopment and international relations with the non-English-speaking world. At a time when the profession of psychology is concerned with the rights of human subjects, it would seem that more than academic concern needs to be expressed for the effect of participating in the "Peace Corps experiment" upon the volunteer subject. This position will receive further discussion later, but first some other observations made by Harris appear to deserve comment from the field point of view.

Evaluation in the Field

The author is in full agreement with Harris's comments on the need for formal evaluation of performance in the field and the implementation of appropriate reward or disciplinary action. A low level of feedback on his performance to the Antarctic volunteer from supervisors in the United States appears to be stressful for these men, and signs of this stress become increasingly evident during

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Volunteer (PCV), and while serving as a research assistant in Antarctica. The opinions expressed by the author are personal and are intended only for clarification, and should not be construed as representing, condoning, or criticizing past or present opinion or policy of any agency of the U.S. Government.

The author received Peace Corps training at Hilo, Hawaii, in 1963 and subsequently served a full two years in the field as a community development worker (Thailand VII) during 1964 and 1965. In 1966–1967 he served in the U.S. Antarctic Research Program as a psychophysiological technician and spent a year at the Amundsen-Scott South Pole Station, Antarctica. Since 1967 he has been continuously a research assistant and associate and graduate student at the University of Oklahoma Health Sciences Center. Currently he is a doctoral candidate in biological psychology, Department of Psychiatry and Behavioral Sciences, University of Oklahoma Health Sciences Center, and research associate in the Behavioral Sciences Laboratories of the senior medical investigator (psychiatry), Oklahoma City Veterans Administration Hospital. The author may be contacted at the Hospital, BSL (151A), 921 Northeast Thirteenth Street, Oklahoma City, Oklahoma 73104.

periods when communication with the United States is not possible (Natani, 1971). Law (1960, 1963) made similar observations for Australian personnel. However, Harris suggested that field evaluation has been chronically absent in the Peace Corps organization. This view is at variance with the author's experience during his period of service.

Some formal evaluation of volunteers in Thailand was evident by Peace Corps staff, and this was implemented by visiting individual volunteers, by calling groups of volunteers in for formal meetings, and by holding discussions with individuals while visiting in Bangkok. Rewards were informally distributed to deserving individuals in the form of assistance on special projects, occasionally by supplying motorized transport, and by distributing results derived from their efforts to other volunteers, for example, the books of Kahn (1965), Madden (1965), and Natani (1965a). Also, productive volunteers were chosen for staff positions after completing their assignments. In general, volunteers were quite competitive and diligent in continually seeking ways to improve their performance. On the other hand, the presence of nonproductive individuals disturbed the dedicated who did not agree with the informal policy that attrition was undesirable and that these individuals should not be unduly pressured because this might precipitate their early termination.

A formal system of staff evaluation also appeared to be in operation, and evaluators were encountered periodically in Bangkok. Both the staff and the evaluators encouraged feedback and were open to suggestions. A copy of a report on development activities (Natani, 1965b) was transmitted to Washington, D.C., via an evaluator in 1965. In 1966 a formal recommendation was sent to the Peace Corps director (Vaughn). It contained suggestions for the establishment of career counseling and appropriate evaluation and reassessment procedures for all volunteers requesting (long-term) extensions of service.

The Problem of Attrition

Several points should be added to Harris's analysis of the problem of attrition. Attrition was already a problem correlated with increased in-country training in 1966 (J. T. English, personal communication). The apparent recent trend toward a reduction of early terminations in training and a rise in early field terminations with in-country training may reflect a group effect. It could also reflect a change, if any, in the policy concerning the undesirability of early field terminations. For in-country training, economic and other considerations involved in returning an individual to the United States now appear to be essentially the same for dropouts whether they occur during training or after assignment. The population of applicants may also have changed, and current trainees may have a different motivational reservoir for social action (Bernard, 1968). In short, the stress-seeking pioneers of the late 1960s and early 1970s may be applying their efforts to other areas. (Incidently, stress-seeking behavior is another neglected area of research.)

However, a hypothesized group effect may be a more potent factor influencing attrition in the Peace Corps setting. Under the former board system, the selection process appeared to foster interpersonal distance among the trainees, especially when they were involved in the assessment of fellow trainees by the use of peer ratings. In other settings, such as the Antarctic, peer ratings can have undesirable effects (Nardini, Hermann, & Rasmussen, 1962), but they may also be useful in promoting the introspection necessary for valid self-selection during Peace Corps training. With regard to Harris's comment on the conflict between the morale problems of peer rating and humanitarian values, the author feels very strongly that one must consider the stresses to which the volunteer may be subjected in the field. In making this consideration, the most humanitarian alternative seems to be the application of procedures that will promote attrition in training of those individuals who do not have firm self-identities and high levels of self-esteem. It would seem that only a tough-minded selection process, based on research as Harris (1973) suggested, will lead to his stated goal,

> to produce in even larger numbers men and women who are sufficiently realistic in their self-perceptions to be capable of deciding voluntarily what commitments they can fulfill and, having once decided, to be capable of fulfilling those commitments through service to their country and to their fellow man [p. 247].

Positive group interactions and high levels of morale during in-country training may also foster a false sense of competence and accomplishment in the trainees. When the new volunteer arrives in a foreign capital after three months of training in the United States and discovers that he still has language difficulties, he realizes that the training process, at best, cannot provide him with total competence for his new role. The usual response is to make an appropriate adjustment and try to do the best he can with his limited resources. On the other hand, when the in-country trainee discovers that he has problems with the local dialect of his assignment and is finally alone, dependent on his own resources, he may react with frustration, discouragement, and disillusionment regarding his training. Thus, attrition would tend to increase with in-country training. If training stress has been minimized, as Harris indicated, then such a group effect would be accentuated when the trainee finally has to depend on his own resources. In any event, we are certainly in agreement with Harris's comments on the possible negative effects of making training more comfortable. The stressful stateside training for selection purposes, combined with in-country training for job competency which

was supported by Natani in 1966, appears to offer the best alternative.

Performance and Adaptation in the Field

Peace Corps service represents only one instance of a situation in which individuals are separated from their accustomed surroundings. Other examples include new students in college dormitories, missionaries assigned to remote regions, and recruits in military service. The "input" from these new environments and the effects of the social processes operating under such circumstances, however, are often too varied to conduct research studies of the separate factors affecting the individual. For this reason, in our work, we have focused on the Antarctic station as a natural field laboratory for the study of human adaptation to geographic isolation and psychological as well as physical stresses. The complexity of the interaction between these variables is greatly diminished at the isolated station, hence their boundaries may be better defined for research purposes. These studies have dealt with both physiological and psychological variables (Blackburn, Shurley, & Natani, in press; Natani, Shurley, & Joern, in press; Natani, Shurley, Pierce, & Brooks, 1970; Shurley, 1971; and Shurley, Pierce, Natani, & Brooks, 1970).

Investigation of human performance in, and adaptation to, remote and exotic environments is needed, and this position appears to be well-defended by Harris. Nevertheless, if we are to succeed in answering the question posed by him, "what type of individual should be placed in a particular environment . . . [p. 234]?" we need to know more about the influence of our daily transactions in human society at home. Negative reactions to an environment are relatively easy to identify, but the presence or absence of positive responses often depends on the final decision of success or failure for evaluation. The understanding of psychological well-being is another neglected area of research. If we knew how an individual achieved positive satisfactions from his accustomed environment, we would be in a better position to predict his response to the presence or absence of these opportunities for obtaining a feeling of well-being in another environment. This appears to be essentially virgin territory, but Bradburn and Caplovitz (1965) and Bradburn (1969) have initiated an attempt to determine the effects of differing degrees of environmental stress upon what has variously been called mental health, subjective adjustment, happiness, or psychological well-being. We have also attempted to use a version of their technique, in a pilot study, to assess the positive aspects of Antarctic wintering reported previously by Mullin (1960). Insufficient data are available at this time to warrant further comment, however.

The Psychologist's Role

A volunteer assigned to a remote location in an underdeveloped country may be potentially in a position for exposure to more disease, accidental injury, and psychological trauma than any other representative of his country, including military personnel, if we discount combat situations. It is not surprising that psychological casualties occur. Therefore, it would appear that the primary role of a psychologist or psychiatrist in a volunteer organization should be a responsibility for the mental health of the volunteers. Harris has focused on the role of the psychologist in selection and his recent elimination from the assessment process, but he leaves the reader in suspense regarding the fate of the Peace Corps psychologist's presence for the prophylactic and therapeutic service. At the very least it seems that the reduction of the psychologist's intimate association with volunteers during training would make his performance as a clinician more difficult when called upon to attend to problems arising in the field. English and Coleman (1966) described four major periods of psychological difficulty during overseas service, that is, (*a*) the crisis of arrival, (*b*) the crisis of engagement, (*c*) the crisis of acceptance, and (*d*) the crisis of reentry. Similar phases of adjustment may be seen in Antarctic volunteers. These crises of adjustment may not be severe, but some individuals may need support during these phases, and especially during the reentry crisis. If recent administrative policies have weakened or curtailed the mental health support system established in the past to deal with the problems of the returning volunteer, then there is justified concern within the profession of psychology (and elsewhere) for the possible negative consequences of this policy.

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KIRMACH NATANI
University of Oklahoma Health Sciences Center and Veterans Administration Hospital, Oklahoma City

Attitudes of Psychology Students Toward Drug Abuse

Stachnik (1972) proposed to do away with criminal penalties for illicit drug use and to offer heroin at methadone clinics for recalcitrant drug users. By contrast, Pratt (1972) summarized a report urging indeterminate periods of civil commitment in compulsory work camps for heroin abusers, with methadone and psychotherapy followed by parole. In a survey of 174 Veterans Administration trainees' attitudes toward drug abuse, an antipathy for use of the prison system was evident. Preferred treatment modalities for opiate abusers were: therapeutic community (63.2%), synthetic narcotic as methadone (62.1%), group psychotherapy (58.0%), and halfway house (56.3%). Court probation, jail term, and tranquilizing medication each received support from less than 5% of the group. When the respondent was asked to check the circumstances under which the prison system was to be preferred to the hospital system, one third of the group selected under *no*

circumstances. A larger number (55.2%) selected the prison system for use plus criminal activity, but the category use plus "pushing" (27.6%) received less support than under no circumstances. Even those unmotivated for treatment were rarely considered candidates for prison. Homogeneous hospital patient groups were preferred, and special hospitals were frequently endorsed for alcohol and heroin abusers. The high regard for freedom of movement was also reflected in endorsements of outpatient treatment and open ward care.

Trainees felt the use of methadone was ethical (80.5%), and the treatment was successful if a hard narcotic user switched to a milder drug such as marijuana (77.6%). About half of the group felt a drug abuser would tell of his drug use, but more than half felt drug abusers would not acknowledge a personal need for treatment (63.8%) or seek recommended treatment (76.4%). Acknowledging the importance of high motivation for hospital care to achieve treatment success (94.3%), prognosis was considered poor (77.0%); users were considered likely to decrease abuse temporarily (87.4%), but unlikely to stop (73.6%).

Trainees' personal experience working with drug abusers was limited, usually less than a quarter of their professional time (47.1%) or none (42.5%). They had relatively limited knowledge about hospital facilities for reality testing an abuser's verbal statements about current drug use. Many of them (44.3%) were willing to undertake full-time work with drug abusers, and 23.6% were unwilling. Stated differently, 53.4% were willing to devote 50%–100% of their professional time to treating drug abusers, and 43.1% were willing to devote quarter time or none. Trainees were more willing to treat drug abusers than alcoholics; only a third of them would spend half-time or more with the latter.

The discrepancy between the trainees' support of treating drug abusers in an open setting and their general pessimism about the effectiveness of treatment is apparent. Nevertheless, many of them are willing to treat drug abusers and necessarily need the educational background to do so. Coping with the problems of drug abuse should certainly be included in graduate school curricula and practicum experience. Opportunities for such training may develop rapidly within law enforcement agencies (Reiser, 1973). The prejudice exhibited toward the legal system may be unrealistic in the *Zeitgeist* that produces much stricter drug penalties in New York State (Newsweek, 1973). The penchant for personal freedom may offer a method of conceptualizing the negative aspects of heroin addiction in preventive education for young target groups. Heroin addiction may legitimately be viewed as limiting choices and personal freedom.

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Wilma J. Knox
Veterans Administration Center
Biloxi, Mississippi

Time for Cerberus to Give His Name

It is the nearly universal practice among editors of scientific journals to transmit submitted manuscripts to one or more referees for evaluation. Some journals attempt to keep the authors anonymous, with respect to the referees, although this practice is often not feasible. In nearly all cases, however, the tradition is observed that the *referees* remain anonymous, both to the authors whose work may be rejected (sometimes mistakenly or carelessly) and to the readers of the articles that have been accepted (also sometimes inappropriately).

This tradition of anonymity seems out of keeping with the present spirit of accountability and openness in other areas of society. If I accept the responsibility to recommend (and thus usually to determine) that a paper ought to be rejected, if I compose a list of criticisms, often embellished with derogatory adjectives, should not the author who has invested deeply of his ego and efforts in the manuscript be at least entitled to know who I am? More importantly, if I know that I shall have to sign my critique, will I not therefore be inclined to do a better job of refereeing? Similarly, if I understand that my name will be printed in a footnote to each article whose publication I have recommended, am I not then less likely to approve a complex paper without really understanding it or a trivial paper whose rejection, while appropriate, might be taxing to explain?

Refereeing manuscripts is an onerous and thankless task; one hesitates to make it any more demanding. But, as an experiment, for the past year I have been signing my own critical reviews; it really isn't that difficult. I believe that it does make me try to be surer of the grounds on which I criticize, and I know that I'm less liberal with gratuitous insults than I tended to be when I was still cloaked in lordly anonymity. Moreover, I do not believe that one necessarily becomes less critical in a signed review or more likely to recommend acceptance of a borderline product. And I am certain that one would not recommend acceptance lightly if one's name were to be printed with the paper in question.

The referee has a responsibility both to the authors and to the readers of scientific papers; he should be *accountable* to both constituencies also. It is time, I submit, for Cerberus to start giving his name.

David T. Lykken
Department of Psychiatry
University of Minnesota

Announcements

Changes in Experimental and Comparative-Physiological Journals Scheduled for 1975

The APA Publications and Communications Board has announced changes in the *Journal of Experimental Psychology* (*JEP*) and the *Journal of Comparative and Physiological Psychology* (*JCPP*) to take place in 1975. The changes reflect the outcomes of surveys of member subscribers to those journals conducted in May 1973. The major effect is in *JEP*, which will be published in four sections. Some material previously appearing in *JCPP* will now be published in the reformatted *JEP*.

The new policies have the support of outgoing editors David A. Grant, of *JEP*, and Eliot Stellar, of *JCPP*, as well as *JCPP* editor-elect, who will be Garth J. Thomas, University of Rochester, and the four editors-elect of the independently distributed sections of *JEP*, who will be Gregory A. Kimble (*General* section), University of Colorado; Lyle E. Bourne (*Learning and Memory*), University of Colorado; Michael I. Posner (*Perception and Performance*), University of Oregon (currently Visiting Professor at Yale University); and Allan R. Wagner (*Animal Behavior Processes*), Yale University.

These changes are of immediate importance to contributors to *JCPP* and *JEP* because the publication lag in both journals now approximates 12 months. Manuscripts should be submitted to the new editors after January 1, 1974 (for *JCPP*) or after March 1, 1974 (for sections of *JEP*) (see addresses below).

The P&C Board's action on *JCPP* removes animal experimental studies of perception, learning, motivation, and performance in which the central thrust is toward general behavior theory. These reports will be accommodated in *JEP* in either the *General* section or the section on *Animal Behavior Processes*. The *JCPP* will continue to publish research reports chiefly directed toward elucidation of physiological, anatomical, or chemical mechanisms of behavior or toward elucidation of biological (including genetic), species-specific mechanisms of behavior. Refinement of these guidelines will be monitored continuously by the editors of *JCPP* and *JEP*.

The Board's action on *JEP* in 1975 is to publish it in four independently edited and distributed sections to which members may subscribe singly or in self-selected combinations. The *Journal of Experimental Psychology: General* (Gregory A. Kimble, Editor) will publish articles in any area of experimental psychology when they are judged to present an integrative report of new research leading to a substantial advance in knowledge. It will include, but not be limited to, articles such as have appeared as *JEP Monographs* and as chapters in contemporary books of "advances" in which authors present the theoretical-empirical implications of their own research programs in a comprehensive fashion intelligible to the general experimental psychologist.

The *JEP: Learning and Memory* (Lyle E. Bourne, Editor) will publish experimental studies on fundamental learning and memory processes in human behavior. An illustrative, but not exhaustive, list of appropriate topics is human conditioning; motor learning; complex discrimination learning; sequential response and probability learning; verbal learning, transfer, and retention; encoding strategies, insofar as they are related to memory; learning to learn; concept formation and abstraction processes; problem solving; deductive reasoning.

The *JEP: Perception and Performance* (Michael I. Posner, Editor) will publish studies designed to foster understanding of information-processing operations and their relation to experience and performance. An illustrative, but not exhaustive, list of appropriate topics is sensory transduction, pattern recognition, perception, attention, motivation, thinking, decision making, stress, and motor control. Dependent variables may be response speed or accuracy, thresholds, verbal protocols, autonomic or CNS activity, or other measures. Studies of perception and performance are often closely linked to physiology and to engineering applications, and contributions with these emphases are also encouraged.

The *JEP: Animal Behavior Processes* (Allan R. Wagner, Editor) will publish experimental studies of basic mechanisms of perception, learning, motivation, and performance with infrahuman animals, in which the principal focus is on concepts and issues in general behavior theory. It will, therefore, reinstate animal experimental studies as a major component of *JEP*, rather than having them split between *JEP* and *JCPP* as minor components of each of those journals.

The above statements reflect the thinking of the APA Publications and Communications Board, but more particularly the thinking of the newly appointed editors of *JEP* and *JCPP*. Further refinements and expansions of these statements will appear in early 1974 issues of the journals. In the case of *JEP*, the four editors of the sections will also serve as members of a Board of Editors of *JEP* and will collaborate continuously in establishing standards of acceptance, allocation of articles to sections, and page allocations. The page allocation to *JEP* in 1975 has been established by the Publications and Communications Board at 2,400 pages, with tentative allocations to sections of 400 for *General*, 800 for *Learning and Memory*, 400 for *Per-*

ception and Performance, 400 to *Animal Behavior Processes*, and 400 in a "page bank" from which pages will be released to sections, if needed, on the basis of consultation between the Board of Editors of *JEP* and the Chair, Publications and Communications Board. In the case of the *JCPP*, the page allocation for 1975 is 1,200, with an additional 300 pages in a "page bank" from which pages will be released by joint agreement between the Editor, *JCPP*, and the Chair, Publications and Communications Board, in the interests of reduction in publication lag. It is the expectation of the Publications and Communications Board that the Editors of the *JCPP* and the sections of *JEP* will be sufficiently selective to maintain a publication lag of not greater than 12 months, while remaining within their overall page allocations.

The addresses of the editors-elect of the two journals are: *JCPP:* Garth J. Thomas, Center for Brain Research, University of Rochester Medical Center, Rochester, New York 14642; *JEP: General:* Gregory A. Kimble, Department of Psychology, University of Colorado, Boulder, Colorado 80302; *JEP: Learning and Memory:* Lyle E. Bourne, same address as Kimble; *JEP: Perception and Performance:* Michael I. Posner, until June, Department of Psychology, Yale University, 333 Cedar Street, New Haven, Connecticut 06520; after June, Department of Psychology, University of Oregon, Eugene, Oregon 97403; *JEP: Animal Behavior Processes:* Allan R. Wagner, Department of Psychology, Yale University, 333 Cedar Street, New Haven, Connecticut 06520.

Deaths

Wesley P. Booman, date unknown
Augusta S. Clay, November 5, 1973
Shirley Elbert, July 7, 1973
Haim G. Ginott, November 4, 1973
Warren C. Gott, date unknown
C. Larry Hagen, September 26, 1973
Elizabeth F. Moller, date unknown
Llewelyn A. Owen, February 28, 1973
Emile Painton, December 5, 1972
Harry Sherman, October 30, 1973
S. Gordon Simpson, July 29, 1973
John F. Souza, date unknown
Barbara S. Stewart, date unknown
Elbert C. Thoroman, August 12, 1973
Donald T. Tomblen, June 1, 1973
George C. Turner, November 3, 1973
Warren P. Van Pelt, March 27, 1973
Marion L. Williams, January 30, 1973
Jesse Zismor, October 29, 1973
Theodore E. Zurett, November 29, 1972

Predoctoral, Doctoral, and Postdoctoral Programs

Adelphi University Postdoctoral Program in Psychotherapy: Now accepting applications for admission for fall semester 1974. The Program in the Institute of Advanced Psychological Studies is a four-year curriculum in psychoanalysis and psychotherapy encompassing personal therapy, supervision, academic courses, seminars, and research. The Program is also accepting applications for its two-year specialty program in group psychotherapy for fall semester September 1974. A PhD in psychology and two years of postdoctoral psychotherapy training are requisites for admission. For further information and applications for either program write to Donald S. Milman, Codirector, Postdoctoral Program in Psychotherapy, Institute of Advanced Psychological Studies, Adelphi University, Garden City, New York 11530. When requesting information, specify which program.

Columbia University Graduate School of Arts and Sciences: Now accepting applications in cooperation with the School of Public Health of the Faculty of Medicine for a PhD program in epidemiology for the fall semester 1974. Students enrolled in the program will have an opportunity to address major health problems of society from an interdisciplinary research perspective. The program permits specialization according to the student's background and interests in biomedical, social, or statistical epidemiology. Areas of study include the measurement of manifestations of disease and related social and behavioral phenomena, the design of field surveys, procedures for collecting and handling large bodies of data, and statistical analyses. Majors in the social sciences, biological sciences, or in mathematics and statistics with supporting background in either of the two other fields are considered to have good preparation for this doctoral program. Graduates with excellent backgrounds and achievements in other disciplines will also be considered. For further information write to Robert Friis, Division of Epidemiology, Columbia University School of Public Health, 600 West 168th Street, New York, New York 10032.

Los Angeles County–University of Southern California Medical Center: Now accepting applications for postdoctoral fellowships in child–adolescent clinical psychology. The program includes training and supervision in crisis intervention therapy (inpatient and outpatient), behavior modification, and community mental health with children, adolescents, and parents. Other special interest areas available are pediatric psychology, delinquency, suicide prevention, drug abuse, abortion counseling, free clinics, etc. Prerequisites: a doctorate (PhD or EdD) from an accredited graduate program in psychology and one-year internship or equivalent. Stipend $8,200 for 12 months, period beginning July 1974; *applications must be completed by May 15, 1974.* For more information write to Ruth Sinay, Director, Child–Adolescent Psychological Training Program, Los Angeles County–USC Medical Center, 1934 Hospital Place, Los Angeles, California 90033.

Postgraduate Center for Mental Health: Now accepting applications for comprehensive professional training in psychotherapy and psychoanal-

ysis for 1974–1975. The program offers opportunities to acquire knowledge and experience in child, adult, group, and family therapy as well as community mental health education and research. While doing therapy with a broad spectrum of patients, the candidates will be instructed by highly skilled supervisors formerly trained at the Center. Applicants must have a doctoral degree in psychology. Part-time and full-time appointments available. Stipends of $6,000 the first and $7,000 the second year available for full-time candidates. Training leads to certification in psychotherapy and psychoanalysis. For further information write to Benjamin Fielding, Dean of Fellowship Training, 124 East 28th Street, New York, New York 10016.

Community Psychiatry Section, Department of Psychiatry, University of North Carolina School of Medicine: Now accepting applications for advanced study in the leadership and administration of community mental health programs and centers. Tutorial, didactic, and practicum teaching provided by experienced faculty of psychiatrists, psychologists, epidemiologists, biostatisticians, public health administrators, social workers, psychiatric nurses, and social scientists. Emphasis: leadership and administration, analysis and evaluation of mental health programs, research and theoretical foundations of mental health programs. Completion of 12-months' training provides eligibility for mental health program administration certificate, completion of 21-months' training with major in School of Public Health provides eligibility for master's degree. NIMH postdoctoral stipends for psychologists: $6,500 ($3,600 tax exempt), including tuition and fees, travel during training and dependency allotments are provided. Additional supplement for training-related field experience (minimum $2,000). Available for fall 1974 and future years. *Applications for 1974–1975 program should be submitted before April 1, 1974.* For further information write to William G. Hollister, UNC School of Medicine, Drawer 801, Chapel Hill, North Carolina 27514.

Massachusetts College of Optometry: Now accepting applications for a new professional program: Doctor of Optometry (OD). The purpose of this program is to provide the PhD in the physiological, behavioral, physical, or applied sciences with an opportunity for accelerated study toward the OD degree. Health professions scholarships and loans are available in limited amounts. These funds may only be awarded to U.S. citizens or those foreign-born candidates who have achieved permanent resident status. For further information write to Joseph Jefferson, Division of Special Studies, Massachusetts College of Optometry, 424 Beacon Street, Boston, Massachusetts 02115.

Award

Twelfth Annual Creative Talent Awards Program 1972–1973, American Institutes for Research: Winner of First Award of $2,000, Alison Clarke-Stewart, Yale University, for dissertation entered in the Human Developmental Psychology field, entitled "Interactions between Mothers and Their Young Children: Characteristics and Consequences." Best-in-Field winners: Robert T. Hennessy, Pennsylvania State University, in the area of Psychophysiology, $1,000 award for work entitled "Stimulus and Nonstimulus Factors Affecting Accommodation with Particular Reference to Instrument Myopia"; Patricia A. Carpenter, Stanford University, in the area of Learning and Motivation, $1,000 award for work entitled "Referential Meaning in Sentence Comprehension and Memory." Seven honorable mentions: Ronald R. Rossi, University of Cincinnati; Ovid Jyh-Lang Tzeng, Pennsylvania State University, in the Sensation and Perception, Learning and Motivation, Psychophysiology, and Comparative Psychology field; Mark Snyder, Stanford University; Larry L. Venham, Ohio State University; Marsha C. Winokur, Yeshiva University, in the Human Developmental Psychology, Personality and Mental Health, and Social Processes area; Carolyn L. Cason, University of Texas; Morris Kwong-You Lai, University of California at Berkeley, in the Measurement and Evaluation, Educational Psychology, and Problem Solving, Decision Making, and Psycholinguistics category.

American Board of Professional Psychology

The American Board of Professional Psychology is pleased to announce and thank the following diplomates who served as oral examiners or field observers of 1972–1973 candidates for ABPP Diplomas in Clinical, Counseling, Industrial and Organizational, and School Psychology:

Norman Abeles
Michael Adams
Leonard W. Allen
Charlotte H. Altman
William F. Anderson
Ethelwyne Arnholter
Bernard Aronov
Marvin L. Aronson
Philip Ash
Robert J. Baldauf
Jean L. Balinky
Patricia Barger
Katherine Beardsley
Bruce Becker
Stanley E. Bell
J. G. Benedict
H. Berkowitz
Theodore H. Blau
Herbert Blaufarb
B. Bloom
Kenneth L. Bloom
Elliot R. Blum
Frank H. Boring
Donald H. Bradshaw
Lawrence Brammer
Mary Brantly
Douglas W. Bray
Roy Brener
Earl C. Brown
A. G. Burstein
James Butcher
Israel W. Charney
June Charry
David I. Cheifetz
William J. Chestnut

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R. M. Dickinson
Richard F. Doctor
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Robert M. Eichler
Forrest L. Erlandson
Norman Farberow
Hanna F. Faterson
A. Fehrenbach
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Norris H. Weinberg
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Arthur Wiens
Ronald M. Wilcox
William Wolking
Helmut Wursten
Robert I. Yufit
Irma Lee Zimmerman
Mary F. Zwikstra

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Eastern Psychological Association: April 18–20, 1974, Philadelphia; April 3–5, 1975, New York City

For information write to:

Murray Benimoff
Department of Psychology
Glassboro State College
Glassboro, New Jersey 08028

Western Psychological Association: April 25–28, 1974; San Francisco

For information write to:

Milton R. Blood
Department of Psychology
University of California
Berkeley, California 94704
or
Georgia Babladelis
Department of Psychology
California State University
Hayward, California 94542

Southeastern Psychological Association: May 1–4, 1974; Hollywood, Florida

For information write to:

Edward H. Loveland, Secretary-Treasurer
School of Psychology
Georgia Institute of Technology
Atlanta, Georgia 30332

Midwestern Psychological Association: May 2–4, 1974; Chicago

For information write to:

James H. McHose
Department of Psychology
Southern Illinois University
Carbondale, Illinois 62901

Southwestern Psychological Association: May 2–4, 1974, El Paso; April 17–19, 1975, Houston

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Rocky Mountain Psychological Association: 1974, Denver; 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque

For information write to:

Irwin Cohen
Veterans Administration Hospital
Mental Hygiene Clinic
1055 Clermont Street
Denver, Colorado 80220

Southern Society for Philosophy and Psychology: April 11–13, 1974; Tampa, Florida

For information write to:

Michel Loeb
Department of Psychology
University of Louisville
Louisville, Kentucky 40208

Psychiatric Problems of Childhood: January 31–February 2, 1974; New York City

For information write to:

D. V. Siva Sankar
Queen's Children's Hospital
Commonwealth Boulevard
Bellerose, New York 11426

Ontario Psychological Association: February 7–9, 1974; Ottawa, Ontario

For information write to:

Tim Hogan
Royal Ottawa Hospital
1145 Carling Avenue
Ottawa K1Z 7K4, Ontario

Biofeedback Research Society: February 15–20, 1974; Colorado Springs, Colorado

For information write to:

Francine K. Butler
Department of Psychiatry #202
University of Colorado Medical Center
4200 East Ninth Avenue
Denver, Colorado 80220

Southeastern Conference of the Society for Research in Child Development and Hyman Blumberg Symposium: March 6–9, 1974; Chapel Hill, North Carolina

For information write to:

Phyllis T. Elardo
Center for Early Development and Education
814 Sherman
Little Rock, Arkansas 72202

Conference on Programs and Practices for Physical Activities for Exceptional Individuals: March 8–10, 1974; Los Angeles, California

For information write to:

An Interaction Conference on Effecting Change
California State University
5151 State University Drive
Los Angeles, California 90032

Educational Technology Conference: Western, March 12–15, 1974; San Francisco; Eastern, March 24–27, 1974, Miami Beach

For information write to:

Educational Technology Publications
140 Sylvan Avenue
Englewood Cliffs, New Jersey 07632

Council of Psychoanalytic Psychotherapists: March 17, 1974; New York City

For information write to:

Mrs. Ruth Marcus
Administrative Secretary
162-05 89th Avenue
Jamaica, New York 11432

National Association of School Psychologists: March 17–21, 1974; Las Vegas, Nevada

For information write to:

Mrs. Elizabeth Day
Convention Chairperson
1111 34th Avenue
Sacramento, California 95822

American Association of Sex Educators and Counselors: The Process of Sex Counseling: March 20–23, 1974; Washington, D.C.

For information write to:

AASEC
3422 N Street, N.W.
Washington, D.C. 20007

Conference on Professional Issues in Behavior Analysis: The Evaluation of Behavior Analysis Personnel and Programs: March 28–29, 1974; Des Moines, Iowa

For information write to:

Anne Connolly
Conference Coordinator
Department of Psychology
Drake University
Des Moines, Iowa 50311

International Conventions

International Neuropsychology Society: February 7–8, 1974; Boston, Massachusetts

For information write to:

Steven G. Goldstein
Department of Psychology
John Dewey Hall
University of Vermont
Burlington, Vermont 05401

Eleventh Annual International Conference of the Association for Children with Learning Disabilities: February 27–March 2, 1974; Houston, Texas

For information write to:

Program Chair
1974 ACLD Conference
2200 Brownsville Road
Pittsburgh, Pennsylvania 15210

Sixth International Round Table for the Advancement of Counseling: April 7–11, 1974; Cambridge, England

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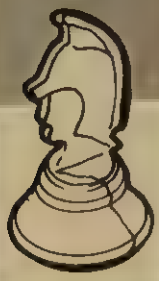

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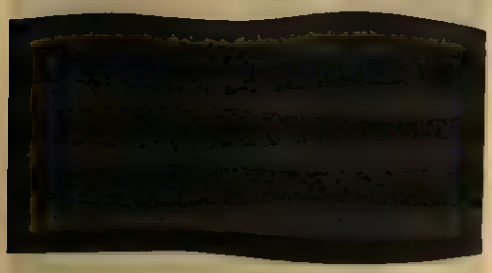

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What Psychology Is About

D. O. HEBB *McGill University*[1]

For a quarter century or more, ever since the end of World War II, psychology has been growing fast in ideas, methods, knowledge—data all over the place. To maintain perspective is difficult enough, but the difficulty is increased by our publications. It is bad not to see the wood for the trees, but worse not even to get to see a real tree because you're lost in the bushes, the undergrowth of insignificant detail and so-called replications, the trivial, the transient, the papers that haven't an idea anywhere about them. This, one must find his way through also. There is a useful maxim that I owe to my colleague Reg Bromiley: What's not worth doing is not worth doing well. The journals are full of papers that are very well done and will not be heard of again. One well-known journal almost makes them its specialty. For all these reasons or in one or another of these ways it is easy to lose sight of the fundamentals of psychology as it stands today.

It's hard to keep up, even in your own specialty. How can you hope to know what's going on—or not going on—in someone else's? But if you don't, if you haven't some general idea at least of others' work, you lose perspective. Psychology is not clinical psychology; it is not physiological psychology; it is not social or comparative or developmental or human experimental psychology. It is something more, comprising all those lines of approach to the central mystery. When the clinical psychologist forgets that he is a psychologist too, what does he become? If he's real good, he becomes a second-rate psychiatrist (since he lacks medical competence). When the physiological psychologist forgets it, when he slides downhill to become an expert on the red nucleus or the cingulate gyrus or the *cornu ammonis*, for its own sake and not as a key to an understanding of behavior, he takes the easy path to simpler problems. He may be a good physiologist, but he's not a psychologist any longer. Psychology is tough, and it's important.

The current flood of papers, that deluge of data, leads us to forget fundamentals. I meet graduate students in seminar each year. They come from good schools and they've had as good teaching as there is. And most of them have no clear ideas about the relation of mind to body, or about consciousness, or what thought is, or free will. I didn't say good ideas (those are the ones I agree with); I only said clear ideas. They seem not to have been led to think about such problems. In seminar they hope the problems will go away if they just keep quiet about them. A majority, I would say, have no clarity even about the heredity–environment question; and this, ladies and gentlemen, I am inclined to give *you* the discredit for, since the literature suggests that some of you don't either.

I begin to sound like a prophet in ancient Israel, misanthropic, damning everybody—like Amos, "Woe unto them at ease in Zion!"—or a certain later prophet, "Woe unto you, scribes and pharisees!" Especially the scribes.

The questions of mind, free will, thought—these are not unimportant matters, not insignificant. That book of Fred Skinner's (1971) *Beyond Freedom and Dignity* has demonstrated that they have real practical meaning. Skinner thinks the questions are important, and so do I. You no doubt realize that my answers differ from his, but I applaud his concern with fundamentals. And some of the criticism of *Beyond Freedom and Dignity* shows (like some of the criticism of Arthur Jensen) that there are questions on which some human psychologists—you know, the ones who don't work with animals—feel no need to think. They have ready-made answers.

One of those ready-made positions goes back to Thomas Jefferson, who swore "eternal hostility" to any form of "tyranny over the mind of man." Skinner proposed to make people want to be good—and caught hell for it. He was told it is wicked to

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[1] This article was the APA Invited Address presented at the annual meeting of the American Psychological Association, Montreal, Canada, August 29, 1973.

The critical advice of Dalbir Bindra and Virginia Douglas is gratefully acknowledged.

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"manipulate" the minds of others, and wicked to do anything to interfere with full freedom of choice. This is evil; this is something that no good humanistic, democratic, libertarian would ever do? Such statements are pure unthinking nonsense.

For what is a moral education? The very psychologists and philosophers who talk most about freedom are the ones who tolerate no nonsense from their children or their students, in moral and political questions. A liberal, democratic, moral education sets out, rightly, to *remove* freedom of choice from a child's mind in moral questions. The tyranny Jefferson objected to was imposing ideas *he* didn't agree with (and *we* don't agree with today, either). Imposing ideas we agree with is OK, and necessary too. Education is in a bad way if a boy on reaching maturity has to sit down and argue out the question before deciding whether race prejudice is a good thing, or cruelty to animals, or fascist governments, or "Watergating"—or if a girl leaving home has still to figure out whether a career in shoplifting or prostitution would be a good idea. Impose ideas? Try to limit freedom of choice? Of course we do, all of us. Skinner's critics will say at once that this isn't at all what they were talking about. Maybe so, but it's clear they didn't stop to think before giving out the word, and I still don't see what's wrong about making people *want* to be good—if we could only make it work!

* * * * *

I am gradually getting around to an answer to the question, What is psychology about? by first telling you some things it is not or should not do. I have said that psychology is not one of the many narrow specialties that, between us, we cultivate. It is more than any one of them. It is important for the welfare of psychology to keep reminding oneself about fundamentals and to *think* about some of those issues. Now, what psychology *is*.

Psychology is a biological science. Before you clinical and social and vocational guidance types get up and leave, hear me a little further. I think anyone will agree—including the monkey trainer, the rat-brain plumber, and those who write down baby talk—that the urgent psychological problems are *social* and *clinical*. These are the big ones on which more light is needed to promote human welfare. These are the problems of prejudice and social conflict—at the worst, war—and the problems of mental disorder, neurosis and psychosis. Knowing more about the rat and pigeon is—I personally am sure—a step toward understanding man; maybe for practical purposes it's a necessary step; but it's peripheral; it's a means, not an end. Memory, however studied, is an essential component of behavior—but one component only. There is a larger picture to take account of, and its social and clinical implications are vital.

So how is social psychology part of a biological science? I am not talking about the social insects, or schools of fish, or dominance in baboons—though these studies are enlightening too—but about man's social behavior looked at in the light of evolution. Man is a social *animal*. His society is complex, the experience and learning of the child growing up in it are complex, and so it is hard to trace the origins of adult human social behavior. With animals, there's a better chance. Also, we are so habituated to human behavior that it is hard even to see some of its outstanding characteristics. Which of us, for example, if asked to list the predictable features of man's behavior would include sleep—an instinctive pattern if there ever was one? A comparative perspective might not correct that particular blind spot, but it does help with others.

Some students are still being taught that man by his nature is selfish; however, 35 years ago Crawford and Nissen's chimps showed that that is false. Students are still being taught, or allowed to think, that man dislikes work; Harlow's monkeys showed the falsity of that idea 25 years ago. Students are still being taught that hostility has to be acquired, from bad teaching or from job competition. From that comes the dangerous doctrine that if there is no competition and no bad teaching, there will be no prejudice. That disastrous misconception could not survive if you observed how a group of "higher" animals—higher in evolutionary status, not in rationality!—treats a stranger in their midst. A dog doesn't have to be taught to growl at strange dogs; the monkey doesn't need to learn to drive off the newcomer, who sins only by being unknown. And this irrational emotionality increases with evolution, instead of decreasing. *Man is a social animal;* seen in that light, both virtues and faults make more sense.

There are other clarifications from work with nonhuman animals that tend to be forgotten in the human social situation. One example concerns the problem of language and language learning. The psycholinguist might have been farther ahead if he had recognized that language is a species-predictable form of behavior, that is to say, instinctive behavior, and recognized also that instinctive behavior in birds and mammals isn't merely a matter of

heredity. Learning is part of instinct too. The whole problem would have been clarified by reading a 20-year-old paper, "The Descent of Instinct," by Frank Beach (1955). Those who know no history are condemned to repeat it.

The social psychologist must recognize that there is a definite, definable sense in which he is a biological rather than a social scientist. He doesn't really belong in the same grouping with the economist, the anthropologist, and the political scientist —not even with the sociologist—if only because of that preoccupation with learning that all psychologists share in one way or another. No psychologist of course *agrees* with any other psychologist, but they all have strong views about learning, reinforcement, and John B. Watson.

More fundamental, perhaps, is the fact that the social psychologist approaches social phenomena from the point of view of the participating individual, instead of regarding the group as an entity. As evidence, consider the traditionally close relation between *social* and *personality* as fields of teaching and research; personality is by its essence the study of the individual. Add to all of this that social psychology has become experimental—not really true of any other social science—and you can see why it is not unreasonable to regard it as a biological science as well as a social one.

And clinical psychology too, which has always had an important connection with the biological field. I may remind you that the definitive demonstration of a breakdown due only to experiential pressure, on an otherwise normal organism, is the experimental neurosis that was first observed by Shenger-Krestovnikova, in Pavlov's laboratory. Perhaps you thought, when I said that clinical psychology is a biological science, that I was going to say that mental disorder is all due to heredity—or brain tumors. I am really not as simpleminded as that. It's true that psychosis sometimes originates in some sort of lesion in the brain, but only sometimes. And it's never heredity by itself—or experience by itself, either. Always, always, it's an interaction of heredity with environmental agents, or—a slightly different proposition—constitution interacting with experience. Even that classical Pavlovian demonstration, the experimental neurosis, was induced by experience but was a function of heredity, too. Only some dogs broke down as a result of the perceptual conflict. The biological view doesn't mean forgetting Freud—after all, Freud and animal-rearing experiments are in full agreement about the lasting effects of the early environment. And behavior therapy: It takes different forms today, but all goes back to Skinner, the rat, and the pigeon. And behavior therapy today is radically affecting clinical procedures.

The biological view is a corrective against going to extremes. No one in touch with animal work today is likely to overlook the role of experience in mental disorder; even when a constitutional disturbance is the principal cause, experience still plays a part and must be dealt with: Supply the missing adrenal cortical hormones, and you may still have to use psychotherapy to get the patient back on an even keel. And the biological view will remind you that there are constitutional factors involved, to a major degree, in many cases of neurosis and psychosis. How anyone can say that there is no mental illness, that neurosis is all a social fiction, I can't see. Anyone who observes the suffering that can go with an endogenous depression must know that is false. Give the devil his due: There are medical elements in many cases, and chemotherapy is frequently essential. The psychological elements are there, too, and we may have to fight to have them recognized, but you can spoil a good case by overstating it.

Anyway, what all this says is that seeing psychology as a biological science need not prejudice a soundly psychological point of view; and it does help, sometimes, to keep one from talking nonsense.

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There are many people who are unhappy about the course of modern psychology, and, I regret to say, this includes some psychologists. The objectors do not want an objective science, but a sort of self-contemplation. Not the hard-shell introspection of Titchener, Külpe, and Wundt, but something sloppier. They tend to be dualists at heart, and they dislike what they think of as the materialism of experimental psychology; and they consider that psychology's true business is not with cats or monkeys, not brain lesions, not the use of tachistoscope or the analysis of variance. The more profound human experiences are what we should be working on. They want us to deal directly with the mystery of existence *now*. Some of this is simply antiscience—antiscience of any kind—which we needn't bother with here. But when someone thinks a science can be run that way, there *is* much to be said.

Subjective science? There isn't such a thing. Introspectionism is a dead duck. It is theoretically impossible: See Charles S. Peirce, America's great-

est philosopher, 100 years ago; Gilbert Ryle, Oxford philosopher; Garry Boring, *Mr. Psychology;* and George Humphrey, American-trained Oxford Professor of Psychology—a pretty distinguished lot for you to disprove if you think introspection is still in business. And then, if you do disprove them (nobody has tried so far), you still have to explain how come, if that subjective approach is the true path to a knowledge of man, it has achieved so little. What can you point to that either Wundt or Titchener left behind him, what light on the problem of mind and thought and feeling? Külpe, in a way, did better, for his work pointed straight to the proposition that psychology is objective, not subjective. William James is not an argument for subjective psychology; he took introspection for granted, but search his pages and you'll find precious little introspective data. Introspecting was *not* what James did.

And Freud—you realize of course that Freud's method with the unconscious was, by definition, objective—the study of that part of mental activity that the patient *cannot* report. Piaget—objective method; Lashley on serial order and thought—objective method; Köhler on insight; Lewin on leadership; Harlow on love—all objective. What is there to cite as a contribution from subjective method that can be put beside their work?

And the same question must be asked about humanistic psychology. What is the payoff? What is its contribution to knowledge? I sympathize with the feeling that scientific psychology, as far as it has gone today, leaves much to be desired in the understanding of man and has little to tell us about how to live wisely and well. I am inclined to think that scientific psychology will always be incomplete in that sense. But the remedy is not to try to remake a science into one of the humanities. Humanistic psychology, I think, confuses two very different ways of knowing human beings and knowing how to live with self-respect. One is science; the other is literature. A science imposes limits on itself and makes its progress by attacking only those problems that it is fitted to attack by existing knowledge and methods. Psychology has made much progress in this century, and the rate of progress is accelerating, but it is limited and must be limited if it is to continue its progress—limited in the questions it can ask, but sure in its results.

The other way of knowing about human beings is the intuitive artistic insight of the poet, novelist, historian, dramatist, and biographer. This alternative to psychology is a valid and a deeply penetrating source of light on man, going directly to the heart of the matter. If you refer to literature as a source of knowledge to a scientific type, he'll laugh at you. How can a novelist or a poet—a poet for god's sake—make discoveries? How can *he* have a knowledge of man that science hasn't? Science is the up-to-date thing; the paraphernalia of experiment and controlled observation and analysis of variance are the ways to find things out. Pick and shovel are out of date, now that we have bulldozers? But you can do things with a pick and shovel that you can't with a bulldozer; a man on foot can make observations that you can't make from a limousine. I challenge anyone to cite a scientific psychological analysis of character to match Conrad's study of Lord Jim, or Boswell's study of Johnson, or Johnson's of Savage.

It is to the literary world, not to psychological science, that you go to learn how to live with people, how to make love, how not to make enemies; to find out what grief does to people, or the stoicism that is possible in the endurance of pain, or how if you're lucky you may die with dignity; to see how corrosive the effects of jealousy can be, or how power corrupts or does not corrupt. For such knowledge and such understanding of the human species, don't look in my *Textbook of Psychology* (or anyone else's), try *Lear* and *Othello* and *Hamlet*. As a supplement to William James, read *Henry* James, and Jane Austen, and Mark Twain. These people are telling us things that are not on science's program. Trying to make over science to be simultaneously scientific and humanistic (in the true sense of that word) falls between two stools. Science is the servant of humanism, not part of it. Combining the two ruins both.

* * * * *

So, then, finally, what is psychology about? And the answer I give you is one I got from K. S. Lashley: Psychology is about the mind: the central issue, the great mystery, the toughest problem of all. I grant that psychology is concerned with other matters, subsidiary questions; in fact, I have just been saying that a science must move slowly and can't hope to go right to the heart of things. There are many subsidiary questions to be clarified before we will have final answers to the central question, before we understand the operations of mind—if we ever do really understand them. Nevertheless, there are some answers possible that were not possible when Lashley first saw a set of Golgi slides of the frog brain and thought it might be possible to find

out how the frog works. We still do not know how the frog works, let alone man. In fact, the problem looks tougher now than it did to Lashley in 1910, but we have made some advances, and there are some things one can say—that a biological science should say—about the human mind.

It is hardly necessary to say that the mind, for Lashley, was not a spirit held in the body. Biological science long ago got rid of vitalism. The idea of an immaterial mind controlling the body is vitalism, no more, no less; it has no place in science. I know that many of you are dualists and *do* believe that the mind is something other than brain activity. Indeed, it is conceivable that you are right. There is no way of proving the null hypothesis, no conceivable way of proving the nonexistence of something as slippery as the soul. But I put it to you that the null hypothesis can be disproved. If you believe in the existence of a spirit that guides man, the scientific and logical procedure for you is to assume its nonexistence, with the expectation that some day it will be found that this "null hypothesis" is insufficient. That is, if you will take as a working assumption that there is no soul, you may one day show that there *is* one. This means that believers and unbelievers can avail themselves today of the same working assumption, of monism instead of dualism. Anything else, today, is not science.

Mind then is the capacity for thought, and thought is the integrative activity of the brain—that activity up in the control tower that, during waking hours, overrides reflex response and frees behavior from sense dominance. I do not propose here to refute, once more, the Watsonian notion that thought is muscular activity, mainly of the vocal organs. Walter B. Hunter, a tough, behavioristically minded scientist, showed how to refute it in 1913. The delayed-response method shows that response is not controlled by sensation alone. There are other demonstrations of the same thing, and the learning theorist who does not recognize it is simply refusing to face reality.

The fact of thought as a semi-independent factor in behavior is something that anyone working with mammals is familiar with. It is summed up in the third law. (The first law is, If anything can go wrong in the experiment, it will; the second law, Training takes time, whether or not anything is learned; and the third law, Any well-trained experimental animal, in a controlled environment and subject to controlled stimulation, will do as he damned well pleases.)

That's an old joke and you may be bored by hearing it again. But it has a significance that may not have struck you. What the third law is talking about is the fact of free will in animals—higher animals, at least—and what it says is that free will is not some fancy philosophical abstraction or something J. B. Rhine thought up, but an ordinary, familiar, biological phenomenon, a product of evolution. Free will is not a violation of scientific law; it doesn't mean indeterminism; it's not mystical. What it is, simply, is a control of behavior by the thought process. Not all behavior is so controlled, even in the higher animal, reflexive response being excluded. But most behavior of man or monkey or ape is under a joint guidance by sense input and the immediately prior pattern of cortical activity; and the cortical component in that control is free will. The idea that free will means indeterminism is simply a misunderstanding.

Let me take a minute more on this, because it's important. I am a determinist. I assume that what I am and how I think are entirely the products of my heredity and my environmental history. I have no freedom about what I *am*. But that is not what free will is about. The question is whether my behavior is entirely controlled by present circumstances. Heredity and environment shaped me, largely while I was growing up. That shaping, including how I think about things, may incline me to act in opposition to the shaping that the *present* environment would be likely to induce: And so I may decide to be polite to others, or sit down to write this article when I'd rather not, or, on the other hand, decide to goof off when I should be working. If my past has shaped me to goof off, and I do goof off despite my secretary's urging, that's free will. But it's not indeterminism.

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Free will thus has a physiological basis, in the relative autonomy of the activity of the cerebrum. Here again is evidence of the way in which physiological and biological conceptions can be clarifying, as we think about the evolution of that equipment up in the control tower—between the ears—and how it works. One would think consequently that physiological psychologists should be first among those who see man as a whole, those who keep us reminded of the main objective: an understanding of that integrative function of the cerebral cortex that makes man what he is. And are they?

Mostly, no. Mostly they are afraid of theory. Mostly they are even worse at keeping an eye on

the ball than the paired-associate learners and the analysis of variance experts. Mostly. But some exception must be made for those whose work takes them into the neurological clinic and exposes them to the real problems of real people. A couple of Montreal examples are Brenda Milner, on the devastating effects of total loss of capacity to form new memories, and Ronald Melzack on pain—pain being pretty good at bringing out man's humanity, in one way or another. But the outstanding example for my present purposes is the work of Sperry and Gazzaniga on split-brain patients. For any one of you who is concerned about the mind–body question, for anyone who proposes to philosophize about the fundamental nature of consciousness, that work is essential. Read that if you never read anything else about the brain. I speak to the dualist especially, the one who considers that consciousness can't be something produced by the brain. Sperry (1968) made a case for his conclusion that longitudinal sectioning of the human brain into left and right halves has the result that the patient has two minds: a left-hand mind and a right-hand mind, each with its own separate purposes, thoughts, and perceptions. The surgeon's knife can cut brain tissue, no problem there: Can it also cut an immaterial mind in two, make a longitudinal section of the ghost in the machine? Today, no one, psychologist, philosopher, neurologist, or humanist, is entitled to an opinion on the mind–body question if he is unfamiliar with the split-brain procedure and its results in human patients.

I don't know, in general, that the objective-biological-physiological approach is always clarifying, but I do know that we have nowhere else to look for ideas about the mechanics of thought. The ideas are speculative, certainly, but when science stops being speculative it stops being science and becomes technology. As an example we can take a long-standing puzzle concerning creative thought and see whether from an objective point of view it really need be so puzzling. What I'm talking about is the report by certain poets and mathematicians and scientists especially, that they have suddenly on occasion been given answers they didn't work out themselves. The poet finds himself listening to a poem, or part of a poem; the lightning calculator hears a voice telling him what the next step in calculation should be; the mathematician gives up on a problem and then when doing something else finds that he has the answer, sometimes not the kind of answer he expected; the scientist gets the solution to his problem in a dream. Now this must seem a disconcerting sort of thing to happen to one, and it's hardly surprising that some of those to whom it has happened have regarded it as a form of supernatural visitation. William Blake considered that some of his verse was dictated to him by his dead brother. A. E. Housman reported, of a particular poem of four stanzas, that two of the stanzas "bubbled up" into his mind; a third stanza came "with a little coaxing"; but then, he said, the fourth stanza, necessary to complete the poem, did not come: *"I had to turn to and compose it myself,* and that was a laborious business." Housman doesn't say where the first three stanzas came from, but he's quite clear that it wasn't him that did it. It's hardly surprising, when we consider this sort of thing, that some of the great religious authors considered that their words came direct from God.

Now, today, God is less likely to get the credit. When the artistic or intellectual product takes one by surprise, a different source is apt to be thought of: namely, the unconscious. For a good example of this line of thought I refer you to Jacques Hadamard (1954), who put the argument beautifully, mainly on the basis of the earlier argument by Henri Poincaré. I won't labor Poincaré's report, which you probably remember: In brief, he sweated over a problem and finally gave up—for the time being at least—and then, unexpectedly, out of the blue, found the answer in his thoughts and made one of his great mathematical discoveries.

Poincaré, and following him Hadamard, discussed this phenomenon as proof of the existence of the unconscious: not as an annex or department, but like a separate mind, richer and more powerful and more creative than the mind we are conscious of—or the one they think we are conscious of—and not at all subordinate. And this seems today to be established doctrine. A science writer says the great mathematicians and scientists are the ones who have learned "how to tap the unconscious"—just like tapping a barrel of beer at a picnic. Literary critics know all about the unconscious. There is a debate for example about Coleridge, and whether maybe too much credit is being given to his unconscious for *Christabel* and *The Rime of the Ancient Mariner*. Maybe Coleridge himself should have some of the credit.

That's getting pretty silly. But we can sympathize a little when we remember how strange some of the reported experiences must seem. Imagine for a moment that you are a theoretical physicist looking for a birthday present for your wife when you suddenly realize that *H psi sub n*

must be identical with *E sub n psi sub n*, coming out of the blue. A thing like that might shake anybody. Or you drink a bottle of beer, go for a walk, and with equal suddenness find yourself the possessor of half a poem, all ready to write down and only needing a little work from you to complete it. Or, like Blake, you hear something like "Tiger, tiger, burning bright/ In the forests of the night," and so on, spoken in your dead brother's voice. Mysterious, puzzling, even frightening.

But what is the nature of the mystery? What is it that is puzzling? Why does this sound so surprising? Similar things in fact happen to us ordinary mortals all the time, only on a smaller scale. You try to think of a way to describe some experience or other, with no success, and then the right words occur to you at the dinner table, or in the john. Trying to repair some gadget or building a model of some sort you are momentarily balked, go on to other things, and then suddenly a solution to the difficulty "occurs" to you, as we say. But this is so common it doesn't seem mysterious at all. So, in the first place, the mathematical and the poetic phenomena may be noticed only because they're so extensive. A poet, for example, isn't surprised when a single happy word or phrase occurs to him —that's what it means to be a poet—but he may be surprised when bigger chunks of verse are presented to his thought.

But the main reason is that we're all brought up to think we know our own minds and what they're up to at any moment. Thus, it's an exciting and romantic idea that each of us has within him an unknown entity, the unconscious, that makes us do things we mightn't otherwise. Freud's unconscious was mostly a doer of dirty deeds, but it's even more romantic now to think of it as something with great intellectual and aesthetic powers. None of this need be rejected. I've already referred you to the eminent psychologists and philosophers who have, each in his own way, concluded that there is no *conscious* mind in Herbart's sense and in Freud's. What goes on we know something about, but only incompletely and by inference, not by direct inspection.

From that point of view, the sudden occurrence of a new idea or new cognitive program is intelligible enough, at least in principle and theoretically. (The details will take a little longer.) I assume that a concept, an idea in the ordinary sense of the word, has the same sort of structure as a percept. We know that a percept of a table, or a car, or a book, or any such familiar object, is perceived and imaged as a series of part-perceptions, with separate visual fixations. If a new creative idea is also a sequentially organized complex, certain things become more intelligible. Consider the diagram in Figure 1, in which each letter represents a possible component of the new idea, the components occurring independently and more or less at random. For the problem solver, these are data and ideas originating in the "preparatory period" emphasized by every writer on this topic; for the poet, they are the result of that intoxication with words, and verbal imagery, that poets are known for. These separate ideational elements fire and subside, fire and subside, until the crucial combination occurs. The combination is represented in the diagram by K–L–M, firing in that order. This is the new scientific idea or the happy poetic phrase.

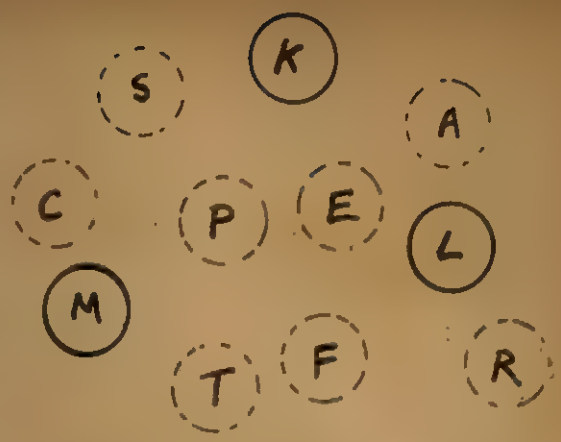

Figure 1. Diagram of a new creative idea.

This model of creativity obviously explains the chance element in discovery, thoroughly documented in the history of science. But it also explains something else: why forgetting a problem might be a positive help—why "to invent, you must think aside." The sequences of ideational components cannot be really random, because of old habits of thought; and if L is strongly associated with, for example, R, the vital sequence K–L–M, without R, is not likely to occur. Thus, we have Claude Bernard's dictum that "those who have an excessive faith in their own ideas are not well fitted to make discoveries," and thus we can understand why, on occasion, a deliberate setting to one side, a deliberate cessation of effort to solve a problem, may lead to the solution unexpectedly. (This, of course, would not be necessary if you knew in advance which are the right ideational elements, but,

of course, you never do, in the truly creative act—it's full of surprises.)

The question that one is apt to ask next is, How does the thinker recognize the creative combination when it occurs? But this is not the right way to ask the question. The real question is, What is its special property? What makes K–L–M in the diagram hold together, instead of just disappearing as the other adventitious combinations do? And there is no answer available. We must suppose, from the subject's behavior at this point, that the combination is one that has some special excitatory value for what is called "the prepared mind"—but not for others. What the physiological basis is, is not apparent. In some way the new combination, the creative idea, is one that can take control of the stream of thought and of behavior.

When we ask what the *psychological* characteristics are of the idea that can grab thought in this way, an interesting point emerges. We usually think of the creative solution to a problem as one that affects practical affairs: The thinker can build a new kind of bridge, or make better bread, and so on. But, in fact, the idea that results often has no such practical significance, but is held as a result of its effect on the thinker's further thought. I have no intention at this stage of trying to trace out the ramifications of this effect, but I wish to draw your attention to one particular aspect that hasn't had enough attention in psychology. This is the attraction of the mysterious, the unsolved problem, the far-out idea that seems to contradict existing knowledge. Such ideas are not at all foreign to science and mathematics. As Barber (1961) has shown, the great new ideas of science at first looked preposterous to professionals as well as to laymen, but then when their organizing and clarifying effect was seen, they became commonplace.

But we can take this further and see an aspect of human motivation that has not been adequately dealt with. The far-out idea can be attractive in and of itself, apart from any clarifying effect. The fact of religion, the universality of strongly held dogmatic belief independent of any need of evidence to support it, is a fundamental fact, species predictable. It is the "will to believe" of James, and it seems that belief becomes stronger, not weaker, when it meets a contradiction. Contradictions, and the preposterous, seem to have an inherent attraction. The relation to Festinger's "cognitive dissonance" seems obvious.

There are parallel phenomena in behavioral science. I venture to say that old-time psychoanalysis, like Christianity, had its startling success *because* its central doctrines were so preposterous. The parallel with religion is reinforced by the frequency of schisms and sects and vicious disagreements about hairsplitting points of doctrine. A less dramatic example, but one I think in the same pattern, is the black box learning theory of 1945 with its denial of an intracranial determinant of behavior, that is, a denial of thought, along with persistent refusal even to discuss opposed evidence such as Hunter's delayed response, or the phenomenon of mental set. And—still current and very much up-to-date—there is the example of parapsychology.

I do not say that life after death is inconceivable, and I do not say that telepathy is inconceivable; I do say that the supporting evidence falls far short of what would be needed to establish such obviously important propositions. A case can be made that it is the romantic, mysterious nature of extrasensory perception that attracts. Telepathy at short range is conceivable, barely, on the ground that the living brain is always broadcasting, though there is absolutely no suggestion of how it might have its effect on a second brain. But this becomes less conceivable when we are told that distance makes no difference to the transmission. And clairvoyance: Where does the energy come from in that case? Or in psychokinesis? I could agree that these doctrines had a rational origin, an evidential basis, if I saw the parapsychologist worry about the physiological implications, or about the problem of the conservation of energy. He is saying that there are big holes in physiology and physics. Set aside precognition, the very idea of which contains a contradiction, and one must still have doubts, to say the least, about parapsychology, as long as it maintains that bland disregard for the physical implications. Science, including biological science, is *one* body of thought.

I may have missed someone in these unprovoked aggressions, in this catalog of gripes, but it's time for me to wind it up. I have argued that psychology is a biological science, including its social and clinical wings; that a science is self-limiting, holding more or less strictly to its own narrow modes of procedure; and consequently that mixing psychology up with other ways of knowing human beings—the literary and artistic way—is to the detriment of both. We must honor the humanities, but a science cannot imitate them. I have answered the implied question, What is psychology about? by saying that its central concern must be man's mind

and thought. Each of us has his own avenue of approach to that understanding, which we must approach by degrees. It is a far prospect, and in the meantime we have to keep on with the study of memory, perception, psycholinguistics, fear, and so on and so forth; but it may be disastrous in the long run for psychology when the specialist digging his own path deeper and deeper loses sight of what others are doing in other fields and so loses an invaluable perspective.

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Supplement to Listing of APA-Approved Doctoral and Internship Training Programs

The Committee on Accreditation announces the following changes in the list of APA-Approved Doctoral Programs in Professional (Clinical, Counseling, and School) Psychology and in the list of APA-Approved Internship Programs in Professional Psychology. These changes update the listings published in the September 1973 *American Psychologist* (pp. 844–848). Complete lists are available on request from the APA Educational Affairs Office.

FULLY APPROVED DOCTORAL TRAINING PROGRAMS

University of Illinois (Clinical PsyD)

Rutgers—The State University (School)

The following fully approved internship training program voluntarily terminated accreditation:

Louisiana State University Medical Center

Some Perspectives for Industrial Psychology

EDWIN E. GHISELLI *University of California, Berkeley*[1]

We industrial psychologists are a curiously introspective lot about our professional selves. For some reason or another we are greatly concerned about what we are doing, how we are doing it, and what we ought to be doing. In the very early days of industrial psychology, our folk heroes, people such as Viteles, Link, Bingham, Paterson, and Burtt, all had their say about the role of industrial psychology and what it should be concerned with. Those of us who formed the next generation continued to insist on telling each other at great length who we are, what matters we ought to consider, and how we should do what we do; and the current generation has continued this custom of a periodic auditing of our field.

I propose now to continue this custom, and I shall discuss a rather mixed bag of matters that I believe we as industrial psychologists ought to think about. I shall consider some notions about the nature of the variables we use, the study of organizations, the role and nature of theory, the impermanence of facts, and individual differences and individuality.

The Nature of Variables

As industrial psychologists we are concerned with various aspects of workers' behavior, together with the many factors that are related to, or determine, it. We examine the sundry variables that are manifestations of workers' mental life or that bear on it, ascertaining their interrelationships and the effects of one upon another. Let us think a bit about these variables.

We become so involved with the psychological variables that we propose as pertaining to occupational behavior, that we come to think and act as though those variables have a certain truth—a reality of the same order as that of physical variables. We fail to keep in mind that psychological variables are intellectual constructs, mere conventions. Generally, to be sure, they are useful conventions, for they provide meaningful descriptions of human behavior. Nevertheless, we must recognize the fact that the psychological traits and properties we formalize have a will-o'-the-wisp nature; perhaps they are there and perhaps they are not.

Each of us tends to see the greater importance and relevance of certain psychological variables over others, and we go to great lengths to persuade our colleagues of their significance. This is not to say that the variables each of us finds to be of compelling interest are entirely unique, for this is not the case by any means. There is a great similarity in our training, and we are all the audience of the same professional and scientific conferences and publications. Consequently, it is not surprising to find that there is some sameness among us in the particular variables we perceive as pertaining to people.

What I am saying is that when we set about examining workers' behavior, the particular variables we distinguish as being relevant emerge from our individual notions about the psychological nature of man. These notions reflect our various individual frames of reference, and since we have very similar backgrounds and operate in quite similar intellectual environments, our various frames of reference are quite similar. The extensive commerce we have with each other in the exchange of information and ideas has almost rigidly institutionalized the sorts of variables that we permit each other to use.

We psychologists are not the only people who speculate about the essence of human nature, nor are we the only ones who have insight into it. The ordinary man constantly observes the behavior of his fellows and ponders about the factors that determine it. As a result of his experiences with his fellow workers, together with the pains and joys he himself has experienced in connection with his job,

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[1] This article was a Distinguished Scientific Contribution Award address presented at the annual meeting of the American Psychological Association, Montreal, Canada, August 1973.

Requests for reprints should be sent to Edwin E. Ghiselli, Department of Psychology, University of California, Berkeley, California 94720.

he develops some quite shrewd notions. The proverbial philosophizing of the taxi driver is not entirely trivial, nor are the chattering comments of the coffee shop waitress absolutely witless.

The working Jill and Joe fend for their existence in the harsh realities of the world. The fact that they survive and pursue useful careers in such a demanding social environment is testimony to the fact that they possess considerable insight into the varieties of behavior human beings manifest and into the sorts of factors that determine this behavior. The layman's ideas about man's mental machinery are by no means without substance, and by ignoring them it is quite possible that we are missing a truly rich lode.

In formal interviews and informal conversations with people about work and matters pertaining to it, my attention has been drawn to a number of variables that describe significant aspects of the behavior of workers, that I had just not thought of before, and that I found both interesting and useful. As an example, let me cite a property which might be called the *hobo syndrome*. This syndrome can be defined as the periodic itch to move from a job in one place to some other job in some other place. I have seen this syndrome in all manner of people, from those engaged in occupations that require little by way of training or skill, to those in substantial managerial positions. This urge to move seems not to result from organized or logical thought, but rather would appear more akin to raw, surging, internal impulses, perhaps not unlike those that cause birds to migrate. Floaters readily provide socially acceptable explanations for their peripatetic activity, but under careful examination these explanations turn out to be little more than rationalizations. The simple fact is that after being in one place for a matter of months, or perhaps a year or so, depending on the strength and periodicity of his itch, the individual is impelled to pack up and move to another place and another job.

Folk songs often are authentic expressions of a people's concerns, characteristics, and motives, and the variable I am trying to particularize for you is often described in them. In the ballad of the hobo, the lay of the wandering cowboy, and the song of the itinerant worker, country and western music fully attests to the genuineness of this recurrent itch to move on. Very likely, an analysis of the content of work songs would give industrial psychologists other new and interesting insights into the behavior of workers. In any event, many of the baffling cases I have come across in interviews that I have had with workers about why they left their jobs are resolved when I consider the possibility of this hobo syndrome as a factor that determines behavior.

Organizations

I now turn to my second topic, the investigation of organizations. Today we call our field industrial and organizational psychology in order to acknowledge that we are concerned with men and women in a particular kind of social setting, the business or industrial organization. We have established new journals and have produced new and different sorts of books as outlets for our surging research and thinking about organizations. Yet I believe we have just begun to recognize a few of the many facets of the matter and to empirically explore them. To be sure, we are showing more and more breadth and sophistication in the ways in which we view our problems by considering the social setting wherein Jill and Joe labor, but I do not believe we go far enough. It seems to me that we tend to think of organizations simply as environments. Thus, we examine such matters as the effects of differences in the climates of organizations on the types of leadership that are most effective in them and the relationships between various sorts of organizational structures and the need satisfactions of the members of those organizations.

Even dealing with organizations just at this level, taking them solely as social environments, we need more innovation. For example, we ought to be giving much more attention to the sorts of sociopsychological dimensions along which organizations vary. There have been some beginnings, for some of the braver and younger among us have directed their thoughts to the kinds of dimensions that can be used to describe organizations. Nevertheless, we are still at a relatively primitive stage, largely concerning ourselves with variables that simply are descriptive of objective properties such as size and sociological variables such as shape. We need much more creative thinking and much more argument and controversy among ourselves about the pertinent sociopsychological dimensions of organizations.

As we well know, there have been numerous studies of the effects of the organization on the individual, showing various ways in which it modifies his behavior and his thinking, but we have given little or no attention to the effects the individual has on the organization. We should be ex-

ploring such important problems as the kinds of people who can make significant changes in the nature and operation of the organization and the different sorts of changes made by persons of different abilities and personality traits.

Furthermore, I believe that it is legitimate to examine the psychological nature of organizations, taking organizations as wholes. Traditionally, the study of organizations has been the province of the sociologist. Nevertheless, I believe that we, too, can consider business and industrial organizations as individuals, individuals who vary among themselves in a number of different psychological dimensions and who behave in ways that are characteristic of all kinds of individuals.

For example, we can examine how organizations learn. When a factory is charged with turning out some new product, the initial rate of production is low, and apparently as experience is gained, production increases but with diminishing returns, so that a graph of production looks exactly like the traditional learning curve of a single human being. The concurrent changes within the organization can be studied. Sometimes the workers who produce the product discover more effective ways of working. The people who are charged with procurement often find suppliers who can deliver the materiel more quickly and more regularly. More accurate ways of scheduling the flow of different parts of the work may be developed. Industrial engineers have given considerable attention to this matter, but industrial psychologists have almost completely ignored it. Yet, certainly there are a number of psychological problems here, and industrial psychologists should give some attention to them.

It is said that a major problem in studying organizations is the difficulty in securing more than a very few of them that can be considered to be truly comparable. As a consequence, in studies of organizations it would appear that the number of cases must necessarily be quite small. When we study people, however, we certainly do not insist that they be comparable in all manner of characteristics. Indeed, in many situations we actually seek heterogeneity in our subjects so that our findings will be more generalizable, and I see no reason why we cannot argue in just the same way when we study organizations. The use of heterogeneous samples of organizations should make empirical findings more meaningful and reliable.

Nevertheless, it is true that the empirical study of organizations will be more difficult than the empirical study of people. Under the best of circumstances, it is not easy to obtain a large number of organizations, no matter what kind they are, that can be manipulated and exposed to a variety of specified conditions as we can do with human subjects. Furthermore, social and economic changes are always occurring so that over time we may not have the constant and controlled conditions that are needed. One solution to this problem would be to use simulations of organizations, miniatures, rather than actual business and industrial organizations.

Artificial organizations might be created in the psychological laboratory, organizations that are comprised of relatively few people, perhaps just 10 or 20. For many years, of course, social psychologists have used small groups of people in the laboratory, assigning them various sorts of tasks and systematically varying the conditions under which the groups operate. Many of these studies have been quite fruitful and have provided us with knowledge we would not be able to obtain otherwise. Furthermore, such studies may clarify issues, thus enabling us to do more pertinent field studies. In a similar way, industrial psychologists could assemble groups of people in the laboratory, delegate different roles to the different individuals so that they form the memberships of organizations, assign them some task as a goal of the organizations' activities, and provide them with whatever facilities are necessary. One does occasionally find in the literature reports of research of this sort, but not often, and seldom by industrial psychologists. I believe that the use of miniature models of organizations might be very fruitful. For example, in some exploratory investigations utilizing miniature organizations, I was able to observe the sorts of structural changes that occur in organizations as they develop from infancy to maturity, and thence to senescence.

Another way of simulating organizations is by means of mathematical models. Mathematical models are, of course, common stuff in other social sciences and in other areas of psychology. In economics, for example, we find a variety of mathematical models of the firm. In the areas of psychometrics and learning, mathematical models have been developed that have proven to be quite useful. We industrial psychologists can just as well develop mathematical models of organizations, models that would be useful in showing the effects of various circumstances on specific aspects of the nature and operations of organizations. Let me give an example of the utility of this procedure. A while ago

I worked out a simple mathematical model which enabled me to ascertain the effects of such factors as size and shape of the organization on the quality of men who reach the upper levels of management as a result of regular promotion procedures. Inasmuch as they deal with careers that extend over long periods of years, it is unlikely that matters such as the foregoing can be studied in actual organizations. It is with situations of this sort that mathematical models of organizations, and perhaps even miniature organizations, provide their greatest service.

The Role and Nature of Theory

Now I would like to say a few words about theory and our use of it. I apologize if I appear to be lecturing you on such a basic topic as theory. Nevertheless, it is true that we industrial psychologists almost entirely have been empiricists, and it is only recently that we have generated any real interest in that gossamer we call theory.

We recognize, of course, the value of theory as a means for integrating the many bits and pieces of knowledge we have gained from a diversity of empirical investigations. As a consequence of this integration, theory enables us to view a wide variety of otherwise disconnected facts in a total meaningful whole, and thus it is possible to extrapolate where there are gaps in our knowledge. Furthermore, as we all recognize, theory is very useful in research, for it provides systematic guides to new directions research ought to take and indicates new ideas that should be explored.

Another valuable function that theory performs, and one that we industrial psychologists generally overlook, is as a means for evaluating the significance of empirical findings. Because empirical studies necessarily utilize samples of individuals rather than populations, the findings of a single study are seldom completely trusted, and so, commonly, some statistical test is applied in order to gauge their dependability. Our statistical testing of differences and relationships has become as stylized as the courting dance of the whooping crane and often is just about as awkward.

Inasmuch as theory provides an integration of findings from a diversity of other investigations, if the results of a single investigation are not congruent with an appropriate theory, then certainly there are good grounds for doubting their dependability. Indeed, demonstration of the statistical significance of a set of findings is not enough. Those findings also ought to fit in with existing theory, or at the very least they should provide a basis for reasonable modifications of current theory or for new theory.

When we develop a theory, we feel committed to defend it to the very end, for we take it as representing the revealed Truth with a capital T. So we bend the large portion of our efforts to finding data that support the theory and explaining away data that are not congruent with it, and consequently we invest too much of ourselves in it.

We take theory as being an approximation of the truth. Our argument is that because our knowledge is incomplete we do not know the precise nature of a particular phenomenon, but only bits and pieces of it. We say that in order for those bits and pieces to be comprehensible we develop a theory that explains them and integrates them into a meaningful whole so that it provides at least a semblance of the truth. As we acquire more and more facts about the phenomenon, we modify our theories or develop new ones to take those facts into account, and thereby come closer and closer to the real truth.

The history of psychology teaches us that seldom are theories disproved by compelling empirical evidence. Rather, theories just fade away because they become less fruitful and less interesting, and so less popular. The work of Wertheimer and Watson did not really completely invalidate the structuralism of Wundt and Titchener. To the generation of psychologists that followed them, the ideas of Wertheimer and Watson were just more exciting, and those of Wundt and Titchener seemed sterile. Whatever happened to Spearman's G? Gone with the wind. Yet, the multiple factor analyses of Thurstone and others did not really disprove Spearman's theoretical position, rather they showed that it is more meaningful and useful to think of human traits as being multidimensional.

Psychological theories wax and wane. They wax as we attempt to overcome the deficiencies of older theory in explaining new facts and seek insights into new problems that now interest us. They wane not so much because they are found to be wrong, but because they do not pertain to the new problems that we, another generation of psychologists, consider to be significant. We ought ever to bear in mind that theories are nothing more or less than conveniences, aids to our understanding the nature of man.

What I am saying is that theories should work for us; we should not work for them. Theories

should be our servants, not our masters. Unfortunately, I fear, the opposite is too often the case.

The Impermanence of Facts

Next I would like to reflect on the survival value of the knowledge we arduously pursue in our so numerous empirical studies and report at such length in the long parade of our voluminous journals. Implicitly or explicitly, our position is that the characteristics of human nature revealed to us from our empirical investigations have the property of enduring truth. We believe that any information we obtain now about human nature was just as true in the past and will be just as true in the future. I wish to suggest to you that this is not necessarily the case. Quite the contrary, what we accept as being facts can by no means be taken to be enduring and established forevermore.

We think of significant fundamental changes in human nature as occurring only over millenia. Thus, we recognize that there are psychological as well as morphological differences between modern men and their arboreal and speluncean ancestors. Nevertheless, is it not possible that changes in the basic character of mental activity can occur over smaller spans of time? Since the turn of the century, has there not been such a variety of most significant social and physical changes in the world that have modified not only the environmental influences on man, but also his patterns of selective breeding? So, is it not possible that in this short span of time there have been significant alterations in the environmental and genetic determiners of human behavior?

Just a few decades ago the son of an immigrant from middle Europe who settled in Chicago invariably married the daughter of another middle-European immigrant, and Back Bay Boston stock maintained itself by careful intermarriage. But the upwardly mobile grandson of those middle-European immigrants attending Harvard Graduate School of Business might well meet and marry a Back Bay Boston lass. It seems highly unlikely that the factor structure of the traits and abilities of the offspring of this marriage, much less their personality dynamics and motivational complex, would be precisely the same as those of their grandparents. Could not the genetic and cultural mix produce something akin to hybrid vigor with rather different psychological laws?

In the last few years we have witnessed the quite sudden appearance of a generation of young people whose social behavior, attitudes, and values are strikingly different from those of the immediately preceding generation of young people. The change occurred so quickly that we are only beginning to recognize its significance. Surely this surging social movement has brought about basic changes in the psychology of people. The differences between the two generations certainly are more fundamental than just differences in mean scores in hair length, attitudes, motivations, and the like. Is it not possible that culture-loaded and culture-focused instruments such as the F Scale, as well as intelligence tests, do not measure precisely the same qualities in the two generations? Indeed, might it not even be possible that with individuals such as the young people of today, eager to learn so that they can quickly effect social changes, such a basic law as the superiority of distributed over massed practice is reversed so that a quick and concentrated exposure to material results in more rapid learning than exposure to it spread out in time?

While there might be disagreement about the extent to which the fundamental psychological properties of man change over time, there is no question but that the nature of the circumstances in which people work, together with the nature of their work, often change significantly over quite short periods of time. The introduction of stapling machines produced immediate modifications in the work of carpenters and in the particular skills they utilize, and the introduction of computers required executives almost overnight to be able to interpret and utilize great masses of new sorts of information and even to think and to state their problems in different and more precise terms. Consider the great changes in the degree of aggressiveness recently being manifested by those traditionally mild individuals, school teachers, as a result of the new permissive attitude toward unionization and strikes on the part of governmental workers.

A fact about human behavior that is established in a particular situation, with the particular sorts of people who happened to be in it at the time, might well not hold at a future time if the sorts of people attracted to that situation change. There are many obvious circumstances that result in a change in the sorts of people who concentrate in, or are placed in, a particular job. War and economic crises produce such changes, and so do social movements and legislative acts. Unquestionably there are many other factors which we simply do not recognize that cause shifts in the sorts of people who apply for a given job and work at it.

We industrial psychologists have given some attention to changes in the nature of the job and organizational situation with the passage of time, and we have given some, though lesser attention, to the possibility of changes in the sorts of people found in a particular job or organizational situation. But as far as I can see, we have not even recognized the possibility that as time goes by significant modifications in the basic psychological machinery of people may occur.

Fundamental to science is the proposition that facts once established are enduring, for if all were change we would have utter anarchy. Hence, in our training as scientists we were taught to believe that unless the attributes of nature have some permanence, it will be impossible to have any understanding of it. So deeply is this belief ingrained in us that I have felt forced to make as strong a case as possible for impermanence so that you will entertain at least some small possibility that what we think of as established facts are indeed not necessarily enduring. I do not ask you to accept the proposition that all facts change, fade, and pass away as time goes by, but just to admit the possibility that some few might.

I invite you to remember that we industrial psychologists are concerned with men and women who live in a dynamic society, a society that constantly seeks to change their nature, and who are engaged in performing tasks that are always undergoing modifications. As a consequence we ought to expect that what we take to be established facts about workers' behavior are not necessarily true forever.

Individual Differences and Individuality

I come now to the last matter I should like to discuss, individual differences and individuality. A good part of the heritage of industrial psychology lies in differential psychology. The pioneers in industrial psychology, beginning with Münsterberg, all realized that it is necessary to deal with the human problems of business and industry within a framework of individual differences. Unlike some of our colleagues, we industrial psychologists have never regarded the differences among individuals as bothersome "error variance." Rather, we have always considered them as real and important.

Perhaps because industrial psychology also has a heritage in psychometrics, we have generally held that it is more fruitful to think of people as differing among themselves in quantitative rather than in qualitative ways. For us, people are distributed along continua, not among a series of separate and distinct classes. That is, we hold that the differences among individuals are best considered as being differences in amount, frequency, or degree, and we believe that except for convenience there is nothing to be gained by attempting to separate people into kinds or sorts.

In recent years, our interest in the quantitative differences among people has led us to the examination of moderator variables and their role in contingent relationships. We have considered contingent relationships in connection with a variety of matters such as the validity of tests, leadership, and motivation.

As I am sure you know, a moderator is a variable that monitors the relationship between two other variables. If scores on a variable are related to the degree of relationship between scores on two other variables, then we say it operates as a moderator. Thus, for example, the higher and higher peoples' scores are on the moderator, the higher and higher is the relationship between their scores on two other variables. Other patterns can also occur. The important thing is that for people whose scores fall at different ranges on the moderator, the degree of relationship between their scores on the other two variables is different.

When we examine a variable to see whether or not it moderates the degree of relationship between two other variables, we are likely to analyze our data in a fairly crude fashion. Typically, we divide individuals into a high- and a low-scoring group on the basis of their scores on the presumed moderator and ascertain whether the degree of relationship between scores on the two other variables is the same or different for those two groups.

Perhaps because we have only recently begun to study contingent functions, we like to see the data relative to a moderated relationship presented in a simple manner such as I have just described. However, inasmuch as presentations of this sort are used repeatedly in study after study, I fear that we have come to think of a moderator as a property on which individuals are differentiated into separate and distinct groups, rather than as a continuum on which individuals are distributed all along its extent. Thus, we may say that the findings of a particular study indicate that in organizations that have a tall structure, supervisory success and authoritarian views about leadership are more closely related than they are in organizations that have a flat structure. The moderating variable, organiza-

tional structure, we treat as being qualitative, a variable that classifies organizations by type or kind. In our concern with convenience we overlook the fact that the moderating variable is indeed a continuous one along which organizations vary in accordance with the *degree* to which they are tall or flat.

This view about the nature of moderator variables has been reinforced as we have come to examine as moderators variables that are the classical examples we cite to students as being qualitative variables. Thus, we examine as moderators of the validity of occupational tests, variables such as sex, race, and even "kind of name."

I have no expertise that would permit me to make definitive statements about sex or race as variables. However, I gather from those who are knowledgeable about these two properties that the nature of variation in them is by no means certain, and, indeed, it may well be better to take them as quantitative rather than qualitative variables.

To illustrate the absurdity of dividing people into separate categories in terms of kind of name, currently a popular game in the Far West and Southwest, I give you the case of my youngest granddaughter, Anna Louisa Ghiselli. Just on the basis of her name alone an earnest investigator without any qualms would readily classify her as a child of Latin descent. *Chiaramente questa bambina è una vera latina.* Her ancestry, however, is about one-quarter English, one-quarter Lithuanian, one-quarter Polish, with the remaining quarter being more or less equally divided among German, Portuguese, Italian, and the three major sorts of Swiss. How representative this child of heterogeneous ancestry is of all western hemisphere people, if not indeed of all people of all hemispheres, even those who are called, or call themselves pure stock.

Unless we are concerned with matters such as certain social perceptions, biases, and the like, I can see no value at all in examining the somatic and nominal properties of people as moderators. Indeed, a case could easily be made that when we do use somatic and nominal properties in this manner it is simply because we are intellectually lazy. As psychologists and social scientists we ought to be concentrating our efforts on the examination as moderators of various psychological and social factors. We should be investigating the moderating effects of quantitative variables such as various sorts of cultural deprivations, roles accepted by the individual, the frustrations he develops which result from aggressions against him, modes of behavior thrust upon him, and the like—solid, directly measured, and significant psychological and social factors.

One of the values of the current social revolution is a reaffirmation that the classes to which people are assigned, classes differentiated on superficial bases such as somatic and nominal characteristics, should play no part whatsoever in making social judgments about people as in personnel selection and evaluation. The position is taken that the sort or kind of person that individual is said to be should be completely ignored, and any differences that are found between so-called "sorts or kinds" of people with respect to employment, job performance, or any social judgment of them should be taken as being purely incidental. If differences between classes are found, those differences are better attributed to more fundamental quantitative psychological and social variables which for one reason or another have some association with those classes.

Indeed, rather than placing importance on the uniqueness of the classes or categories of people, we ought to be focusing our attention on the uniqueness of the individual. Individuality, too, is a real property of people, and yet we have almost completely ignored it. We have not even developed a working definition of individuality, much less ways for measuring or describing it. Is the uniqueness of the person, his individuality, a matter not worth the attention of the industrial psychologist? Should we continue to devote a substantial portion of our attention to highlighting the differences among arbitrary categories of people? A few years ago we happily joined the attack on the stereotype of the organization man, but where is our interest in his opposite, the individualist?

I am certain that systematic investigations of individuality would be most fruitful and would turn up some interesting and surprising findings. For example, in the Italian civil service, an institution noted for its inflexibility and strict adherence to rules and regulation, quite unexpectedly I found that the more individualistic a manager was, the more highly was his work regarded by his superiors.

The intercorrelations among people can be computed from the scores they earn on any set of variables. Invariably these correlations will differ in magnitude so that at one extreme there are some individuals whose intercorrelations are high inasmuch as the order of their scores is very similar, and at the other extreme there are other individuals

whose intercorrelations with other people are low inasmuch as the order of their scores is quite unlike that of any other people. The remaining individuals are distributed between these two extremes. By this simple procedure individuality can be demonstrated and can be taken to be a continuous quantitative variable of exactly the sort we are all used to. There is, then, nothing odd or peculiar about the property of individuality which makes it elusive or especially difficult to deal with.

The many attributes which all men share testify to the oneness of mankind. Human beings are sufficiently similar that we have been able to make many useful and meaningful generalizations about them. However, while quite obviously it is important to study what is common to all men, we must not overlook the richness of what is unique to the individual. And surely there is much more to be gained from studying the person in the fullness of his individuality than there is from studying what he has in common with others who are placed with him in one or another of a series of contrived categories which are presumed to make qualitative distinctions among people. The point I have been trying to make is certainly not new. It is a cry that echoes down the long halls of the history of man, a cry of the individual for a recognition of his individuality, his uniqueness, and we industrial psychologists have not heeded it. The matter was well phrased some thousands of years ago in the Talmud: "The greatness of God is infinite; for while with one die man impresses many coins and they are all alike, the King of Kings, the Holy One —blessed be He—with one die He impresses the same image on all men, yet not one of them is like his comrades."

Opponent Processes as a Model of Neural Organization

LEO M. HURVICH *University of Pennsylvania*
DOROTHEA JAMESON *University of Pennsylvania* [1]

On an occasion such as this one we have an opportunity to pull back from the trees, the underbrush, and thorny bullweed to raise again, in case it has been lost sight of, the central question of visual perception: Why do things look as they do? Because they are what they are? No, because we are what we are.

Some of you will surely recognize the question as one raised precisely in those words by Kurt Koffka (1935). Without getting embroiled in the specifics of the mind–body problem and its many proposed resolutions—it was the subject of a recent major article in *Science* by Globus (1973)—it seems clear to us that the answer to the broad question "Why do things look as they do?" will be given in an informative way only when the principles of neural organization are known. To describe more specifically what we mean by this, we shall ask you to bear with us while we review first the opponent process principles of neural organization in relation to our own earlier work in color and brightness perception, remind you of some of the more recent electrophysiological data that are particularly relevant to these principles, and then discuss briefly the way in which the same principle of spatial neural organization can be shown to account for very different visual phenomena.

The phenomena of visual perception can be approached in a variety of ways, and they are well summarized in Koch's (1959) volumes on the study of psychology and Floyd Allport's (1955) volume on theories of perception, as well as in the many monographs that explicate a particular theoretical approach. For example, there is Graham's (1965) S–R approach, Gibson's (1966) view of perception as a direct apprehension of the invariants of distal stimulation, Helson's (1964) broadly unifying principle of adaptation level, Hebb's (1949) cell-assembly concept, approaches that emphasize motivational aspects, directive state, the transactional view, and so on. But for a long time, views of visual perception could be roughly divided into either of two camps that might be described as mosaic versus Gestalt, point-for-point image representation versus field representation, or elementistic or wholistic. Moreover, if what is given in the neurophysiology is a response mosaic that reflects each transitory change in the stimulus mosaic, there is obviously much cognitive decoding and resynthesis to be done on route from stimulation to perception. The elementalists consequently often stressed the importance of learning as the crucial process whereby the meaningless mosaic of sensory elements could be interpreted as the meaningful forms, outlines, and solid objects of the real visual world. The Gestalt approach to perception, on the other hand, was more compatible with a relatively greater stress on nativism, if nativism here is interpreted as an inherent physiological organization into delimited fields of higher nervous activity that lead directly to figure percepts. So the difference in point of view with respect to elementalism versus Gestalt also carried over into a difference in emphasis with respect to empiricism versus nativism. Conceptual differences of this sort also had their parallels in the rival views of the mechanism of color vision, and these existed long before the formal warfare between the Structuralists and Gestaltists was declared.

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Requests for reprints should be sent to Leo M. Hurvich, or Dorothea Jameson, Department of Psychology, University of Pennsylvania, 3815 Walnut Street, Philadelphia, Pennsylvania 19174.

On the one hand, all of the phenomena of color vision were considered to be interpretive resyntheses of three elementary fundamental sensations that were attributable to three kinds of retinal receptors and their associated nerve fibers; on the other hand, these same phenomena were considered to be attributable to a visual response mechanism whose fundamental properties and principle of organization could be inferred directly from the properties observable in the phenomena themselves.[2]

Consider, for example, the simplest of afterimage phenomena. Figure 1 shows the bust of a young girl; if you fixate rigorously on a single spot, for instance, the tip of her nose, and then shift fixation to a uniform field, the girl will reappear as an afterimage. You have all seen afterimages of this sort. Elementary texts seem to favor a stimulus that will generate an American flag, or a photograph that will give us W. C. Fields resplendent in top hat. But most of you may not have seen this particular afterimage because the figure comes from the estate of Goethe and was probably painted by him. It appears in a recent American edition of Goethe's monograph on color theory that was translated and edited by Herb Aach (1971) after Mattaei's German edition.

Goethe, as most of you know, was fascinated by visual color phenomena, and although he was mistaken about some of the physical principles involved in producing different kinds of colored stimuli, he had some sharp insights into the principles inherent in the perceptual effects. The negative afterimage he illustrated with this figure suggests, by its very name, the principle of response rebound of opposite quality. When a primary pattern of sustained stimulation has built up one mode of response in the visual system and the stimulus is then suddenly removed, the consequence is a different mode of response, one that is opposite to, or the negative of, the initial response.

Obviously, an opponent physiological rebound is not the only way to account for a negative afterimage that is projected on a uniformly illuminated surface, and elementalists feel quite comfortable with the explanation that the phenomenon results from what is called *local adaptation* or *fatigue* of the retinal receptors.[3] If the cone receptors are

Figure 1. Black and white photograph of Goethe's figure for colored afterimage. (For color plate, see Aach, 1971.)

first selectively desensitized by the different wavelengths contained in the different parts of the primary stimulus that is imaged on the retina, then only those receptors that have not been desensitized or bleached can respond to the homogeneous white light of the projection surface, hence the negative afterimage. Other characteristics of afterimage phenomena, which are best summarized as manifesting an oscillatory pattern of opposite effects that tend to damp out in time, make it difficult to avoid postulating an organization of antagonistic neural processes that wax and wane in temporal alternation as the system returns to equilibrium. These oscillatory events tend not to be featured in accounts that emphasize localized receptor bleaching to explain the occurrence of negative afterimages.

The mosaic of receptors hypothesis is still less comfortably accommodated to the phenomena of simultaneous contrast, which demonstrate another principle of neural organization. Just as the negative afterimage can be seen as an instance of *opponent temporal organization*, namely, primary response followed by opposite rebound, simultaneous contrast can be seen as an instance of *opponent spatial organization*, namely, a primary response in one spatial location inducing an opposite response in an adjoining neural region. These spatially opponent effects can, of course, be seen in strictly achromatic patterns, as in Figure 2, where

[2] A discussion of these views in their historical context can be found in Hurvich (1969).

[3] Brown (1965) reviewed such explanations in some detail and also discussed a number of specific photochemical hypotheses that have been advanced to account for afterimage effects.

Figure 2. Achromatic contrast illustration. (Pattern from Albers, 1963, Plate IV-2.)

the gray on the right looks darker than that on the left only because of the luminance difference between the light surround on the right and the darker one on the left.

These spatially opponent effects are equally obvious and important in the organization of the hue-coding system. In the colored original of Figure 3, the two X figures appear in two differently colored surrounds. Although the Xs are spectrally identical retinal stimuli, the different qualities of response induced in the crossed lines by the different surround activities give rise to qualitatively different percepts of two differently colored Xs. An elementalist who is incurably biased against the concept of opponent neural organization to explain spatial contrast has, for his explanation, the choice either of very rapid local adaptation together with eye movements that will cause the surround to desensitize the focal area, or else he must resort to a cognitive unconscious inference hypothesis. In this specific case, it would mean unconsciously assuming that the two sides of the figure are illuminated by lights that are different on the left and on the right. One can usually work out the necessary inferences to come up with a cognitive account of such contrast effects, but it sometimes comes out sounding more like a Watergate cover story than an explanation.

Just to keep the record straight, we have no doubt that there are many situations in which what we see is strongly influenced by what we think is out there, but the universally observed phenomena of simultaneous contrast require something more than that kind of explanation.

If we consider a little more closely the specific character of the oppositions evident in both afterimage and contrast phenomena, they can be itemized very simply: What is light in the primary image is dark in the afterimage and vice versa, what is blueish in the primary image is yellowish in the afterimage and vice versa, and what is reddish in the primary image is greenish in the afterimage and vice versa. Similarly in the simple contrast situation, the figure in the lighter surround looks darker than the same figure in the darker surround, the figure in the bluer surround looks yellower than the same figure in the yellower surround, and the figure in the redder surround looks more greenish than the one in the greener surround. What we have listed are three pairs of visual qualities that show mutual opposition within each pair: white versus black, blue versus yellow, and red versus green.

Figure 4 describes the appearance of a spectrum with as economical a vocabulary as possible. Starting on the left at the shortest visible wavelengths, around 400 nanometers, we see violet or reddish blue; moving from left to right toward longer wavelengths, the reddishness diminishes until we see simply blue in the vicinity of 475 nanometers; beyond this there is increasing greenness and diminishing blueness and we see simply green near 500 nanometers. Toward longer wavelengths yellowness enters in increasing proportion relative to green, and when we reach approximately 580 nanometers the spectral light appears simply yellow. Orange, or more economically stated, yellow–red, comes in at wavelengths longer than 580 nanometers. The red increases relative to yellow at still longer wavelengths, but spectral light never quite becomes uniquely red out to the long-wave limit of the visible spectrum at about 700 nanometers.

Note that to describe the hues of the spectrum we need the hue names *red*, *yellow*, *green*, and *blue*,

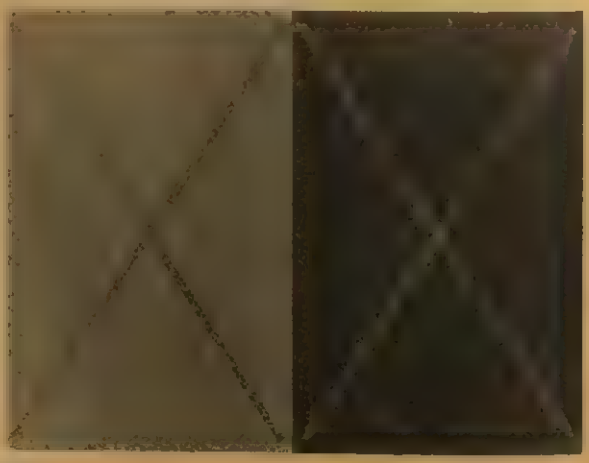

Figure 3. Black and white photograph of colored pattern that illustrates hue contrast. (For color illustration, see Albers, 1963, Plate VI-3.)

but also note that if we are to include all aspects of the spectral light qualities we would, for example, further characterize the yellow near 580 nanometers as more whitish than, say, the very slightly yellowish red at the spectral extreme. The achromatic whitishness or blackishness of each color appearance is a descriptive component just as necessary as the single or binary hue names. Note further that the hue descriptions include the binary pairs red–blue or green–blue but never yellow–blue, also yellow–green or yellow–red but never green–red. Thus, in the simple description of hue qualities we find that yellow and blue are mutually exclusive, as are red and green. This is, of course, true for all colored objects and is not restricted to the spectral light situation. The opponent character evidenced in this property of mutual exclusiveness has the same paired characteristics as the opposition observable in both afterimage and contrast phenomena. What we see does indeed seem to be trying to tell us something about the way the visual nervous system is functionally organized. Moreover, the opponent or mutually exclusive characteristics of the hue qualities themselves suggested the principle of measurement by which the qualitative descriptions that we have just itemized could be subjected to precise psychophysical measurement and expressed as quantitative spectral functions (Jameson & Hurvich, 1955). This psychophysical measurement procedure was a hue cancellation or *bucking* technique. The relative strength of a given hue elicited by spectral stimuli of a series of different wavelengths was estimated by determining the variable energy required at a fixed wavelength that elicited the opposite hue in order for the effects of the two mutually opponent hue processes to precisely cancel each other. For example, we measure how much "yellow" is generated by each of a series of different spectral wavelengths between 500 and 700 nanometers by determining how much energy of a 480-nanometer blue-generating stimulus is necessary to just balance out each of the yellows. The results of this

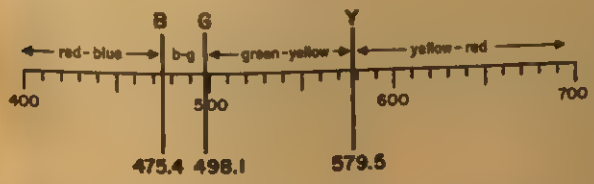

Figure 4. Description of hues of spectrum with loci of unique hues determined experimentally for one individual. (From Hurvich & Jameson, 1951.)

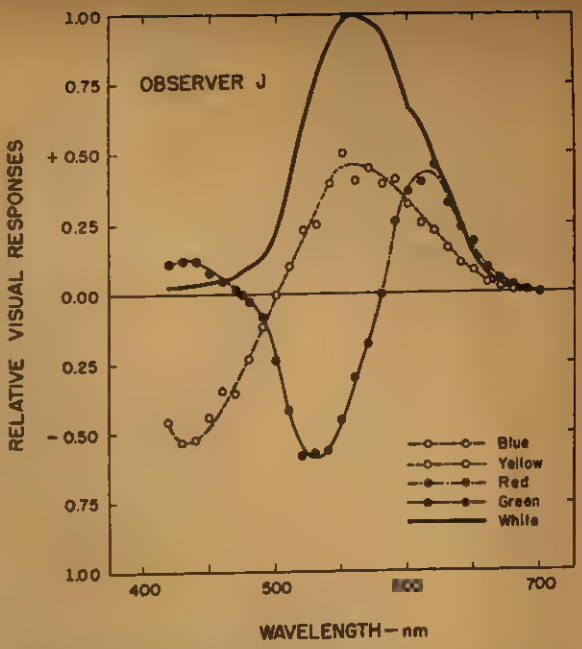

Figure 5. Psychophysical measures of chromatic and achromatic responses. (From Jameson & Hurvich, 1955.)

experiment for one observer are shown in Figure 5. These are old data which were published in the *Journal of the Optical Society* in 1955, and we show them here simply as basic data for human color vision against which we can compare some of the electrophysiological findings that have emerged from various laboratories, first for fish and then for a variety of animals up through the macaque monkey.

These psychophysical functions not only provide a more precise description of color appearances, but also, since they are quantitative expressions of the three qualitative variables basic to color vision, they permit the derivation of other basic quantitative data of color vision such as the three-variable color-mixture data, wavelength discrimination data, and so on. Such derived functions, compared with independent data measured in various other laboratories, were published many years ago in the *Journal of the Optical Society* (Hurvich & Jameson, 1955) and summarized in the *Psychological Review* (Hurvich & Jameson, 1957).

Figure 6 is a schematic diagram of the conceptual model developed to show how the three paired color variables of the quality-coded neural response system are related to the cone receptors in which the initial spectrally selective light absorption takes place. At the time that this model was developed, the spectral absorptions of the three kinds of cone

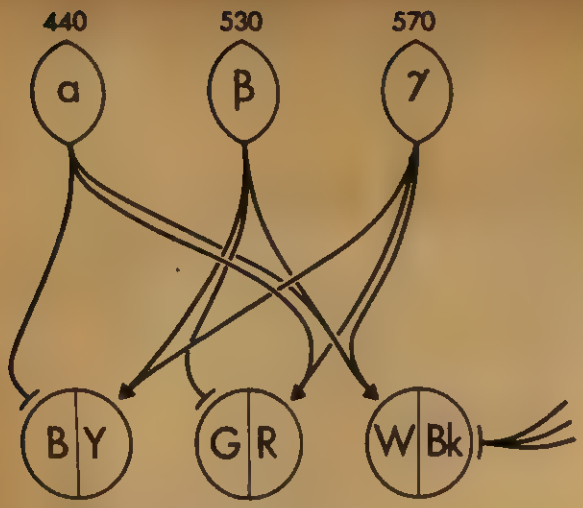

Figure 6. Schematic diagram of relations between cone absorptions and opponent response processes.

receptors were not independently known, and their absorption characteristics had to be inferred on the basis of psychophysical data together with specific assumptions about the bleaching characteristics of visual pigments. We now have more information both about bleaching properties and about the spectral absorptions of the cone photopigments (Dartnall, 1972). This information comes from a technique known as retinal reflection densitometry (Rushton, 1958; Weale, 1957), which measures light reflected back from the whole retina before and after bleaching, and also from a technique known as microspectrodensitometry, which can measure, at least approximately, the spectral absorptions of isolated, individual retinal cones (Brown & Wald, 1963, 1964; Marks, Dobelle, & MacNichol, 1964). It is on the basis of this independent evidence from the discipline of visual photochemistry that the three kinds of cones, labeled here alpha, beta, and gamma, are specified as containing photopigments whose absorption maxima occur approximately at 450, 530, and 570 nanometers, respectively (Jameson & Hurvich, 1968). Note that in this model there is no suggestion that the receptors themselves provide the color coding. Rather, the color coding is assumed to be associated with the neurophysiological events at the opponent process level (Jameson, 1972). This level is organized, according to the model, in three kinds of paired processes. Within each pair, one response mode is labeled positive and one negative to indicate their opposite physiological characteristics and to accord with their opponent and mutually exclusive perceived characteristics. Which mode of response is considered positive and which negative is immaterial. The plus and minus signs are used as conventions to indicate oppositeness of response mode and also to express the property that the ultimate net neural response is determined by the algebraic sum of the signed arousal inputs from the excited receptors to which each neural system is functionally related. Thus, if the incident light on a given part of the retina is heavily weighted in short-wave energy, it will be absorbed more strongly by the short-wave alpha receptor than by the beta or gamma receptor, and the signed input from the more strongly excited alpha receptor will be greater than the combined inputs from the less strongly excited beta and gamma receptors which are of opposite sign. Consequently, the net response of the blue–yellow color-coded neural system will be signed as a blue response. If the nature of the light stimulus is changed so that it is now more heavily weighted in energy from the long-wave region of the spectrum, then it will be less strongly absorbed by the alpha receptor than by the beta and gamma receptors, the signed input from the less strongly excited alpha receptor will be less than the combined inputs from the more strongly excited beta and gamma receptors which are of opposite sign, and the net response of the blue–yellow color-coded neural system will now be signed as a yellow response. So the model leads us to expect that, even if we were red–green blind and lacked the red–green opponent response mechanism entirely, we would still retain some spectral color discrimination because the hue-coded response of one sign for short wavelengths would switch to one of the opposite sign for longer wavelengths. Congenital protanopes and deuteranopes and normal vision in the retinal periphery are instances of this kind of reduced color vision (Hurvich, 1972, 1973; Moreland, 1972).

How might this bimodality of response be expressed in the electrical responses of individual nerve cells? The first discovery of wavelength-specific neurophysiological response in individual cells of the retina was made by Gunnar Svaetichin (1956) in the isolated fish retina. And this vertebrate, unlike the cat, is known to have good color discrimination. The responses took the form of graded changes in dc potentials that varied in magnitude from one wavelength to the next, and in some cells showed a reversal in polarity of response, from hyperpolarizing potentials at short wavelengths to depolarizing responses at longer wavelengths, or vice versa. Figure 7 shows such records from Svaetichin's work. These results have

been confirmed many times, in the retinas of many species and by different investigators in many different laboratories (Abramov, 1972). The electrical potentials are thought to be responses of the horizontal cells of the retina (Svaetichin & MacNichol, 1958), and they are often referred to as S-potentials in recognition of Svaetichin's early recordings. To illustrate the complexity of the retinal network, Figure 8 shows a diagrammatic cross section from the recent work of Dowling and Boycott (1966) with the rods and cones at the top, horizontal cell sending its processes laterally in the layer between the receptors and the different kinds of bipolar cells, and amacrine cells also sending their processes laterally between the layer of the bipolars and the different types of ganglion cells,

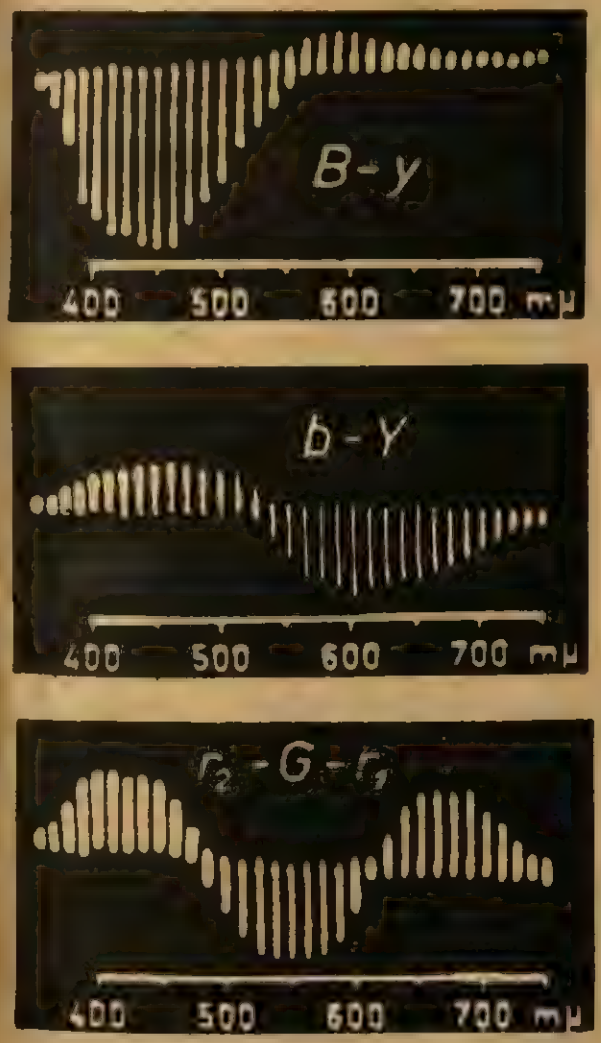

Figure 7. Records showing reversal of polarity of electrical graded potential responses to light stimuli of different wavelengths. (From Svaetichin et al., 1963.)

Figure 8. Summary diagram of the contacts among retinal cells of various types. (R, rod; C, cone; MB, midget bipolar; RB, rod bipolar; FB, flat bipolar; H, horizontal cell; A, amacrine cell; MG, midget ganglion; DG, diffuse ganglion. After Dowling & Boycott, 1966.)

shown at the bottom of the diagram. At the later neural cell level, that of the ganglion cell, the responses are characterized by spike discharges rather than by graded potential changes alone. Instead of electrical polarity reversals exhibited by the graded potentials, the spectrally opponent responses of ganglion cells are exhibited by spiking discharges versus suppression of spiking discharges. For example, when short-wave light stimulates the retina, there may be spiking at the onset of stimulation and suppression of spiking at stimulus offset, whereas at longer wavelengths there may be suppression of spike discharge at stimulus onset followed by a burst of spikes at stimulus offset, the so-called off-discharge. Spectrally opponent responses of this sort have been recorded from ganglion cells of the fish retina and the retinas of other species as well as from the optic nerve, and from both the lateral geniculate and visual cortex of the monkey (DeValois, 1965; Hubel & Wiesel, 1968; Wiesel & Hubel, 1966). Figure 9 shows records of such responses from the work of DeValois and his co-workers. Compare the response to a 440-nanometer light above with the response of the same cell

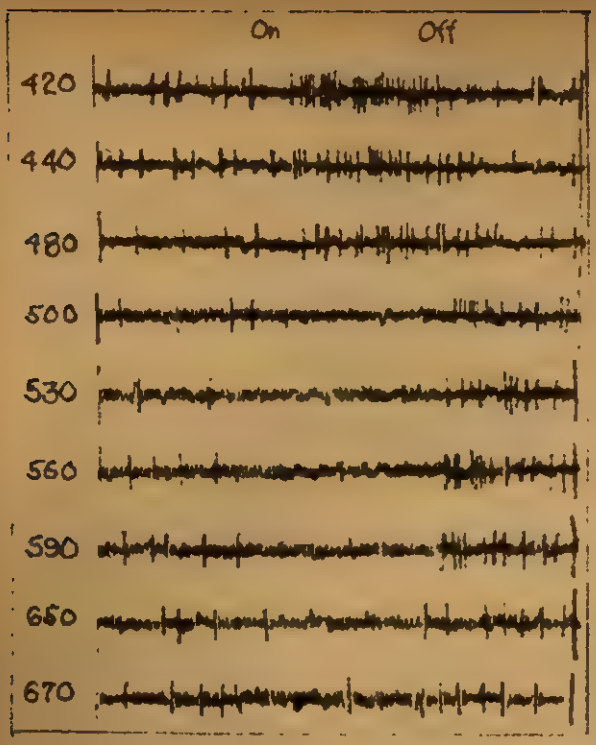

Figure 9. Series of records showing change in response of a spectrally opponent cell in the macaque LGN. (The cell shows on-responses to short wavelengths [420–480 nanometers] and off-responses to the long wavelengths [500–670 nanometers]. This figure is our tracing of a photographic reproduction from DeValois, 1965.)

to 590 nanometers below. These are records from the lateral geniculate body of the macaque monkey, and in these so-called spectrally opponent cells there is a change from spiking to stimulation at short wavelengths to suppression of spontaneous discharge during stimulation and an after-discharge of spikes for stimuli of longer wavelengths.

If we assume that the coding in a cell of this sort is blueness when the cell fires and yellowness when the cell firing is inhibited by stimulation, then we can relate this pattern directly to the negative afterimage phenomena with which we started this discussion. A short-wave stimulus is imaged on the retina, cells with this particular coding fire, and that firing codes blueness. When the stimulus is removed the cell goes into a silent phase, the coding is the opposite of blueness, namely, yellowness or the afterimage hue. And eventually the cell returns to its spontaneous firing rate again. If the stimulus is of a longer wavelength, the cell is silent while the stimulus persists and fires when it is removed. This is consistent with a yellow primary hue in this instance followed by the blue afterimage hue when the cell again fires at stimulus off, again with the "firing equals blueness" coding. This is not to say that no bleaching or receptor adaptation or desensitization occurs when an afterimage is developed, but rather to say that the neural organization is such that the opponent activities fundamental to hue perception lead, *sui generis*, to opposite aftereffects.

What of the spatial contrast phenomena that show qualitatively similar effects? If we first put these contrast phenomena into the perspective of the conceptual model, we can then see how the model (Figure 10) relates to the detail seen in electrophysiological studies of individual nerve cells. In looking earlier at the schematic of the conceptual model we considered only what might be called the vertical interrelations, namely, those between the receptor units and the opponent neural response pairs. What the model assumes, in essence, is that specific lateral interconnections exist such that activities aroused in a given paired system, whether in a blue–yellow system, a red–green system, or a white–black system, both influence and are reciprocally influenced by ongoing activities in corresponding adjacent and surrounding neural systems. This influence is assumed in the model to be both mutual and opponent, that is, if, say, blue, or blueness-coded, activity is ongoing in one functional unit of the blue–yellow system, opposite, namely, minus or yellow, activity is induced in adjacent functional units of the same coding. The same mechanism operates, according to the model, in the opponent red–green system, where redness activity

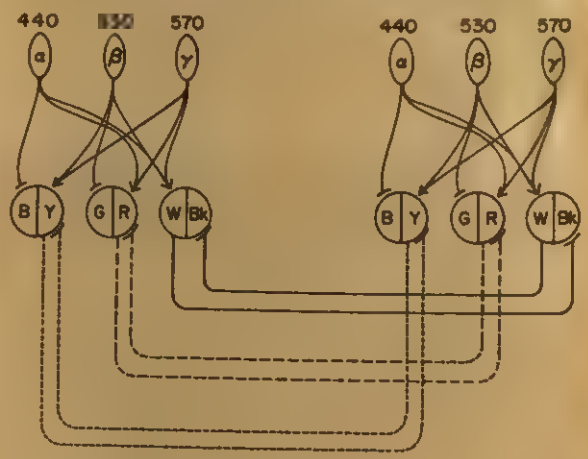

Figure 10. Schematic diagram of model illustrated in Figure 6 expanded to include representation of reciprocal lateral influences at the opponent process level.

induces greenness activity in neighboring units, and vice versa, and in the opponent white–black system. The achromatic white–black system differs from the hue-coded opponent pairs only with respect to symmetry of arousal. There is no external stimulus to excite blackness directly. In terms of what we called the vertical interrelations, all three types of cone activities arouse activities of common sign in the achromatic system and the code is "whiteness." The oppositely signed blackness response comes about only indirectly as an aftereffect of the direct focal stimulation of the retina, or through the system of laterally induced opponent activities. It is the lateral, opponent, induced activities that are, we believe, responsible for the phenomena of simultaneous contrast. Since the conceptual model has been developed in a quantitative way that also expresses these lateral opponent effects in equation form, we have been able to derive quantitative psychophysical functions to express the amount of hue and brightness contrast effect produced for a variety of stimulus parameters. Again we are referring here to theoretical derivations long published and compared with psychophysical measures from both our own and various other laboratories (Jameson & Hurvich, 1964). There is one set of psychophysical functions contained in that paper that we would like to remind you of because it makes an additional point about the mechanisms of visual perception and a basic principle of neural organization to which we have not yet alluded.

When we think of visual contrast effects, usually the first examples that come to mind are effects that we would have to describe as nonveridical. A gray paper in a light surround tends to look black; a gray paper in a green surround tends to look red, and so on. On the other hand, there are all the phenomena of perceptual constancy which attest to the veridicality of our perceptions of objects despite the changes that their retinal images undergo with changes in illumination, and so on. Is it possible that the same visual mechanism that causes surfaces of *constant* characteristics to appear to *vary* in their perceived color and brightness can also bring about the approximate constancy of object appearances? We believe the answer to be very importantly yes.

Figure 11 shows the extent to which the model predicts that brightness constancy will result by the action of the opponent lateral induction processes that account for simultaneous contrast. The stimulus situation to which the figure refers is a visual pattern consisting of a number of different re-

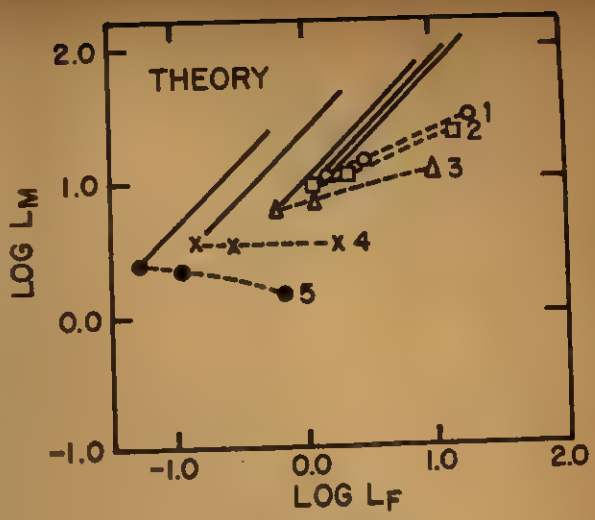

Figure 11. Theoretical predictions of brightness changes with changes in illumination level of a five-element pattern. (Solid lines, —, indicate photometric predictions; dashed lines, – –, indicate visual predictions based on opponent spatial interaction model [Jameson & Hurvich, 1964].)

flectances that might be described as varying in appearance from near white to very dark gray, and viewed at three different levels of illumination. The amount of light reflected from each part of the pattern increases in the same proportion as the level of illumination is increased, and, if the eye were simply a photometer, then the matching luminance, plotted on the ordinate, would simply increase for each different area in the pattern in direct proportion to the increase in overall illumination, plotted on the abscissa. This photometric prediction is shown by the family of straight lines with a slope of one that are drawn in Figure 11. Each different symbol in the graph refers to an area of different reflectance in the pattern. The equations that embody the opponent spatial interaction concept predict, however, that the changes in apparent brightness will be much less than those that would be recorded by a photometer. The visual predictions are shown by the dashed lines. The pattern as a whole will not show perfect constancy, although a particular area within it may, in this case the area numbered 4 and represented by the Xs in the graph. The departures from constancy are predicted to be such that the pattern will be a little more contrasty, a little sharper, if you will, at the higher levels of illumination. The experimentally measured matching luminances are shown in Figure 12. Here you can see the increase in

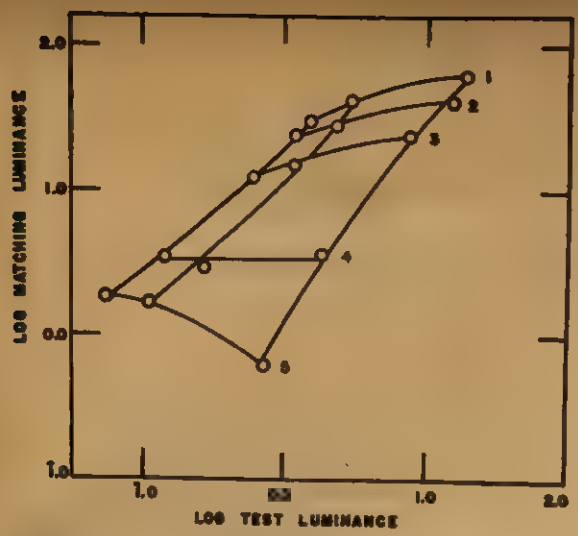

Figure 12. Matching luminance data for apparent brightness changes with changes in illumination level of a five-element pattern. (From Jameson & Hurvich, 1964.)

gradient from left to right between the darkest and lightest areas at the different illumination levels.

Painters and artists, of course, do not need a model of the spatial interaction in the visual nervous system to represent this effect in their canvases and drawings. If they want to recreate the impression of bright illumination in a drawing or painting, they do so by manipulating their pigments or shading to increase the contrast from one area to the next. And as we know from the impressionists, they also do not need to be told that the same kinds of effects occur in the opponent hue mechanism: In a painting, increasing the yellow of the fully illuminated area and the blue of the shadowed area is quite effective in representing bright sunlight. But our main point here is that the mechanism of lateral interaction, the same mechanism that causes the changes in appearance that we call contrast, also brings about the relative stability with change in overall illumination that we call constancy, or more correctly in terms of our actual perceptions, approximate constancy.

We wish to consider now another aspect of the spatial neural organization that we believe can account for another set of apparently mutually contradictory phenomena, namely, contrast, on the one hand, and reverse contrast, spreading, or assimilation, on the other. To explore this issue we must look more closely at the detail of the opponent spatial organization as revealed in studies of the electrophysiological responses of individual nerve cells in the retina and other visual centers.

We know that when responses are recorded from ganglion cells in the retina, such a single cell responds not simply to punctiform stimuli located within very tiny areas of the retinal image surface, but rather to stimulation within a relatively large area which is known as the receptive field of the cell. Moreover, the receptive field is characterized as eliciting one type of response in the ganglion cell for stimuli that fall in its center of receptivity, say, "on" responses, and an opposite mode of response, in this case spike inhibition during stimulation with subsequent "off" responses for stimuli that impinge on its peripheral, or surround, area of receptivity (Kuffler, 1953).

We know from the electrophysiological work that at the level of the retina, in fish, the ground squirrel, the cat, and other species, the receptive fields have a typical organization into a roughly circular center with antagonistic annular surround. This type of organization is illustrated in Figure 13. Receptive fields with approximately circular center-surround organizations of this sort can also be located in the higher neural centers, in the lateral geniculate of the cat and the monkey, for example, and we know from the extensive work of DeValois and his associates that many geniculate cells also have spectrally opponent or, apparently, hue-cod-

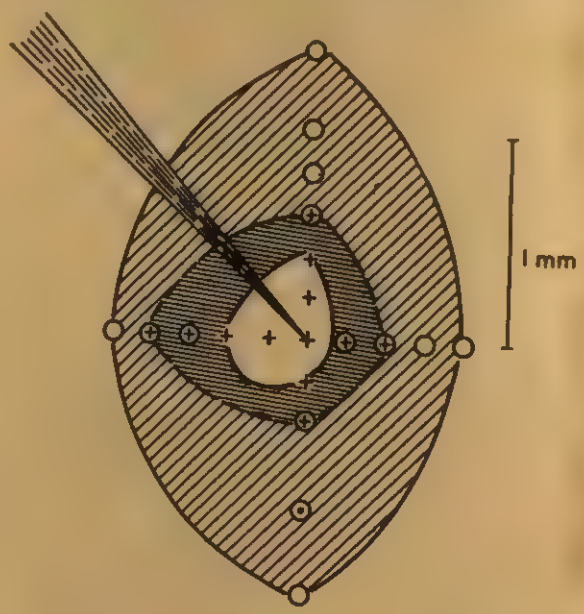

Figure 13. Receptive field map of ganglion cell in cat retina. (Crosses represent on-discharges [central region], circles represent off-discharges [diagonal hatching, peripheral region]. Both on- and off-discharges occur in intermediate region [horizontal hatching]. From Kuffler, 1953.)

ing properties. Most of DeValois' (1965) records were obtained with large or diffuse light stimuli, and thus provide no information on the receptive field characteristics of the individual geniculate cells. The spatial properties of those monkey geniculate cells whose receptive field properties have been explored with stimuli of appropriate dimensions, for example, by Wiesel and Hubel (1966), seem to fall into a number of different classes whose significance is not directly apparent. Arrangements that would properly code both hue and hue contrast would be center versus surround opposition together with wavelength opposition of reversed sign in both center and surround. Cells of this sort have been detected by Hubel and Wiesel (1968) in the monkey only in the visual cortex where the circular center-surround receptive field organization is not typical but where spatial characteristics that are apparently more highly specialized emerge. Rather than pursuing the neurophysiology of line detectors, curvature detectors, and so on, let us consider a diagrammatic representation that illustrates the essence of the opponent spatial organization that emerges from specific relations assumed between groups of retinal receptor units, on the one hand, and single neural visual cells, on the other. The organization diagrammed in Figure 14 represents a cell that shows a spatially opponent receptive field organization but no wavelength specificity with respect to mode of response. Note that the cell is under the influence of activities engendered when light is incident upon and absorbed by a relatively large number of receptors on the retina, that the inputs from those receptors located in the center of the receptive field of such a cell are associated with one mode of response, and that inputs from other receptors located in the periphery of the cell's receptive field are associated with the opposite mode of response in the cell. If the center is excitatory of spike discharge the periphery is inhibitory, and vice versa. The response profile of such an arrangement is shown in the lower part of the figure and is identical to what Békésy (1968) termed the *neural unit* in dealing with Mach band phenomena. Given stimuli of the appropriate dimensions, such an organization is ideally suited to heighten contrast; thus, a weak light falling on the receptors in the center of the receptive field would cause some firing in such a cell, but a stronger light falling on the receptors in the surround of the receptive field would inhibit or diminish the firing rate of the cell. The net response of the cell would then be equivalent to or even less

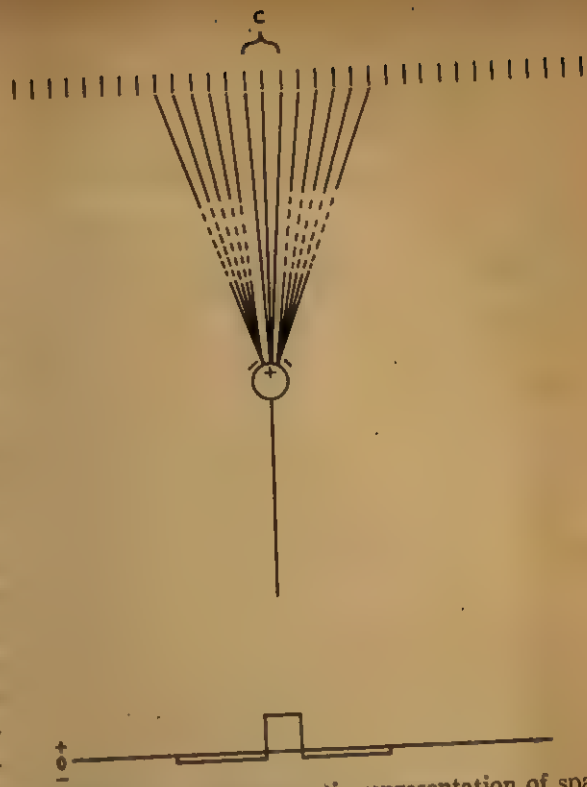

Figure 14. Diagrammatic representation of spatially opponent receptive field organization. (See text.)

than what it might be were there no light at all impinging on the receptors centered in its receptive field. So because of this arrangement, a gray spot centered in a light surround would, in terms of its physiological coding, appear darker or blacker than it would if the response depended on the photometric luminance of the gray spot alone. There are, of course, many such cells whose receptive fields on the retinal surface overlap, but a mathematical analysis of the outcome will, under the circumstances described, lead to the same outcome with respect to heightened contrast that we have just described for the single cell that is under the influence of an organized field of receptor inputs.

It should be emphasized here, however, that the outcome of any such spatial arrangement depends very strongly on the dimensions of both the pattern of stimulation on the retina and the receptive field sizes relative to that pattern. Thus, for example, adjacent retinal areas of stronger and less strong illumination that are of very small dimensions such that the adjacent differences in illumination fall not in the center and surround of the receptive field, respectively, but well within, say, the circumscribed central area, will produce no enhancement of re-

Figure 15. Black and white rendering of highly magnified colored detail from Seurat's "Grande Jatte."

sponse differences, but rather they will produce a summated response in the cell. Moreover, an image grain of the sort envisaged here, where the spatial dimensions are very small relative to the dimensions of the receptive fields of the cells responding to the related receptor inputs, will fail to be resolved at all by such an assembly of visual cells. The perception will be one of uniformity, or of perfect spatial light mixture. Outside of the laboratory, we are probably most aware of such spatial light mixture when we view a pointillist painting at a sufficiently remote distance. On approaching closer to such a painting we begin to resolve the variegated texture of the painted surface, and if we get too close the perception becomes one of an abstract array of differently colored dots or blobs. (Figure 15 is a black and white rendering of a colored detail.) This is a greatly magnified detail of the "Grande Jatte" by Seurat, who, as you know, exploited the spatial light-mixture effect in the development of his very finely structured pointillist technique. A photograph of the full painting is shown in Figure 16.

Figure 16. Black and white rendering of Seurat's "Grande Jatte."

In view of the crucial nature, with respect to the perceptual outcome, of the dimensions of the receptive fields of visual cells relative to the dimensions of the stimulus pattern imaged on the retina, information about the dimensions of receptive fields of individual cells becomes highly relevant to any conceptual modeling intended to handle contrast and mixture phenomena. From the electrophysiological evidence available for the visual system of the monkey, two general findings about these dimensions can be stated. One is that the diameters of the receptive field centers are smallest near the fovea and tend to increase in size as distance from the fovea increases. The other finding, equally important, is that for any given region of the retina there is a very appreciable spread in the measured diameters of the receptive field centers. Figure 17, from Hubel and Wiesel's work (1960), provides the evidence for both of these statements.

We have seen that, depending on the diameter of the receptive field, for a given pattern of stimulation on the retina, there is a physiological basis for either accentuation of contrast, or mixture and failure of visual resolution. But given the range of receptive field sizes even for cells responding to the same part of the retina, we should anticipate that the responses of some cells would lead to one kind of perception and the responses of other cells would lead to the other. We have, in fact, perceptions that we can readily interpret as the outcome of precisely such a physiological organization, in the visual phenomena called *assimilation*, the Bezold spreading effect, or sometimes, reversed contrast.[4]

[4] For colored illustrations of this effect, see Evans (1948, Plate XI opposite p. 192).

These are phenomena in which a repetitive pattern is clearly resolved against a differently colored background, and yet the hue or lightness of the pattern seems to be more like the background color, or vice versa, as if the pattern and background were mixing with each other to some extent, rather than contrasting with each other. If the resolution of the pattern as distinct from its background is being carried out by the responses of visual cells with small receptive fields, while at the same time cells with receptive fields too large to provide such resolution are responding to the same parts of the stimulus pattern imaged on the same part of the retina, then the phenomenon of mixture or assimilation of pattern and background at the same time that it is clearly resolved loses its mystery (Jameson & Hurvich, 1972). We shall report on some quantitative aspects of this problem in the near future, but the effect is a large one and easily demonstrated. The pattern shown in Figure 18 is, in the original, printed in three colored inks, namely, a red, a green, and a blue. Separate hues are seen in each of the center bull's-eye and annulus patterns, but they can appear to be mixed in the easily resolved horizontal stripes.

A number of other visual phenomena can also be related to these receptive field size variations. Thomas (1970) considered some of them in a recent review article, and they are unquestionably at the base of the many effects that can be shown to be frequency specific (Blakemore & Campbell, 1969), since spatial frequency is a special case of variation in image size. Moreover, some organization of cells with common spatial properties must be assumed if we are to explain the various specific adaptation phenomena that are variants of the McCollough effect, which McCollough (1965) originally demonstrated as a specificity that conjoined hue, size, and orientation. The way in which such highly specific cellular receptive field organizations can arise at the cortical level through inputs of systems of cells with shared hue and dimensional properties would require a whole additional exposition. Hubel and Wiesel (1965) worked out a detailed schema for achromatic cells for the cat cortex, and a similar scheme works out for the human or monkey cortex if we add the necessary additional variable of hue specialization with its bimodal opponent properties.

One of the general points that we should like to emphasize about the visual system is that the system is organized by means of opponent processes to yield a general tendency toward reestablishing equilibrium whenever the balance of the system is

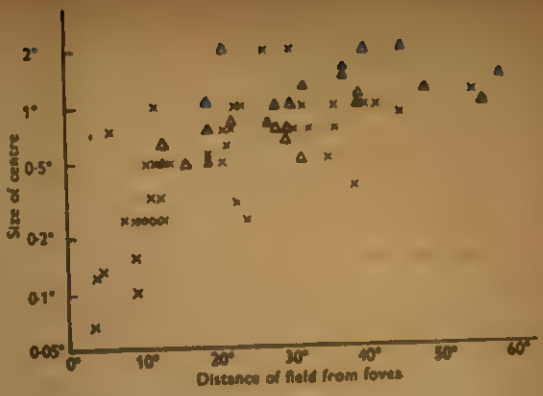

Figure 17. Diameters of receptive field centers in degrees (logarithmic scale), plotted against distance in degrees of each field from the fovea. (X, on-center units; Δ, off-center units. From Hubel & Wiesel, 1960.)

disturbed by stimulation. Thus, the light stimulation that is followed by a back reaction, which can be observed phenomenally in afterimages, is more correctly understood as stimulation of one state, which state itself acts as an internal stimulus for the arousal of the opposite neural process. If the stimulation were to continue unchanged in time, the two opponent processes would cancel each other in time, and there would be no phenomenal experience except the negative one of disappearance—fixed images disappear. So the opponent neural organization is an equilibrium-seeking device, but this equilibrium seeking is itself thwarted by an opponent system of another sort, namely, the efferent system to the muscles that under normal conditions keeps the eyes in motion so that the retinal image cannot be still even in a motionless external

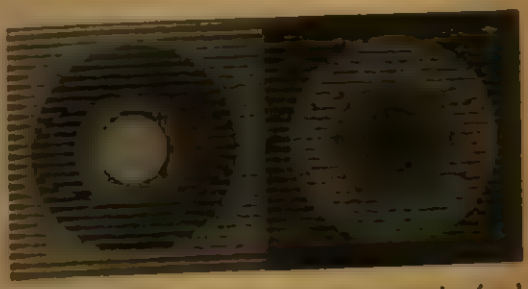

Figure 18. Black and white photograph of colored pattern that shows both contrast and assimilation. (See text. Colored original by David L. Burke Design Corporation for Scott, Foresman Publishers.)

environment. Similarly, the opponent center-surround organization of the receptive fields of nerve cells that serve so well to accentuate border contrast, produce Mach bands, and so on, are ideally organized to turn off when the retina is presented with a spatially homogeneous field of stimulation, or Ganzfeld. A visual field that contains no spatial gradients contains no spatial information, and hence nothing is lost by an opponent organization of the neural system that gives a net response of zero when the information content is zero, even though, in terms of light quanta, there may be an appreciable amount of stimulus energy flooding the retina.

It is the equilibrating effect of the opponent processes during continued stimulation and the release of the strong back-reaction when the primary stimulus is suddenly removed that Solomon has found especially enlightening in relation to a totally different class of phenomena. What Solomon and Corbit (1973) were concerned to understand were sequences of emotional states, or animal behaviors that imply emotional states, and they found the opponent process concept a fruitful one to pursue in their attempts to relate a variety of behaviors associated with learning, habituation, and addiction. As one example from the animal learning area, dogs given shock react with a set of responses that typically suggest a state of unpleasant emotional arousal. When shock terminates, another set of responses occurs that imply what might be called pleasurable relief or perhaps even joy. With longer periods of exposure to the shock stimulus, the intensity of the initial, unpleasant-affect behavior diminishes, but what might be called the relief syndrome after shock termination shows no comparable decrement in the intensity of its expression. The failure of the primary response and the aftereffects to build in intensity or wane together seems paradoxical at first, but it follows naturally if, by analogy with the visual situation, what is happening during the primary stimulation is (*a*) a direct response appropriate to the direct stimulus and (*b*) an opponent neural response that builds in time in such a way as to restore the system to a new equilibrated state. In the visual situation the image fades more or less during continued exposure of the eye, but the afterimage seen when the eye is relieved from the continued stimulation increases in strength with the duration of the primary stimulus, just as the dog who whimpers less with continued exposure to shock still wags his tail at least as vigorously when the noxious stimulation is terminated. For many other examples, and the potential application of the opponent process concept to the problem of drug addiction, we can better refer you to Solomon and Corbit (1973).

The opponent principle can be seen as operating not only as between states of nervous activity and between inputs at the cellular receptive field level, but also as between areas of the brain that seem to be mutually and reciprocally related in the control of specific behavior. Sprague (1966) discovered such an organization in the control of visually guided behavior in the cat. In this instance, removal of a large area of visual cortex on one side of the brain resulted in functional blindness in a large part of the animal's visual field. Additional removal of more brain tissue from the superior colliculus on the opposite side of the brain had the surprising result, not of increasing the visual disability, but rather of restoring to a large extent the functional efficacy of the presumably blind part of the visual field. And it is not hard to find the general principle of balanced control also operating in the behavior of human patients who have suffered serious brain damage. A case that we personally observed this past spring gave evidence of an aphasic *anomia*—loss of ability to produce names of familiar objects and persons—after severe temporal lobe damage. During the acute recovery stage, the patient talked volubly and excessively, and the anomia was extreme. As time passed and the spontaneous verbal speech flow diminished, the anomia also began to show marked improvement. As Oscar Marin, the clinical neurologist in the case pointed out to us, it was as if the recovery of an inhibitory control in the regulation of the speech behavior was necessary to restore the quality of the output and to release the verbal retrieval processes once again.

By citing these few examples from the recent literature and from our own recent observations, we hope we have not given the impression that we believe the principle of opponent processes to be a newly discovered one in behavior or in physiology. We are, of course, well aware of the history of this general concept in human thought, whether in Hegelian philosophy, the dialectical materialism of Marx and Engels, Pavlovian excitation and inhibition, Sherrington's reflex control of muscle antagonists, matter and antimatter in contemporary physics, or the fundamental Yin and Yang principle that permeates the Oriental point of view. A general principle, universally applied, easily becomes a useless cliché. But our point here is that

the opponent process concept, used as a guiding principle in analyzing specific aspects of particular psychological phenomena, may continue to provide the most useful key to the behavior of the nervous system, as it has already proved to do in the analysis of particular visual phenomena.

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Editorship Nomination

The APA Publications and Communications Board invites nominations for the editorship of the *Journal of Counseling Psychology* for the term running 1976 through 1981. In order to provide a year of overlap with incumbent Ralph Berdie in 1975, the Board will appoint an editor-elect this year. Members wishing to suggest candidates should prepare a brief statement of one page or less in support of each nomination and mail no later than March 1, 1974, to Anita DeVivo, Executive Editor, APA Journals, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

The Semantics of Children's Language

PATRICK SUPPES *Stanford University*[1]

Interest in language, especially that of children, has become a central concern of contemporary psychology. This interest is somewhat surprising, because everyone recognizes the enormous complexity of language use and language development in the child. In spite of a widespread recognition that there is little hope of having a complete theory of these matters at any time in the near future, a general air of activity and in some cases of optimism prevails about the progress being made. For example, it is now possible to cite a fairly large number of references in the literature on almost any aspect of language development in the child, ranging from phonology to semantic comprehension, with probably the largest number of studies being on the syntactical or grammatical development of children's language. It is surely in this area that the most progress has been made over the past decade or decade and a half.

I have been involved in several such grammatical studies myself (Suppes, 1970; Suppes, Smith, & Léveillé, 1972), and I believe in the intrinsic merit of the diversified work that is going on all over the world. On the other hand, I do feel that too great an emphasis has been placed on grammar or syntax, and too little on semantics. I want to try to help redress the balance by making the case for an intensive study of the semantics of children's speech. I shall be especially concerned to argue that there are as many conceptual and technical tools available for semantical analysis as there are for syntactical analysis, but as yet these tools are less familiar to psycholinguists and to psychologists in general than is the concept of a generative grammar. Moreover, psychologists have developed a habit of listening to the latest word from linguists on syntactical matters, and to some extent this has transferred to semantics. Part of my thesis is that while linguists have had little of interest to say about semantics, there is a deep and conceptually rich tradition in logic and philosophy that can be used for the semantical analysis of children's speech and that can provide a body of methods and concepts appropriate to the task. It is not my objective to be controversial and dialectical in this lecture, but I want to emphasize from the beginning my conviction that when it comes to semantical matters, the tradition of model-theoretic semantics that originated with Frege (1879) in the nineteenth century is *the* serious intellectual tradition of semantical analysis, and the recent offshoots by linguists, to a large extent conceived in ignorance of this long tradition, have little to offer in comparison.

Logical Tradition

The tradition I identify as beginning with Frege—although it has an informal history that runs back at least to Aristotle—is concerned with providing a precise and explicit analysis of the meaning of an utterance. This is not the place to give a history of these developments, but I do want to sketch the central concepts that are particularly relevant to the analysis of children's language. It came to be recognized fairly early that one way to analyze the meaning of an utterance is to state under what conditions the utterance is satisfied. In the case of declarative sentences, this amounts to giving truth conditions; in the case of questions, conditions on a correct answer; and in the case of commands, conditions on a response that satisfies the command.

Perhaps the most important step after Frege was the explicit analysis of the concept of truth by Tarski (1935) in his celebrated monograph. Tar-

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[1] This article was delivered as a Distinguished Scientific Contribution Award address presented at the meeting of the American Psychological Association, Montreal, Canada, August 1973.

The research reported here draws upon collaborative work with a number of younger associates, especially Elizabeth M. Gammon, Madeleine Léveillé, Freeman Rawson, Robert L. Smith, Jr., and Peter Wu. The work has been supported in part by National Science Foundation Grant NSFGJ-443X and in part by Office of Naval Research Contract ONR N0014-67-A-00112-0049 to Stanford University.

Requests for reprints should be sent to Patrick Suppes, Institute for Mathematical Studies in the Social Sciences, Ventura Hall, Stanford University, Stanford, California 94305.

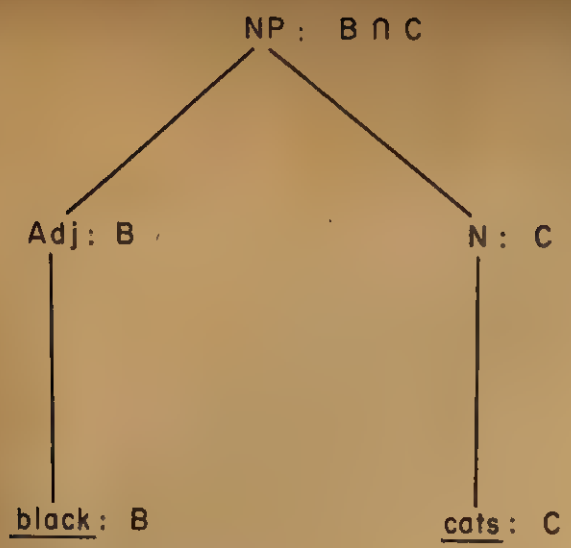

Figure 1. Semantic tree for the English noun phrase *black cats*.

ski's analysis of the concept of truth is restricted to formal languages, and much of the development of semantics in logic and philosophy since the 1930s has been concerned with formal languages. No doubt the preponderance of emphasis on formal languages has put off deeper perusal of these matters by psycholinguists who might otherwise be attracted to the theoretical developments. What has come to be called the theory of models in logic, which is really the semantical theory of formal languages, has been relatively inaccessible to outsiders until fairly recently.

As I have already indicated, it is my contention that the concepts developed in this logical tradition are of central relevance to the semantics of children's language. I want to sketch why I think this is so, but also to emphasize that I do not claim that all the intellectual problems that must be faced in giving an adequate theory of the semantics of children's language can be solved by any simple and direct application of the concepts taken from the logical theory of models. I shall have more to say about this later.

The basic semantical notion is that of a sentence or a collection of sentences being satisfied in a model. The intuitive idea of such satisfaction is close to the intuitive concept of truth.

Thus, when Nina (one of the children I shall be quoting extensively in this article) says "Bring me some more candy canes," the semantics of satisfaction of that command is, it seems to me, as straightforward as satisfaction of the simple mathematical command "Add 5 and 7." We all have a clear intuitive understanding in both cases of what satisfaction of the command is and what the appropriate state of affairs would be in which either command would make sense with its satisfaction being describable in relatively simple terms.

To make these intuitions explicit is much easier in the case of elementary formal languages. Their syntax is restricted to sentential connectives, predicates, variables, the universal and existential quantifiers, and parentheses for punctuation. A model for a sentence of such an elementary formal language consists just of a nonempty domain, a subset of that domain for each one-place predicate, a binary relation for each two-place predicate, and so forth. A simple, but explicit, formal definition of what it means for a sentence to be satisfied in such a model is then easy to give.

Once a concept of a model is introduced for an elementary formal language, the most important semantical relation between sentences, that of logical consequence, is easily defined. One sentence of the language is a logical consequence of a second just when the first sentence is satisfied in any model in which the second is satisfied.

Direct application of these ideas to a child's language encounters at least three sorts of difficulties. First, the syntax of the child's language is more complicated than that of elementary formal languages, even in the case of a child between the age of two and three years. Second, the idea of considering *any* model of a child's utterance seems on occasion too broad and leads to a notion of consequence that seems too general. Third, the sentences of an elementary formal language are self-contained in their meaning relative to a model that can be easily described. In the case of much of a child's speech there is a vagueness of context and at the same time an apparent high dependency on context that is not easily dealt with by any explicit notion of model. I want to say something about each of these difficulties in turn.

Semantics of Context-Free Grammars

The approach to the first problem of syntax is to restrict initial analysis to that part of the child's language for which a context-free grammar can be written. The semantics of such context-free languages or context-free fragments then consists essentially of two parts. The first part is the assignment of a denotation to individual words or phrases occurring in the child's speech. In many instances, this assignment of denotation would of

course vary from one context of use to another, but this variation merely corresponds to using different models on different occasions. Thus, when Nina says "Get big ball," we assign the denotation of a certain two-place relation to *get*, we assign the set of balls in the immediate environment as the denotation of the word *ball*, and we assign a denotation to the adjective *big* that is not simply the set of big things in the environment, but something more complicated that I shall discuss later in a more detailed consideration of adjectives.

The second part of the semantics is to provide rules for putting the individual denotations together in a way that fits hand and glove with the syntactic structure of the utterance by assigning a semantic function for combining denotations to each production rule of the grammar. This may sound somewhat abstract and complex, but the intuitive idea is simple, as may be illustrated in some examples drawn from a second corpus, a group of six-year-old black children talking to each other (these children were born and live in Southern California). One of the children said "Black cats could climb up a ladder." Let us look at the subject noun phrase *black cats*. Our grammar for noun phrases contains the production rule NP → Adj + N. As shown in the tree in Figure 1, the semantic rule attached to the grammatical rule is simply that of intersection for this simple noun phrase. On the left-hand side of each node we show either a terminal word or a grammatical category, and to the right of the colon in each node, the denotation of that node. Thus, in the present case the denotation

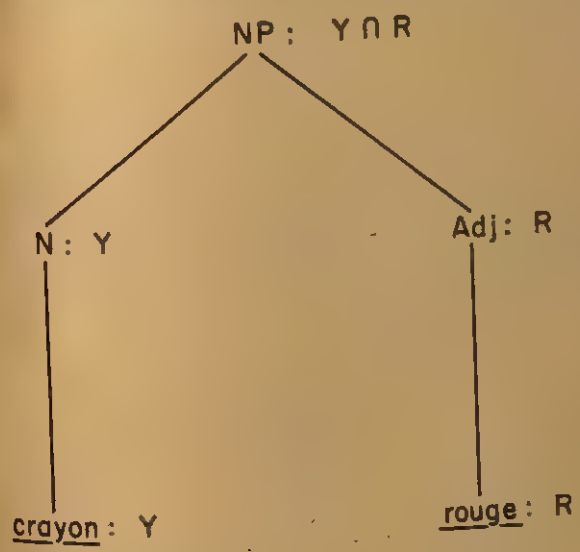

Figure 2. Semantic tree for the French noun phrase *crayon rouge*.

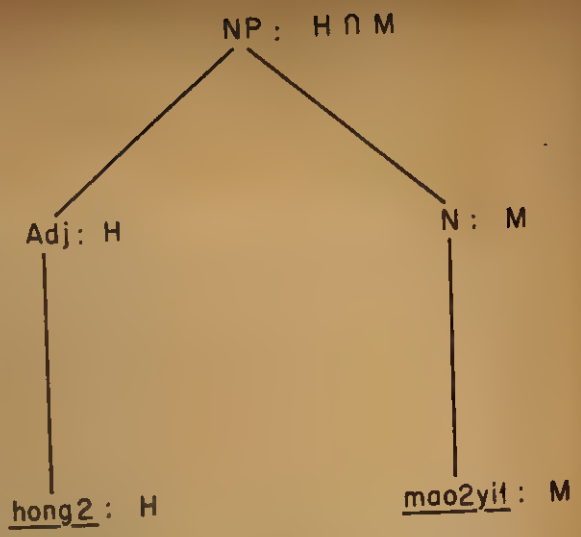

Figure 3. Semantic tree for the Mandarin noun phrase *hong2 mao2yi1*.

of *black* is the set B of black things, and the denotation of *cats* is the set C of cats. Using the semantic rule of intersection, we see that the denotation of NP as shown in the tree is simply $B \cap C$.

A French sample of a similar sort, taken from the corpus of Philippe, a young Parisian child, indicates that the semantic tree for a noun phrase looks very similar, even when the adjective follows the noun, due to the fact that intersection of sets is commutative (see Figure 2). In the simple case under consideration, similar semantic trees can be found in languages quite different from English. Here is an example drawn from the spoken speech of Lingling, a two-year-old child whose language is Mandarin and who lives in Taiwan. The semantic tree for *hong2 mao2yi1* (red sweater) is shown in Figure 3.[2]

The three examples I have given are exceedingly simple. It is no triumph for any theory to be able to give an analysis of them. On the other hand, it is not appropriate here to undertake a full-scale systematic analysis of the speech of any of the children referred to. Extensive technical reports already exist for several of the corpora, and some of the analysis is so detailed and massive that I wonder if the reports will have any readers besides the authors themselves (Smith, 1972; Suppes, 1971; Suppes & Gammon, 1973; Suppes, Smith, & Léveillé, 1972).

[2] Examples drawn from the Lingling corpus are written in pinjin notation for Mandarin with tones indicated by the numerals 1–4 to make possible linear processing of computer input and output.

To provide illustrations that I think are suggestive of the direction taken by the full-scale analysis, I would like to consider four topics: the definite article, adjectives, quantifiers, and the expression of propositional attitudes in children's speech.

THE DEFINITE ARTICLE

In English the definite article is not used at the very beginning of connected speech by young children, but it does appear early. In the case of Nina, shortly after she was two years old frequent uses are found: *the little boat, the little baby, I want the next page, I don't want the finger, I want the lion.*

The use of the definite article is even more common in spoken French at the age of two or shortly thereafter. Here are some of Philippe's examples: *pas chercher les voitures, dans la main, tombé le nours, la voiture, va chercher l'avion, tombé sur la bête, de l'eau, plein sur la cuisse, mangé le yaourt, le train, le métro, la valise, les chaussons là, chercher les livres, faire de la musique sur le nôtre.*

Because there is no exact equivalent of the definite article in spoken Mandarin, I shall only consider the semantics of English and French. This does not mean that there is no way of giving definite descriptions in Chinese; rather, it means that different semantical and syntactical methods are used.

There is a familiar story in logic and philosophy about the analysis of definite descriptions and the introduction into formal logic of an explicit notation for the definite description operator. One of the classics of twentieth-century philosophy is Bertrand Russell's (1905) famous article on definite descriptions in which he shows how such descriptions can be eliminated in favor of more primitive logical concepts. His classic example is "The present king of France is bald." For analyzing children's speech or the informal talk of adults, the kind of analysis provided by Russell and the subsequent logical literature on definite descriptions does not seem to provide exactly the right approach, because the objective of the kind of analysis that deals directly with the spoken language is, as I have indicated, to provide a semantics that fits hand and glove with the syntax.

For this purpose, I have found that in many cases, but perhaps not all, it is useful to let the definite article *the* in English (and usually the corresponding definite article in French, for example) denote a set C that is the union of the set of objects in the perceptual surround, with the set of objects denoted by phrases in immediately preceding sentences in the conversation, and in some cases also the set of objects denoted by images or symbolic storage in long term memory. Thus, when Nina said, "I want the lion," the set C denoted by *the* is intersected with the set of lions both real and toy, and if everything is in good order, the set intersection consists of a unique object. The set C, as I have described it, might in some cases be too large and thus the intersection would be too large. One device by which we can narrow the set C is to use a sequence of sets C_i with each element of the sequence being contained in the preceding member. The C_i that is appropriate in a given case is the first one that leads to a unique object when intersected with the set denoted by the remainder of the noun phrase. Without entering into technical details, I do want to remark that in the case of the definite article the correct semantic function for the production rule is not simply intersection, but intersection only in the case when everything is normal. When it is not, some Fregean or Russellian device is used. Obviously, also the analysis must change when the definite article is used with a plural noun phrase, as when Nina says, "I want the big boxes." In this case, the semantic function places a different cardinality requirement. Rather than a unique object in the intersection there must be at least two objects for the normal meaning to go through.

Whether the explicit decision is taken to let the definite description denote when the proper number of objects is not found in the intersection is really a technical decision and not an important conceptual one. One alternative is to treat the phrase as meaningless and nondenoting, and another is to introduce some arbitrary abstract object as Frege suggested. What is interesting is that the decision here is about as arbitrary and conventional as it is in the case of highly formalized languages, for example, as in a formalized language for axiomatic set theory.

To give you a crude idea of how the set C works in the analysis of the spoken language of the Southern California black children I mentioned earlier, Elizabeth Gammon and I looked at the 4 [illegible] noun phrases in the corpus (Suppes & Gammon, 1973). It was our approximate judgment after we examined the 363 uses of the definite article that in 120 cases the set C could be restricted to the perceptual surround. In [illegible] cases the set C could be restricted to objects denoted in immediately pre-

ceding sentences, and in 28 cases familiar objects like the moon, whose image or symbolic description was stored in long term memory, seemed appropriate. In 12 cases the object referred to seemed to be in the mind of the speaker alone, and there was no easy extraction of the appropriate set.

To give a sense of the frequency of occurrence of the definite article in the case of Nina, her speech recorded periodically from the age of 23 months to 39 months, with a corpus consisting of 102,340 tokens, contains 4,144 occurrences of *the*. Thus a requirement of any systematic semantics of Nina's speech is to provide an analysis of the definite article.

In the case of Philippe's spoken French, running from the age of 25 months to 39 months, there are 56,982 tokens. *le* occurs 1,641 times, *la*, 1,457; *l*, 619; and *les*, 777 times.

Of course, the semantics of the demonstrative adjectives is very close to the semantics of the definite article. I shall not consider the ways in which we think they should be given a somewhat different semantics from that of the definite article to indicate continuing focus (*this* in English) or change of focus (*that*). In the case of Nina, there are 2,075 occurrences of *that*, 1,497 occurrences of *this*, 246 occurrences of *these*, and 541 occurrences of *those*.

SEMANTICS OF ADJECTIVES

I turn now to some semantic subtleties involved in the use of adjectives. Native speakers of English will say almost without exception *big red book*, *little blue box*, and so forth, but not *red big box* or *blue little box*. There is a good semantic explanation of this fixed order, and the order is found almost without error in young children's speech from the very beginning of the production of phrases that use an adjective of size as well as an adjective of color or some other adjective of simple classification. In the case of Nina, there are 170 occurrences of *big* and 674 occurrences of *little*. Here are all the instances in which either *big* or *little* is combined with an adjective of classification (there are other instances of intensifiers, for example, *great big*, that I shall not examine): *big black; and a big white one, and there a big white one; big black coal, big dirty in her big mouth, big tiny mouth; yeah my big blue dog. I want the little tiny baby; little tiny flower, the little tiny one; and my little soft monkey, I get this pink little blanket* (the only case of reversal of the fixed order mentioned above), *I want my little white blanket, little green table, Mrs. Wood's little green house; Mrs. Wood's little green house, where's the little tiny oval; where's the more little green chair?, where's the little green chair fell down?, yup that's my little washing machine like you have a washing machine downstairs.*

In the case of standard spoken French, the proper order is the adjective of size preceding the noun, which is followed by the adjective of classification. Here are a few samples from Philippe's speech: *chercher deux petites cabanes bleues; où il est le petit filet jaune?, une petite table carrée, une petite fenêtre ronde, après il a mangé le petit chaperon rouge; et un petit nez noir; le petit bateaux bleu; les petits bouts ronds là; le petit bateaux bleu où il est?, la voiture elle fait tomber la petite machine verte, c'est quoi les petits bouts ronds?, c'est quoi ces petits bouts ronds là?, tu vois les petits ponts jaunes?, tu vous un petit raisin rouge?* (*pour voir*), *moi je prepare des petites cabanes bleues.*

In the case of Jinglings Mandarin, the word order is similar to that of English: that is, the adjective of size or of some other characteristic of variable intensity precedes the simple adjective of classification. Again, as in the case of Philippe, I have not attempted to present a full listing, but only to give some examples: *xiao shi shen ma* (small real prince), *xiao shi shu lao hu* (small real rat), *xiao ai ren* (small short man), *xiao bai tu* (small white rabbit), *da bai e* (big white goose), *da yu bengl* (big wild ox), *da mu ji* (big female chicken).

The same regularity is exhibited almost without exception in the speech of the youngest old children from Southern California. What is impressive in these last sample corpora is the uniformity with which the standard usage is exhibited even in quite early stages of a child's speech. It might be argued that the problem is simpler in French. Still impressive is the way in which Philippe uniformly puts *grand* or *petit* before the noun and adjectives of color not before or somewhere after the noun.

Let me illustrate, with an artificial example, what I think is a reasonable explanation of the underlying semantics and why we place the adjectives of variable intensity prior to the adjectives of classification. In Figure 6 we see a set of dots in the top row marked 1, 2, 3, 4, 5, 6, 7, 8. If we ask for the derivation of big dot we get the set of the single large green dot. If we ask for the derivation of the red big dot we get a more complex derivation, because we first start with big dot

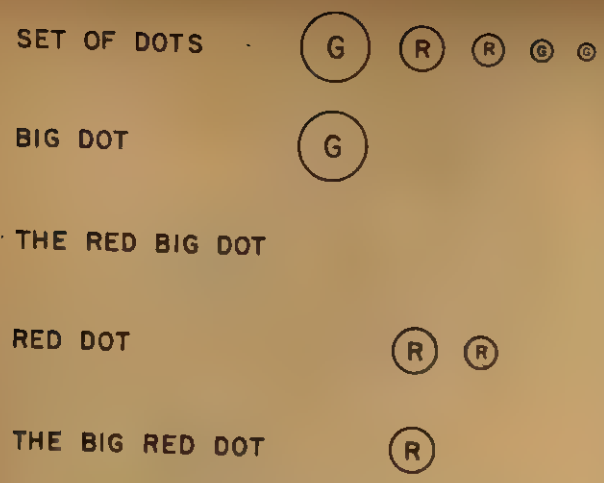

Figure 4. Denotations of various noun phrases illustrating the order of modifying adjectives.

which is green, and then ask for red, but there is no big dot that is red.

On the other hand, if we start the other way and ask for the denotation of *red dot*, we get the two red dots. Then if we ask for the big red dot we apply the intensive adjective *big* and select the larger of the red dots.

The idea behind this can be expressed in abstract terms in the following way: First, we intersect the set denoted by the noun with the set denoted by the adjective of classification. We order this set according to the ordering relation denoted by the adjective of intensity, in the present case, the adjective *big*. If we are talking about the upper end of the ordering, we then select as the denotation of the noun phrase the single largest object or something close to the single largest object, for example, a small subset. It is easy to make all this quite precise in terms of a few mathematical symbols, but that is not my objective here.

For those who are somewhat uneasy about the absolute definiteness of my example in terms of dots, they can substitute *biggest* for *big* and obtain a quite unambiguous analysis. Thus, in the second line we talk about the *biggest dot*, in the third line the *red biggest dot*, which is most unusual English, and so forth. I should mention that I have not analyzed the example in terms of the completely semantically unambiguous superlative, because the use of the superlative in children's language is relatively rare. As already mentioned, there are 370 occurrences of *big* in the Nina corpus and 674 occurrences of *little*. In contrast, there are just five occurrences of the comparative *bigger* and three of the superlative *biggest*, and just three of *littler*, none of *littlest*, none of *smaller*, and one of *smallest*. When I have spoken of the ambiguity of *big* I mean that exactly what initial segment of the objects ordered by size is selected as the denotation will vary from one circumstance to another. I do think the analysis in the present case that sharply differentiates the semantic function associated with an adjective like *red* as opposed to an adjective like *big* is in first approximation correct and represents a useful insight into the subtleties of the use of adjectives in English. Let me mention also that there are cases, and they are proper cases, of usage in English in which the order is reversed. For instance, if someone is looking at two large chairs among a collection of chairs and one of them is green and one of them is red, it is appropriate to say, Give me the green big chair.

Further complex things about the semantics of adjectives are encountered in children's speech. For example, there are 260 occurrences of *another* in the Nina corpus and 227 occurrences of *other*. The semantics of these two adjectives is obviously not in any sense the simple intersection function. Cardinal numbers and their different usages and positions raise additional problems, but here there is a longer tradition and therefore a more straightforward sense of what is correct.

As fascinating as these further details about adjectives can be, in order to give a survey I move on to the special case of quantifiers, which can be treated as adjectives, but in my own analysis should not be.

QUANTIFIERS

To keep the story simple I restrict myself only to the classical quantifiers *all* and *some*. And here is Nina at her classical best: *all colors, all of they gonna go in here; all the animals did; all the ducklings going to the California; all these and these children are gonna eat; mommy I need all the pieces cut; and I got some lambs; and I want some people; and I made some pancakes; and some snakes were wrapped up together.*

It is my contention that the meaning of *some* and *all* in these examples from Nina's speech is almost without change the same as in classical mathematical language when we say *All even numbers are divisible by two* or *Some prime numbers are twin primes*, or as another parallel from the classical examples of Aristotle: *All men are mortal,*

some men are tall. At the age of 30 months Nina was handling quantifiers with beautiful precision. Compare this fact. It was not until 1879, upon the publication of Frege's *Begriffschrift*, that an explicit theory of quantifiers was formulated. Today, the theory of quantification, in the sense of the theory of the universal and existential quantifiers, is taken to be at the heart of general logic. I have even heard one paper by a philosopher in which the theory of predication and quantifiers was taken to be the most distinguishing characteristic of human as opposed to nonhuman, animal thinking. I am pleased to report that Nina and other children like her seem to have these matters under good control at a very early age.

There are two aspects of Nina's use of quantifiers that I think are representative of the use of a large number of children, but which receive no treatment in Aristotle and very little in the logical tradition from Frege onward.

The first is that both the existential and universal quantifiers, but especially the existential quantifier, occur more in object position than in subject position. The object position for quantifiers is especially natural in stating commands or expressing wants or needs, and it is exactly the logic of such statements that has been ignored until recently in the development of explicit logical theory. Three examples that have not yet been cited, but that would be typical of Nina's use of the existential quantifier in object position would be these: *I want some diaper pins; I will get some blocks; I want some more toys.*

The second point is that the universal quantifier is so often used in conjunction with the definite article. There is a kind of concreteness and contextualism about this, which is especially reinforced by the earlier analysis given of the definite article, that makes this the natural way to talk, rather than using the universal quantifier in unrestricted form. Thus when Nina says *all the animals did,* she is referring to the toy animals in the immediate context, and the definite article *the* makes that clear. The same kind of remark applies to her statement about all the ducklings. An example in which she may use a demonstrative adjective rather than the definite article is this: *All these clothes are getting off.* The force of the definite article or the demonstrative adjective is to restrict the universal quantification to the contextual set at hand, and this is where a restriction properly belongs. Aristotle's or Frege's unrestricted universal quantifier can be defended in terms of logical simplicity, but for working purposes the canonical use of quantifiers is *all the xs,* not *all xs.* This has been too little remarked upon in logical theory, but is evident enough in Nina's speech.

One final remark is needed on Nina's use of *all* and *some.* I have not covered the many different uses of these words as adverbs or parts of adverbial phrases, but in relative frequency these uses are as common as the direct use as quantifiers. In almost all cases, however, the semantical function of the words is similar to their straight function as quantifiers. I have deliberately avoided defining explicitly the semantic functions associated with the production rules by which quantifiers are introduced, not because I think the subject is unmanageable, but because the explicit treatment is rather technical. At least I do not yet understand how to present it in a way that is not technical. The semantics is simplest in my judgment when quantifiers enter at a high level in the derivation tree, and not at the level of adjectives that are part of noun phrases.

PROPOSITIONAL ATTITUDES

There is an important and fundamental distinction in logic that is not needed for the classical development of the foundations of mathematics, but that is essential for obvious distinctions in the use of ordinary language. This is the distinction between language that is purely extensional and language that is intensional. The most obvious way in which language is used intensionally is in the expression of necessity, possibility, or propositional attitudes. Examples of the expression of propositional attitudes are statements about beliefs, needs, wants, expectations, or fears. They are expressed by a wide variety of verbs in every natural language.

Because of the importance and interest in these matters in any complete analysis of the semantics of natural language, let me give one example that is classical in the philosophical literature, but is not directly pertinent to Nina's speech or that of other children her age. The example concerns the non-truth-functional or nonextensional character of belief statements. Thus, from knowing that the statement *The earth is flat* is false, we can infer neither the truth nor falsity of the statement *John Jones believes that the earth is flat.* Similarly, from the truth of the statement *The sun is larger than the earth* we can infer neither the truth nor the falsity of the statement *Aristotle believed that*

the sun was larger than the earth. I have no direct analogues to these examples in the speech of Nina, because no form of the verb *believe* or the noun *belief* occurs in our extensive sample of her speech. As you would also expect, there are no instances of statements of possibility or necessity. There are, however, numerous instances of the expression of propositional attitudes, especially expressions of wants or needs, and there are many expressions of *because* or *-cause*.

The case of the various forms of *want*, for example, *wanna*, is striking. Of the large corpus of over 100,000 words, somewhat over 1% are occurrences of some form of *want* (*want*, 883; *wanna*, 359; *wants*, 60; *wanted*, 6).

One useful distinction in the use of terms or noun phrases in the expression of propositional attitudes occurs already in Nina's speech. This is the distinction between attributive and referential use of terms, a distinction already recognized by medieval logicians who characterized the referential reading as giving us a statement *de re* as opposed to the attributive reading which yields a statement *de dicto*. Both attributive and referential uses occur in the Nina corpus. Here are some examples of attributive uses: *He don't want a Band-aid on; I want milk; I want some more toys; I want another story OK;* and here are some examples of the referential use: *I want the next page* (the next page, of course, having a definite reference or denotation); *I want this* (with *this* having reference to a particular object in the environment); *I want her to wear the blue dress* (where the phrase *her to wear the blue dress* refers to a definite concrete act).

In the examples I have cited there seems to be little ambiguity between attributive and referential use of the noun phrases in object position. In the philosophical literature on these matters there has been considerable discussion of the kinds of examples that do give rise to ambiguity. A typical instance would be: John wants to marry a French girl. Given only this sentence, it is ambiguous whether the reference is to some one particular girl or whether John is looking for a wife, does not have a definite person in mind, but wants her to be a woman who is French. Such examples can also occur with the use of the definite article as well as the indefinite article. For example, Hintikka (1973) cited "John believes that the richest man in town is a Republican." It is ambiguous whether John has in mind some particular individual or simply believes it true that whoever is the richest man in town, he will turn out to be a Republican.

I have examined Nina's use of *want* for the same kind of ambiguity. It exists with the indefinite article, but so far as I can see not with the definite article. Here are several examples with the indefinite article: *Do you want a pink balloon?; her don't want a cup; her want a blanket OK; I don't want a jersey on; I want a toy.* In all these cases, as Hintikka rightly remarked in his own analysis of a different range of examples, additional factual information, in our case the kind of information available in the corpus from preceding sentences or comments on the situation, makes clear whether the usage is attributive or referential.

The question of theoretical interest is what uniform account semantically is to be given of these matters. For example, can the kind of model theory characteristic of elementary formal languages, which are normally extensional, be extended to the kind of language expressing propositional attitudes as found in Nina's speech? If I had asked this question 30 years ago, the answer would almost certainly have been pessimistic and rather negative, because the semantics of modal concepts or propositional attitudes had scarcely been developed. Fortunately, an intense concern with these matters within logic proper in the past three decades has created a considerable logical apparatus, and a deep understanding of the semantical problems involved has developed. In saying this I do not mean to suggest that all the conceptual problems are solved or that the application of current theory to children's speech is simple or straightforward. I do mean to suggest that many powerful and subtle methods are available and can be applied.

The most important theoretical point in these developments is that the semantics of sentences expressing propositional attitudes can be given a precise treatment in terms of the concept of a set of possible worlds.

To illustrate the idea of a set of possible worlds, let me first consider some classical modal concepts before turning to Nina's speech. If I say "It is possible that it will rain tomorrow," then the semantics of this statement is that there is a possible world in which it will rain tomorrow. If on the other hand I say "It is necessary that an object that is red is colored," then I mean that in every possible world this state of affairs must hold. On the other hand, if I say "It is contingently true that Bertrand Russell lived to be more than 90

years old," I mean that in some possible worlds this assertion would be true and in others it would be false. When Nina says "I don't want a jersey on," one interpretation of the semantics of this is that there is a possible world in which Nina does not put a jersey on, and it is in this world that she wants to be. (When I say "one world," I mean usually a set of worlds having this characteristic.) In general when Nina says "I want X," the semantics can be given a precise interpretation in terms of her desire for the actual world to be drawn from a certain set of possible worlds, and she is asking for actions that will make this take place.

(A technical remark is in order about these matters. A tradition exists in the logical and philosophical literature that defines a proposition as a function from possible worlds to truth values. This, for example, was the definition preferred by the late Richard Montague. On the basis of this kind of analysis, we would say that Nina's statement of a want or need expresses a proposition that is a function from possible worlds in which the want is satisfied to truth values. For various reasons, I do not like this analysis. For instance, one of the difficulties of Montague's view is that any two logically equivalent sentences express exactly the same proposition. Rather, I prefer that the structural characteristics of a sentence be an integral part of its expression of meaning. The identification of an utterance expressing a want with the function mapping possible worlds into truth values ignores the grammatical structure of the utterance. On the other hand, I emphasize that in the semantic analysis of such utterances expressing wants the simple theory of denotation discussed earlier is not satisfactory, and a more complicated theory building on the theory of possible worlds is required. For example, once we pass from a single fixed world or model to a set of models or worlds, simple adjectives like *red* no longer denote a set, but at the very least a set in each possible world or, put another way, a function that is a mapping from possible worlds into sets of objects in that world, in each case the set of objects being the set of red objects in that world.)

At this point many of you may feel that I have pulled you a long way from psychological questions to philosophical questions that seem to have little relevance to Nina's thoughts, actions, and language. I wish to urge upon you the thesis that this is not at all the case. Psychologists have mainly ignored the complexities and subtleties of the expression of propositional attitudes by young children even at their earliest stages. There may be a simpler way of giving a full-scale analysis of these matters, but if there is, it is not known to me, and it is certainly not widely available in the current relevant literature on these matters. What I have hoped to convince you of, and I think in the limited space available I have not been able to do the job adequately, is that already a host of methods and subtle distinctions are available, which we may effectively use for a better understanding of the explicit semantics of children's speech. What I have tried to make evident in my discussion of propositional attitudes is this: Even from the very beginning of a child's speech, in the age from two to three years, the whole battery of semantical problems associated with propositional attitudes, a set of problems among the most subtle in current semantical theory, arise in providing a detailed and accurate theory of children's speech.

Consequence and Paraphrase

In my introductory remarks I mentioned the desirability of changing the classical logical notion of consequence, and I now turn to this topic. In order to give the discussion a focus, I shall relate my remarks about logical consequence and a wider concept of consequence to psycholinguistic discussions of paraphrase and how problems of paraphrase arise in the analysis of the semantics of children's speech.

Frege's definition of paraphrase is that two sentences are paraphrases of each other just when they have the same logical consequences. If we use the characterization of propositions stated above, that is, a proposition is a function from possible worlds to truth values, then we may also characterize in equivalent fashion two sentences as being paraphrases of each other just when they express the same proposition. It is surprising to find this semantical characterization of paraphrase essentially unreferred to in the psycholinguistics studies on the paraphrasing ability of children or adults.

For example, in the careful and extensive study of Gleitman and Gleitman (1970), *paraphrase* is characterized as a notion that is properly thought of as a part of generative grammars, but this seems to me a clear error. Gleitman and Gleitman emphasized that transformations of a sentence can express paraphrases, but in the standard semantical Fregean sense, two sentences that have totally un-

related derivational histories can be logical paraphrases of each other. In practice, moreover, what is used in experimental tests of paraphrasing ability is really a semantic and not a syntactic criterion. The explicit use of model-theoretic semantics would put the experimental studies in this area on a sounder conceptual basis.

Nevertheless, I think there is adequate ground for changing the Fregean definition, which is too stringent. When Nina says things that are ungrammatical to our adult ears, it is usually easy for us to paraphrase what she has said, even without knowledge of the context. Consider the following: *No, I wanna go to what that thing and see if I'm tall; no more presents he doesn't give me; now let me try other Rachel's sunsuit, mommy; I feel better my diaper rash.* On the other hand, the following sentence of Nina's might present some difficulty even within context: *I want to save this on with the same time.*

In paraphrasing Nina's speech or someone else's, we almost always assume without question certain background information and knowledge that is not included in Frege's strict definition of paraphrase in terms of logical consequence. As I mentioned earlier, within model-theoretic semantics it is easy to give a characterization of logical consequence; namely, one sentence is a logical consequence of another if the first sentence is satisfied in any model in which the second is satisfied. In other words, we consider all possible models. But, we can get a less strict notion of consequence by reducing the set of possible models to some smaller set in which basic intuitive knowledge is held rigid across all the models in the set. Two sentences can then be said to be paraphrases relative to this reduced set of models if they both are satisfied in exactly the same models of the reduced subset.

This reduced notion of consequence, which I shall call R-consequence, has other uses than in the treatment of paraphrase. It is also the appropriate concept, in my judgment, for the analysis of questions and answers in children's speech. In the age range I have been mainly discussing, children primarily answer questions by adults, and it is only a year or so later that the tables are turned and they begin asking the questions and expecting answers. For this early stage of answers or the later stage of questions, the concept of R-consequence provides the proper concept for characterizing semantically correct answers to a question. (Notice that I do not say *the* semantically correct answer, because in general any one of an indefinite number of paraphrases will be accepted as correct.) I return to the importance of questions and answers in children's speech when I discuss problems of verification.

I conclude this article with a discussion of three types of problems: problems of context, problems of process, and problems of verification.

Problems of Context

When Nina says "I need some more" or "I want some too," it is clear that a full semantical account of these utterances is dependent on the context of utterance. This dependence on context is characteristic not only of children's speech, but of much casual adult talk as well. It stands in sharp contrast to sentence tokens of a formal language that are self-contained and timeless in character, or even to mathematical statements or other scientific statements in ordinary language in formal textbook or treatise mode. The way in which the kind of model-theoretic semantics I am advocating can be extended to cover such matters of context has already been indicated in the treatment described for definite articles. I see nothing standing in the way of similar extensions to other problems of interpretation, as in the case of the two examples just cited.

On the other hand, elaboration of the context to give an adequate account of what Nina was perceiving or remembering at the time she was speaking is to extend in quite substantial ways the framework of classical semantics to include concepts and requirements that have been investigated in all their complexity for many years by psychologists. Putting together what we know about remembering and perceiving with the kind of semantic structure for language I have outlined is a formidable theoretical task that has yet hardly begun. It seems to me, however, that this is the direction in which we should try to take account of context and, in the doing, build a much richer psychological model of Nina's and other children's language behavior.

Problems of Process

Building such a model of remembering and perceiving takes us at once to the consideration of process. A proper criticism of the semantic theory I have outlined is that there are no serious considerations of processing. There is no temporal

analysis of the machinery children or adults use for semantic processing of sentences they speak or hear.

This absence of an explicit analysis of process has been brought home to me in a salient way in some of our own research at Stanford University in the past year or so. In attempting to understand these problems of process and how model-theoretic semantics can be applied to the actual production of speech, we have turned to the problem of implementing an ongoing question-answering system for out computer system. We have chosen a domain that has no requirements of context, namely, elementary mathematical language—with an emphasis on the natural language and not on the elementary mathematics. But even here we have found that substantial theoretical extensions have had to be made to the model-theoretic semantics as described here or as found in the standard logical literature in order to have a workable computer program for actual question answering. My younger colleagues, Robert L. Smith and Freeman Rawson, have now advanced this analysis of process a fair distance (see Rawson, 1973). Space does not permit me to compare this work with earlier work by William Wood, Terence Winograd, or other computer scientists concerned with creating computer programs of a similar sort.

One way of describing the situation is similar to what I said about the problem of context. It is not that model-theoretical semantics as such is wrong, but rather that it has to be extended in order to obtain a working model. Just as a theory of remembering and perceiving must be built into an adequate model of a child in order to give a full account of his speaking and listening, so the model-theoretic semantics of even so definite a subject as elementary mathematics must be extended to fixed and definite dynamic ideas about the order in which functions are called, the precedent procedure for processing of given functions, and in general, the many kinds of considerations that enter into the construction of a compiler or interpreter for a computer language.

Problems of Verification

As in psychological theorizing and experimentation in all areas, it is seldom the case that we can construct a theory that is adequate to account for all aspects of the psychological process under investigation. This widespread fact of theoretical incompleteness doubly holds for phenomena as complex as those of language. Without providing a full theory of remembering, perceiving, or internal processing we can still ask for empirical tests or verification of theoretical concepts such as those of model-theoretic semantics that I have been discussing.

What must be faced is that the verification of a semantic theory is more difficult and less direct than the verification of grammatical theory. For instance, if one writes a grammar for the corpus of a child's speech, it is possible in a direct way to say what percentage of the utterances in the corpus the grammar parses, and if probabilistic criteria are imposed additionally how well it generates utterances with approximately the same frequency as the frequencies observed in the corpus. Verification that the denotations assigned to terminal words or the semantic functions assigned to production rules of the grammar are correct is not amenable to such straightforward and direct attack. It is of course the case that in testing any theory there are areas of intuitive judgment that cannot be reduced to a formal algorithm of verification, and in many cases the semantic analysis given of the utterances of young children by the kind of apparatus I have described can receive widespread intuitive agreement as to the correctness of the analysis. All the same, the central problem of verification is a subtle one and is not easily handled. For example, in his dissertation, Robert L. Smith (1972) gave an extensive semantic analysis of the corpus of Erica. The Erica corpus consists of 27,922 words. Erica was about 32 months old at the time the corpus was collected and thus fits within the range of the corpora of Nina, Philippe, and Lingling. Smith has written a completely systematic model-theoretic semantics for Erica, assigning a semantic function to each production rule of the generative grammar he also wrote for the corpus. The total system is complex and difficult to comprehend in any simple way. The problem arises of how to test in a systematic way that his semantical analysis is correct. Intuition can be tested on each rule, but it is a strain on the viability of intuition when the number of rules is large and some of them are complex. So far as I can see, we have at the present time no direct way to test the correctness of such a systematic semantics. It is a genuine theoretical construction, and we must use classical methods of indirect analysis to test its validity. The difficulty of providing such a direct test is one of the reasons we have been involved in writing a question-

answering computer program for elementary mathematical questions and commands, because the intuitive agreement and understanding of such questions and commands is absolutely sharp and objective in character, and so the correctness of the underlying semantics is subject to a universally agreed upon test. To some extent, this same approach can be extended to the corpus of speech of a young child when the adult speech is recorded as well, for the question-answering pairs can be analyzed in similar fashion, and in many cases a highly objective criterion is possible for evaluating the semantic analysis of the question-answer pairs. All the same, a fair portion of the question-answering between adult and child is not subject to the same sharp test that obtains for elementary mathematical questions.

The other route is to carry only so far the analysis of spontaneous speech and to turn to experimentation for the detailed verification of semantical concepts and theories. It is my present view that this step is essential, and we shall not be able to collect the evidence to persuade the skeptic until comprehensive experiments that adequately test the kind of semantical ideas I have discussed today are performed. Such experimentation is not hopelessly complex and does seem feasible. There is already a useful tradition of experimentation with three- and four-year-old children, especially in the form of the creation of toy or puppet characters that talk and perform simple actions, and it seems possible to build reasonably sharp tests of comprehension into such situations. On the other hand, designing adequately structured experiments that elicit speech from two-year-olds to provide a clear test of their underlying understanding of what they are saying is not a simple matter. It is an area of methodology that badly needs development, and I call on those of you who are interested for support and help.

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In Retrospect
Forty-Six Years

CARL R. ROGERS *Center for Studies of the Person*
La Jolla, California [1]

From 1927 to the present time I have been a practicing psychologist. I have made diagnostic studies of children and have developed recommendations for treatment of their problems; in 1928 I developed an inventory of the inner world of childhood which—may Heaven forgive me—is still being sold by the thousands. I have counseled with parents, students, and other adults; I have carried on intensive psychotherapy with troubled individuals—normal, neurotic, and psychotic; I have engaged in and sponsored research in psychotherapy and personality change; I have formulated a rigorous theory of therapy. I have had 40 years of "teaching experience," fostering learning through both cognitive and experiential channels. I have engaged in facilitating personal development through the intensive group experience; I have tried to make clear the processes of both individual therapy and the group experience through recordings, demonstrations, and films; I have tried to communicate my experience through what now seem to me to be "countless" writings, tapes, and cassettes. I have played my part as a worker in professional associations of psychologists; I have had a continuous, varied, controversial, and richly rewarding professional life.

So it has occurred to me that there might be some interest in the question, What does such a psychologist think about as he looks back on close to a half century of study and work? It is to that question that I will address my remarks. What is my own current perspective on these years, thinking both about my professional life and its various periods of development and change?

An Astonishing Impact

I believe the major element of my reaction as I look back on my work and its reception is *surprise*. Had I been told, 35 or 40 years ago, of the impact it would have, I would have been absolutely unbelieving. The work that I and my colleagues have done has altered or made a difference in widely different enterprises, of which I will mention several. It turned the field of counseling upside down. It opened psychotherapy to public scrutiny and research investigation. It has made possible the empirical study of highly subjective phenomena. It has helped to bring some change in the methods of education at every level. It has been one of the factors bringing change in concepts of industrial (and even military) leadership, of social work practice, of nursing practice, and of religious work. It has been responsible for one of the major trends in the encounter group movement. It has, in small ways at least, affected the philosophy of science. It is beginning to have some influence in interracial and intercultural relationships. It has even influenced students of theology and of philosophy.

My work has, to my knowledge, changed the life directions and purposes of individuals in France, Belgium, Holland, Norway, Japan, Australia, New Zealand, and South Africa; in 12 foreign countries readers can find some of my work in their own language; if someone wishes to read a complete collection of everything I have written, he will find it —in Japanese. I look with utter astonishment at this long list of statements.

A TENTATIVE EXPLANATION

Why has my work had such a pervasive impact? I certainly do not attribute it to any special genius of my own, and most assuredly not to any far-sighted vision on my part. I give full credit to my younger colleagues throughout the years for their

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Requests for reprints should be sent to Carl R. Rogers, Center for Studies of the Person, 1125 Torrey Pines Road, La Jolla, California 92037.

expansion and deepening of my thought and work, but even their efforts do not account for this far-reaching influence. In a number of the fields I have mentioned, neither I nor my colleagues have ever worked, or been involved in any way, except through our writings.

To me, as I try to understand the phenomenon, it seems that without knowing it I had expressed an idea whose time had come. It is as though a pond had become utterly still, so that a pebble dropped into it sent ripples out farther and farther and farther, having an influence that could not be understood by looking at the pebble. Or, to use a chemical analogy, as though a liquid solution had become supersaturated, so that the addition of one tiny crystal initiated the formation of crystals throughout the whole mass.

What was that idea, that pebble, that crystal? It was the gradually formed and tested hypothesis that the individual has within himself vast resources for self-understanding, for altering his self-concept, his attitudes, and his self-directed behavior—and that these resources can be tapped if only a definable climate of facilitative psychological attitudes can be provided.

This hypothesis, so new and yet in a way so old, was not an armchair theory. It had grown out of a number of very down-to-earth steps.

First, I had learned through hard and frustrating experiences that simply to listen understandingly to a client and to attempt to convey that understanding were potent forces for individual therapeutic change.

Second, I and my colleagues realized that this empathic listening provided one of the least clouded windows into the workings of the human psyche, in all its complex mystery.

Third, from our observations we made only low-level inferences and formulated testable hypotheses. We might have chosen to draw high-level inferences and to have developed abstract, untestable, high-level theory, but I think my own earthly agricultural background deterred me from that. (Freudian thinkers chose this second course, and this marks, in my estimation, one of the most fundamental differences between their approach and the client-centered approach.)

Fourth, in testing our hypotheses, we uncovered findings regarding persons and relationships between persons. These findings and the theory that embraced them were continually changing as new discoveries emerged, and this process continues to the present day.

Fifth, because our findings have to do with basic aspects of the way in which the person's own capacities for change can be released and with the way in which relationships can foster or defeat such self-directed change, it was discovered that they had wide applicability.

Sixth, situations involving persons, change in the behavior of persons, and the effects of different qualities of interpersonal relationships exist in almost every human undertaking. Hence, others began realizing that perhaps the testable hypotheses of this approach might have almost universal application, or might be retested or reformulated for use in an almost infinite variety of human situations.

Such is my attempt to explain an awesome and otherwise incomprehensible spread of ideas which began with a very simple question, Can I, by carefully observing and evaluating my experience with my clients, learn to be more effective in helping them to resolve their problems of personal distress, self-defeating behavior, and destructive interpersonal relationships? Who could have guessed that the groping and tentative answers would spread so far?

PSYCHOLOGY'S AMBIVALENCE

You may have noticed an omission in the listing of the areas of impact of my work. I did not say that I and my colleagues have affected academic, or so-called "scientific," psychology. This was not an oversight. I believe an accurate statement would be that we have had very little influence on academic psychology, in the lecture hall, the textbook, or the laboratory. There is some passing mention of my thinking, my theories, or my approach to therapy, but, by and large, I think I have been a painfully embarrassing phenomenon to the academic psychologist. *I do not fit.* Increasingly I have come to agree with that assessment. Let me amplify.

The science and profession of psychology have, I believe, profoundly ambivalent feelings about me and my work. I am seen—and here I must rely mostly on hearsay—as softheaded, unscientific, cultish, too easy on students, full of strange and upsetting enthusiasms about ephemeral things like the self, therapist attitudes, and encounter groups. I have defamed the most holy mysteries of the academic—the professorial lecture and the whole evaluation system—from the ABCs of course grades to the coveted hood of the doctor's degree. I can best be handled by most writers on psychology in

one paragraph as the developer of a technique—the "nondirective technique." I am definitely not one of the ingroup of psychological acedemia.

The other side of the ambivalence is, however, even more striking. Psychology as a whole, science and profession together, has showered me with honors—many more, I believe, than I deserve. To my amazement I was awarded one of the first three awards for scientific contribution, and this was back in 1956 when I was much more controversial than I am at present. I had been chosen president of the American Association for Applied Psychology. I had been elected president of the American Psychological Association. I had been appointed or elected chairman of important committees and divisions, and these honors often touched me. Yet, never have I been so emotionally affected as I was by the scientific contribution award and its accompanying citation. When I was elected to an office it could have been partly due to my ambition, for I was ambitious to get ahead in my profession. But this award was to me, in some sense, the "purest" recognition I had ever received. For years I had been struggling to objectify knowledge in a potential field of science that no one else seemed to be concerned about. It was not ambition or hope of any reward that pushed me on. In the empirical research itself there was more than a little desire to prove something to others—clearly not a scientific goal. But in the basic phases of the work—the careful observation, the recorded interviews, the hunches as to hypotheses, the development of crude theories—I was as close to being a true scientist as I ever hope to be. But it was clear, I thought, that my colleagues and I were just about the only ones who knew or cared. So my voice choked and the tears flowed when I was called forth, at the 1956 APA Convention, to receive, with Wolfgang Köhler and Kenneth Spence, the first of the awards for a *scientific* contribution to psychology. It was a vivid proof that psychologists were not only embarrassed by me, but were to some extent proud of me. It had a greater personal meaning than all the honors which have followed, including the first award for professional contribution, given last year.

I did enjoy last year's citation, especially the honesty of the statement that I was a "gadfly" to the profession—only now that statement promotes me to the status of "respected gadfly." I liked that expression of the ambivalence.

Two Struggles

As I look back over the years, I realize I have engaged in two struggles which have professional significance.

STRUGGLE WITH PSYCHIATRY

The first struggle has to do with the determination of many members of the psychiatric profession that psychologists should not be allowed to practice psychotherapy, nor to have administrative responsibility over "mental health" work, especially if this involved psychiatrists. I first met this opposition in Rochester, New York, when our highly successful Child Study Department, a branch of a social agency, was being reorganized in 1939 into a new and independent Rochester Guidance Center. A vigorous campaign, partly aboveboard and partly behind the scenes, was made to discontinue my services as director and to substitute a psychiatrist. There seemed to be no question about the quality of my work. The argument was simply based on the view that a psychologist could not head up a mental health operation—it was simply "not done." Although we had employed psychiatrists on a part-time basis for years, that suddenly became something that was out of the question. I could not point to any important precedent, nor could I claim the support of any professional group. It was a lonely battle. I am very grateful to the board of directors, who were almost all laymen, for eventually deciding the dispute in my favor. It had been a life-and-death struggle for me because it was the thing I was doing well and the work I very much wanted to continue.

After an interim lull at Ohio State University, the struggle was renewed with even more vigor at the University of Chicago. Not one of the rapid succession of chairmen of the Department of Psychiatry was willing to cooperate with the unorthodox fledgling Counseling Center. Finally, one of these men demanded of the University administration that the Counseling Center be closed, since its members were practicing medicine (namely, psychotherapy) without a license. There was still no professional support for our activities from the APA or any other psychological organization. I mounted a blistering counterattack, with all the evidence I could muster. Again I am grateful, this

time to the chancellor of the University, for his (to me) fair-minded consideration and his suggestion to psychiatry that they drop their demand, which they did. These are the only two times I engaged in open combat with psychiatry. For the most part, my strategy has been twofold. I have endeavored to reconcile the two professions in their pursuit of a common goal. I have also tried to move ahead so rapidly and so far that the right of psychologists to practice in a field in which they were preeminent in research, and fully equal in practice and in theory building, could not be challenged.

But when pushed into a corner, as on these two occasions, I can fight with all the effectiveness that one develops in a family of six children. People who know only my thoughtful or gentle side are astonished at my attitude and behavior in a situation of all-out war. I should, in warning, have raised the banner of the early colonies, on which was emblazoned a rattlesnake and the motto "Don't tread on me!"

I am happy to say that at the University of Wisconsin my joint appointment in psychology *and* psychiatry was a pleasant resolution of these struggles. Indeed, I initiated the formation of a group of psychologists and psychiatrists who gradually defused an incipient legal and legislative battle which was splitting the two professions in that state.

STRUGGLE WITH BEHAVIORISTIC PSYCHOLOGY

The other struggle of my professional life has been on the side of a humanistic approach to the study of human beings. The Rogers–Skinner debate of 1956 is one of the most reprinted writings in the psychological world. It would be absurd of me to try to review that continuing difference in any depth. I will simply make a few brief statements as I look back over these years.

To avoid misunderstanding, let me say immediately that I concur with the idea that the theory of operant conditioning, its development and its implementation, has been a creative achievement. It is a valuable tool in the promotion of certain types of learning. I do not denigrate the contribution it has made. But this is not the basis of divergence.

Let me also say that I have a great personal respect for Fred Skinner. He is an honest man, willing to carry his thinking through to its logical conclusions. Hence, we can differ sharply, without damaging my respect for him. I was invited by several periodicals to respond to *Beyond Freedom and Dignity* (Skinner, 1971) and declined primarily because I felt he had a right to his views. My one disappointment in regard to Skinner is his refusal to permit the nine-hour confrontation we held at the University of Minnesota in Duluth to be released. It was all taped and is the deepest exploration in existence of the issues between us. All of the other parties to the meeting had understood that it was agreed that the tapes, or transcripts of them, or both, would be released. After the meeting, Skinner refused his permission. I feel the profession was cheated.

I have come to realize that the basic difference between a behavioristic and a humanistic approach to human beings is a *philosophical* choice. This certainly can be discussed, but cannot possibly be settled by evidence. If one takes Skinner as of some years ago—and I believe this is his view today—then the environment, which is part of a causal sequence, is the sole determiner of the individual's behavior, which is thus again an unbreakable chain of cause and effect. All the things that I do, or that Skinner does, are simply inevitable results of our conditioning. As he has pointed out, man acts as he is forced to act, but as if he were not forced. Carried to its logical conclusion, this means, as John Calvin concluded earlier, that the universe was at some point wound up like a great clock and has been ticking off its inexorable way ever since. Thus, what we think are our decisions, choices, and values are all illusions. Skinner did not write his books because he had chosen to present his views, or to point to the kind of society he values, but simply because he was conditioned to make certain marks on paper. Amazingly to me, he admitted as much in one session in which we both participated.

My experience in therapy and in groups makes it impossible for me to deny the reality and significance of human choice. To me it is not an illusion that man is to some degree the architect of himself. I have presented evidence that the degree of self-understanding is perhaps the most important factor in predicting the individual's behavior. So for me the humanistic approach is the only possible one. It is for each person, however, to follow the pathway—behavioristic or humanistic—that he finds most congenial.

Saying that it is for the individual to decide is not synonymous with saying that it makes no difference. Choosing the humanistic philosophy, for example, means that very different topics are chosen for research and different methods for vali-

dating discoveries. It means an approach to social change based on the human desire and potentiality for change, not on conditioning. It leads to a deeply democratic political philosophy rather than management by an elite. So the choice does have consequences.

To me it is entirely logical that a technologically oriented society, with its steady emphasis on a greater control of human behavior, should be enamored of a behavioristic approach. Likewise, academic psychology, with its unwavering insistence that "the intellect is all," has greatly preferred it over the humanistic approach. If the university psychologist accepted the latter view, he would have to admit that he is involved, as a subjective person, in his choice of research topics, in his evaluation of data, in his relationship to students, in his professional work. The comfortable cloak of "objectivity" would necessarily be dropped, exposing him as a vulnerable, imperfect, subjective being, thoroughly engaged, intellectually *and* emotionally, objectively *and* subjectively, in all his activities. This is understandably too threatening.

Let me simply add that what is really at issue is the confrontation of two paradoxes. If the extreme behaviorist position is true, then everything an individual does is essentially meaningless, since he is but an atom caught in a seamless chain of cause and effect. On the other hand, if the thoroughgoing humanistic position is true, then choice enters in, and this individual subjective choice has some influence on the cause-and-effect chain. Then, scientific research, which is based on a complete confidence in an unbroken chain of cause and effect, must be fundamentally modified. I, as well as others, have attempted partially to explain away this dilemma—my own attempt was in a paper entitled "Freedom and Commitment" (Rogers, 1964)—but I believe we must wait for the future to bring about the full reconciliation of these paradoxes.

In all candor I must say that I believe that the humanistic view will, in the long run, take precedence. I believe that we are, as a people, beginning to refuse to allow technology to dominate our lives. Our culture, increasingly based on the conquest of nature and the control of man, is in decline. Emerging through the ruins is the new person, highly aware, self-directing, an explorer of inner, perhaps more than outer, space, scornful of the conformity of institutions and the dogma of authority. He does not believe in being behaviorally shaped, or in shaping the behavior of others. He is most assuredly humanistic rather than technological. In my judgment he has a high probability of survival.

Yet, this belief of mine is open to one exception. If we permit one-man control, or a military take-over of our government—and it is obvious we have been (and are) perilously close to that—then another "scenario" would take place. A governmental–military–police–industrial complex would be more than happy to use scientific technology for military and industrial conquest and psychological technology for the control of human behavior. I am not being dramatic when I say that humanistic psychologists, emphasizing the essential freedom and dignity of the unique human person, and his capacity for self-determination, would be among the first to be incarcerated by such a government.

But enough of this issue. I have strayed into the future. Let me return to my retrospective look and to some less serious reflections.

Two Puzzlements

There are two very different issues which have puzzled me; one of the minor, the other of deeper concern.

REGARDING THEORY

By 1950, I wondered increasingly if my thinking could be put into a coherent theoretical form. At about this time came a request from Sigmund Koch (1959–1963) to contribute to his monumental series of volumes, *Psychology: A Study of a Science*. This was just the slight nudge I needed, and for the next three or four years I worked harder on this theoretical formulation than on anything I have written before or since. It is, in my estimation, the most rigorously stated theory of the process of *change* in personality and behavior which has yet been produced. As one young psychologist with a background in mathematics said to me recently, "It is so precise! I could restate it in mathematical terms." I must confess this is close to my opinion.

I was very pleased that it would be in Koch's series, because I felt sure that these volumes would be studied by graduate students and psychologists for years to come. I do not have exact data, but I suspect these volumes are in fact very little used. Certainly my chapter "A Theory of Therapy, Personality, and Interpersonal Relationships as De-

veloped in the Client-Centered Framework" is the most thoroughly ignored of anything I have written (Rogers, 1959). This does not particularly distress me, because I believe theories too often become dogma, but it has, over the years, perplexed me.

REGARDING CREATIVE LEADERSHIP

The second puzzlement is of a different order. In my younger years, while I was not a hero-worshiper, I definitely looked up to a number of men whom I felt were "*real* psychologists," whereas I existed on a poorly accepted fringe. I remember the community and professional furor when Leonard Carmichael was brought to the University of Rochester in 1936 as chairman of psychology: a special laboratory equipped to his specifications, a cluster of fellowships provided for his students, every acknowledgment paid to his brilliance and leadership. There was probably some envy in my attitude, as I labored away in a ramshackle frame building set aside for the Child Study Department of the Society for the Prevention of Cruelty to Children, but my feeling was mostly one of admiration and expectancy. I felt the same way toward perhaps a half-dozen others —better trained in psychology than I, in my judgment more brilliant, with books and research studies already to their credit. Here were the men who would produce the great ideas in psychology, who would exert the same kind of intellectual and world leadership as that of outstanding chemists, physicists, and astronomers. I had no doubt at all that I had picked those who, a generation later, would be the preeminently creative and productive leaders of our science.

In every case I have been mistaken. Carmichael, since I have mentioned his name, has gone on to become a revered administrator, operating in the highest levels of the establishment. The others I selected have also had perfectly reputable careers, some outside of and some in psychology. But the dazzling promise of their younger years has not been fulfilled. For some reason this has puzzled me very deeply, because they have one attribute in common. They have lost any truly vital *creative* interest in psychology. Why? Were their interests too narrow and unsatisfying as they grew older? Did they lack any basic conviction or philosophy which might have guided their work? Did their efforts come to seem to them irrelevant to the larger social scene, their contributions too picayune? Was their initial work done primarily to impress their fellow psychologists, a motive which declines in importance with age? Did they endeavor to stand on and defend their early work, thus inhibiting themselves from reaching out into the creative unknown? I do not know. It has thoroughly perplexed me and made me very wary indeed of trying to pick prospective leaders of creative thought.

The Sources of My Learnings

As I try to review all of the rich streams of thought and experience which have fed and are feeding my professional life, I can discriminate several of the most important sources.

CLIENTS AND PARTICIPANTS

First and overwhelmingly foremost are my clients in therapy and the persons with whom I have worked in groups. The gold mine of data that resides in interviews or group sessions staggers me. There is, first of all, the gut-level experience, which absorbs the statements, the feelings, and the gestures, providing its own complex type of learning, difficult to put into words. Then there is the listening to the interchanges in the tape recording. Here are the orderly sequences that were missed in the flow of the experience. Here, too, are the nuances of inflection, the half-formed sentences, the pauses, and the sighs, which were also partially missed. Then, if a transcript is laboriously produced, I have a microscope in which I can see, as I termed them in one paper, "the molecules of personality change." I know of no way of so combining the deepest experiential learning with the most highly abstract cognitive and theoretical learnings as the three steps I have mentioned: living the experience on a total basis, rehearing it on an experiential–cognitive basis, and studying it once more for every intellectual clue. As I said earlier, it is perhaps the most valuable and transparent window into the strange inner world of persons and relationships. I feel that if I subtracted from my work the learnings I have gained from deep relationships with clients and group participants, I would be nothing.

YOUNGER COLLEAGUES

The second most important source of stimulation for me is my symbiotic relationship with younger

people. I do not understand this mutual attraction. I just feed upon it. In my youth I surely learned many things from my elders, and at times I have even learned from colleagues in my own age bracket, but certainly for the last 35 years any real learnings from professional sources have come from those who were younger. I feel a deep gratitude to all the graduate students, younger staff members, and inquiring youthful audiences who have educated and continue to educate me. I know that for many years, given the chance to associate with professional colleagues of my age, or with a younger group, I inevitably drift to the latter. They seem less stuffy, less defensive, more open in their criticism, more creative in suggestion. I owe them so much. I started to write down examples, but to give a few would be unfair to the hundreds who have so freely contributed their ideas and their feelings in a relationship which has also lighted sparks of creative thinking in me. They have excited me, and I have excited them. It has, I hope, been a fair exchange, though I often feel I have gained more than I have given. I feel a great pity for those persons I know who are growing into old age without the continuing stimulation of younger minds and younger lifestyles.

SCHOLARLY READING

Then much farther down the scale I would put what is often regarded as a major source of learning, the printed page. Reading, I fear, has most of its value for me in buttressing my views. I realize I am not a scholar, gaining my ideas from the writings of others. Occasionally, however, a book not only confirms me in what I am tentatively thinking, but lures me considerably further. Kierkegaard, Buber, and Polanyi, for example, would fall in that category. But I must confess that when I wish to be scholarly, serendipity plays a very important part. Serendipity, in case you have forgotten, is "the faculty of making fortunate and unexpected discoveries by accident." I have an eerie feeling that I have that faculty. Let me give you the latest examples. In preparing a current paper, "The Emerging Person: A New Revolution," I was aware of a few of the writers who were presenting similar views. But then Fred and Anne Richards (1973) sent me a copy of their book *Homonovus*, just off the press. It was most timely. John D. Rockefeller III (1973) (I have had two contacts with him) likewise sent me a copy of his book *The Second American Revolution*, which was also highly pertinent. Then I was talking with a friend from northern California about my fantasies for my APA paper and he said, "Did you read the article by Joyce Carol Oates in the *Saturday Review*?" I had to confess complete ignorance not only of the article but of the author. His Xerox copy of the essay not only gave support to my view, but opened my eyes to a whole new facet of modern fiction. So, while one section of that paper may make it appear that I spent days or weeks researching in the library, at least half of that impression is due to serendipity. It has been a very frequent aid in my life.

My Concern with Communication

Still peering back—through my neck is getting stiff from that posture—I can see what is perhaps one overriding theme in my professional life. It is my caring about communication. From my very earliest years it has, for some reason, been a passionate concern of mine. I have been pained when I have seen others communicating past one another. I have wanted to communicate myself so that I could not be misunderstood. I have wanted to understand, as profoundly as possible, the communication of the other, be he a client or friend or family member. I have wanted to *be* understood. I have tried to facilitate clarity of communication between individuals of the most diverse points of view. I have worked for better communication between groups whose perceptions and experiences are poles apart: strangers, members of different cultures, representatives of different strata of society. To give adequate examples would compass the length of my career. I will cite only one. The filmed experience of a group involved in the drug scene included straight individuals, such as a narcotics agent, and "stoned" individuals, including a convicted drug pusher. There were blacks and whites, the young and middle-aged, ghetto products and members of the middle class. The group process by which communication and closeness became a living part of this diverse group is an experience I shall never forget. It is unfortunate that the film's title, *Because That's My Way*, chosen for us, catches so little of the vivid interchange which occurred (Station WQED, 1971).

This obsession with communication has had its own unexpected rewards. I held a half-hour interview with a young woman named Gloria (some of

you may have seen the film) and a deeply communicative contact was established. To my complete surprise, she has kept in occasional touch with me for eight years, primarily in appreciation for the closeness we achieved. With Randy, the convicted drug pusher in the drug film, I was in constant correspondence for more than a year. Mr. Vac, one of my clients in our complex research on psychotherapy with schizophrenics, tracked me down after eight years with a "Hi, Doc," to let me know that he was still doing well and had never returned to the state hospital, even for a day. I think such rewards are savored more as the years go by.

In Sum

So I can sum up my informal look at my professional past by saying:

I am amazed at the impact of our work;

I have a dim comprehension that the time was ripening for it;

I look with amusement and affection at the ambivalence I have created in psychology;

I see with satisfaction the war with psychiatry concluding;

I am pleased to have played a part in the continuing drama of the behavioristic versus the humanistic philosophy;

I am puzzled and humbled by the disregard of what I see as my theoretical rigor;

I am perplexed by the later careers of some of the truly shining lights I have seen;

I am especially grateful for the gift of vital learnings from the people whose development and growth I have endeavored to facilitate;

I have confidence in the young, from whom I have continuously learned;

I discern more sharply the theme of my life as having been built around the desire for clarity of communication, with all its ramifying results.

The Now—and the Future

I should stop here, but I cannot. It is always a strain for me to look backward. It is still the present and the future which concern me most. I cannot close without a quick overview of my current interests and activities.

I am no longer actively engaged in individual therapy or empirical research. (I am finding that after one passes 70, there are physical limitations on what one can do.) I continue to engage in encounter groups when I believe they might have significant social impact. For example, I am involved in a program for the humanizing of medical education. Up to the present, more than 200 high-status medical educators have been involved in intensive group experiences which appear to be more successful in facilitating change than we had dared hope. Perhaps more humanly sensitive physicians will be the result. It certainly represents a new area of possible impact.

I have also helped to sponsor, and have taken some part in, interracial and intercultural groups, believing that better understanding between diverse groups is essential if our planet is to survive. The most difficult group was composed of citizens of Belfast, Northern Ireland. Represented in the group were militant and less militant Catholics, militant and less militant Protestants, and English. The film of that encounter portrays its difficult and partial progress toward better understanding—a first step on a long road. I see it as a small test tube attempt which might be utilized in greater depth and much more widely.

I continue to write. I recognize that while my whole approach to persons and their relationships changes but slowly (and very little in its fundamentals), my interest in its applications has shifted markedly. No longer am I primarily interested in individual therapeutic learning, but in broader and broader social implications. Perhaps the titles of some of my recent papers (completed or in progress) will best give you a glimpse of where I am in my present work. The approximate titles are, "My Philosophy of Interpersonal Relationships and How It Grew"; "Bringing Together the Cognitive and the Affective–Experiential in Education"; "Some New Challenges to Psychology"; "Some Social Issues Which Concern Me"; "The Emerging Person: A New Revolution." As I state these, the question arises in my mind, as it often has in the past, "Am I spreading myself too thin?" Only the judgment of others can answer that question at some future date.

And then I garden. Those mornings when I cannot find time to inspect my flowers, water the young shoots I am propagating, pull a few weeds, spray some destructive insects, and pour just the proper fertilizer on some budding plants, I feel cheated. My garden supplies the same intriguing question I have been trying to meet in all my professional life: What are the effective conditions for growth? But in my garden, though the frustra-

tions are just as immediate, the results, whether success or failure, are more quickly evident. And when, through patient, intelligent, and understanding care I have provided the conditions that result in the production of a rare or glorious bloom, I feel the same kind of satisfaction that I have felt in the facilitation of growth in a person or in a group of persons.

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Scientific Awards Announcement

The Committee on Scientific Awards is accepting nominations for its award program. The Committee selects up to three persons as recipients of the **Distinguished Scientific Contribution Award** who, in its opinion, have made the most distinguished theoretical or empirical contributions to scientific psychology in recent years.

A new award, the **Distinguished Contribution for Applications in Psychology,** has been authorized by the Board of Directors and will be given for the second time this year. This award will be presented to an individual who, in the Committee's opinion, has engaged in a program of research which is systematic and applied in character.

The awards are subject to the following limitations: (*a*) members of the Committee, former recipients of the awards, the President and the President-elect of the APA shall be ineligible. (*b*) The Committee shall seek diversity in selecting recipients, avoiding as far as possible the selection of more than one person representing a specialized topic, a specific material, a given method, or a particular application.

Names and appropriate information which will guide the Committee on Scientific Awards in conducting an intensive career review and evaluation should be forwarded to Office of Scientific Affairs, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. *Deadline for nominations is March 1, 1974.*

Salvation and Its Vicissitudes

Clinical Psychology and Political Values

BENJAMIN BEIT-HALLAHMI *University of Michigan Counseling Center* [1]

Discussions of values, as opposed to discussions of technology and ethics, are relatively rare among clinical psychologists and constitute only a limited component of graduate training. One major reason for this is that many clinical psychologists consider their technology to be value free. Personally, I find this view rather amusing. I can recall several case conferences when a statement by one participant was reacted to by another with a victorious, criticial "But this is a value judgment!" My reaction is, "But I thought everything we were doing here was based on value judgments." This last statement is likely to enhance my reputation as a cynic who has little respect for the sacred cows of the profession and the delicate feelings of his colleagues. It is interesting to note that those who emphasize the role of values in professional work are often labeled "cynics," and those who remind us of glaring inequalities and exploitation are labeled "materialists," while those who deny the importance of values and morality and justify exploitation and inequality are, apparently, idealists and purists. What becomes clear is that, for most psychologists, there is no need to talk about values, because they are taken for granted and are agreed on by a majority of their professional colleagues. There is little need to discuss what we all agree about; discussion is more likely to arise in cases of conflict. What we all take for granted may be ego syntonic, personally and professionally, may be consistent with our self-image, and is probably consistent with our self-interest and the need for survival in this society, but it is exactly what we take for granted that most clearly reflects our biases.

What we normally deal with when we talk about values in psychology are "professional ethics" (American Psychological Association, 1963). Professional ethics, as defined by the majority of psychologists, deal with the rules of our game vis-à-vis society. They reflect both an honest concern with the rights of others and the wish to preserve our professional monopoly and privileged status, but these rules of professional conduct stay within the realm of technical questions and our reputation as good citizens. Professional ethics concentrate on the moral implications of our relationship with the client, but do not deal with the moral implications and the political consequences of this relationship as they extend to other areas of the client's life.

In our actions as clinical psychologists, we express our implicit theories of human nature and human society. Normally we discuss theories of human nature and refrain from talking about our theories of society. It is assumed that we can leave the task of dealing with the nature of society to others, and the decision to do that relegates the political implications of psychology to the professional unconscious, where it is destined to lie repressed unless uncovered through some radical treatment or some severe trauma (cf. Simon, 1970). Analyzing implicit value statements in our professional work will lead us to focus on the political implications of our daily activities. The relationship of professional psychology to the rest of society and especially its political implications are evidently open to several interpretations. One view simply asserts that psychologists always serve those in power, whether they are aware of it or not. Another view regards psychology as a major force working for cultural change or cultural rebellion. We will have to check both views against reality and will find that the truth does *not* lie somewhere in between.

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[1] This article is partly based on a paper presented at the Michigan State University Conference on Clinical Psychology, East Lansing, May 1973.

Requests for reprints should be sent to Benjamin Beit-Hallahmi, who is now at the Department of Psychology, University of Haifa, Haifa, Israel.

Szasz's (1961) analysis of the psychiatric profession seems pertinent to what most clinical psychologists do. Psychiatric activity, according to Szasz, is a form of social control. The psychiatric professional activity is a combination of three roles: (*a*) theoretical scientist—an expert on game-playing behavior; (*b*) social engineer—one who sorts out players and assigns them to games they can play; and (*c*) social manipulator—one who influences people to induce them to play, or cease to play, certain games. In the psychiatrist's main role as a social engineer, he sorts people into pigeonholes of "identities" in which they belong, and in his role as a social manipulator, or therapist, he makes sure that they stay there. The extent to which most therapists foster social conformity and social control, rather than self-actualization, is clear in a nationwide survey of therapists by Goldman and Mendelsohn (1969).

Our dilemma is summarized in a quotation from the play *The Cocktail Party*, by T. S. Eliot (1965). Eliot's heroine goes to a psychiatrist and starts her interview with the following statement: "I should really like to think there's something wrong with me—Because, if there isn't, then there's something wrong . . . with the world itself . . . [p. 132]." Our clients, when they come to us, ask the same question: Is there something wrong with them, or is there something wrong with the world around them? And with few exceptions, psychologists tell them that there is something wrong with them and nothing wrong with the world. We usually ignore the possibility of structural social problems, which in turn cause some of the discomfort in our clients. The importance of interpersonal and social conflicts tends to be obscured by our preoccupation with internal conflicts (cf. Hurvitz, 1973; Mills, 1959). As Szasz (1970) suggested, the psychological approach is used to de-ethicize and depoliticize ethical and political issues. By deciding to become "neutral" or "irrelevant" we are taking a significant political stand.

In individual work and in theorizing about groups, we formulate explanations for emotional states, especially those likely to create conflict. We do have ideas about the sources of interpersonal conflict and personal unhappiness. We normally refer to anger and sadness as "hostility" and "depression," and a crucial part of our social role is to explain their sources. We are called on to explain the sources of oppression and the way it is felt by the individual. The major question is whether the sources of oppression are external or internal. The traditional psychodynamic view saw the disturbed person as oppressed by his symptoms, inhibitions, and other inappropriate behaviors (Hartmann, 1960). This view of oppression as mainly internal is still accepted by most psychologists today. Those who explain their problems and oppression as being caused by external sources are rejected by us as externalizing, defending themselves against the more demanding internal frame of reference.

A major function of psychologists today is to rationalize inequality, or, in more elegant language, to explain the differential allocation of rewards in society. Psychologists are called on to make individual judgments as to who shall be rewarded, and they are also involved in more general explanations as to who shall inherit the earth, and why. The way psychologists rationalize inequality is by telling us that people at the bottom of the social ladder are lower on IQ tests, show less ego strength, have weaker superegos, and are unable to delay gratification. (See Hollingshead & Redlich, 1958; Levy, 1970; Mehlman & Fleming, 1963; Reissman, Cohen, & Pearl, 1964.) If only psychology had been sufficiently developed in the 1840s in the United States, we would have read a psychological analysis of slavery, describing slaves as "unable to delay gratification, low on frustration tolerance, having psychopathic tendencies, scoring lower on intelligence tests, and in general being unmotivated, impulsive, and violent." An interesting conclusion would have been that slavery is a psychological syndrome, transmitted from generation to generation. Support for this conclusion would have come from the observation that children born to slaves tend to become slaves themselves. If the above description seems absurd, one should look at the way psychologists discuss the problem of poverty today; then this absurd pattern will become familiar. (See Billingsley, 1970; Herzog, 1970; Ryan, 1971.) The extent of person blaming as a strategy in psychological research of social problems and the various forms this technique takes are brilliantly described and illustrated by Caplan and Nelson (1973):

The psychological view of human suffering is discovered to be, upon a closer look, a repetition of the old conservative morality: Man is by nature sinful and corruptible; most of an individual's suf-

fering is blamed on himself in a variety of ways. The modern psychological way of doing it is by describing a variety of personality deficiencies: lack of motivation, inability to delay gratification, low frustration tolerance, "psychopathic personality," etc. When we look at psychology as a mechanism of social control, an interesting analogy comes to mind. If we combine the emphasis on individual responsibility, internal causation, and individual solution to problems, it all sounds a little like a call for soul searching and individual salvation, and indeed it may be. The similarity between the functions of religion and psychology was also noted by London (1964), who described the enterprise of psychotherapy as "the salvation of secular man" and psychotherapists as "secular priests." When we look at the content of what both psychology and religion offer the individual, the similarity is rather striking: Both move away from the social and material world, to deal with the invisible world of feelings and fantasies. Both offer salvation at the individual and internal level, not in any social, political, or economic way. Both psychology and religion tell us that the road to happiness is through individual change and not a change in the world around the individual.

IS PSYCHOLOGY A FORCE FOR SOCIAL CHANGE?

Is psychology indeed the opium of the people, as I was implying above? Most psychologists see themselves as part of a movement toward positive social change and will be terribly offended if we suggest to them that they are part of a social control mechanism. Many psychologists have an image of themselves and their profession as counterdependent, rebellious, and critical of society. Many clinical psychologists see themselves as the vanguard of a new humanistic revolution. Recent developments in the technology of psychotherapy and especially new group approaches are hailed as signifying a new age and a "human potential movement." Some of the claims of this movement rest on the assumption that the atmosphere of social groups and organizations can change with the help of proper therapeutic procedures, and the age of Aquarius will dawn upon us as a result of greater individual awareness and liberation.

A popular book detailing "awareness exercises" is appropriately entitled *What to Do Till the Messiah Comes* (Gunther, 1971). The title implies an expectation of a major change in this world and in this society. But until this eschatological event comes, the reader is offered a series of experiences to tide him over. These are all temporary measures, but that is what we must do while we are waiting for real change. We can change our very private environment, while the Messiah is expected to do the rest. This anecdote raises some serious questions: (*a*) Can psychology change society in any way? (*b*) Do we have the potential or power to bring about change? (*c*) What is the real effect of the things we do "until the Messiah comes"? Is it possible that all these temporary measures may be delaying change?

One of the claims heard most often from psychologists who are involved in changing individuals goes like this: "We help people get their heads together, so that they can change the world around them." As Halleck (1971) suggested, helping people get their heads together often results in the elimination of their desire to change the world. People who are thoroughly pleased with their lives do not make good revolutionaries. Both Halleck (1971) and Halmos (1970) showed how psychological interpretations and emphasis on individual motivation neutralize potentially political energy.

One answer to the question of gap between what psychologists are doing and what they think they are doing has to do with the social origins of people who become clinical psychologists. Marginality is central in the experience of clinical psychologists, both individually and as a group. As a group, psychologists are notoriously deviant. They differ from majority views in both attitudes and life-styles (Roe, 1956). They are openly critical of social norms and social roles. How can we integrate this view of psychologists with their role as social engineers and social controllers?

There is indeed a paradox here, the paradox of those who are marginal and deviant and who are put in a position of being controllers and resocializers. We might say that the minimally deviant psychologists are resocializing the extremely deviant, those who are defined as "mentally disturbed," "mentally ill," or "criminal." Society's way of recruiting social controllers is based on both individual dynamics and societal needs. Often, the social regulator is very close to those he regulates, both in dynamics and origins. We often comment on how policemen are very similar to those they arrest, in both dynamics and social origin. The difference between the cop and the robber is that the cop becomes aware of his impulses early enough

to control them by becoming a controller. Similarly, choosing the career of a social engineer may have something to do with recognizing one's own impulses and defending against them. In terms of individual dynamics, we may speculate that this career choice is related to an incomplete identification with the parents in early childhood. This identification is completed by assuming the parental role in work with deviants and by identifying at the same time with the client, who is rebuilding his own identification. In terms of individual mobility, choosing the career of a psychologist means for most individuals who make this choice moving upward and assimilating into the majority because most clinical psychologists come from socially marginal groups (Henry, Sims, & Spray, 1971).

Recent Developments and Their Implications

Recent developments in professional practice can be looked at from the perspective of their relationship to the traditional view presented above, either as challenges to, or derivatives of, traditional approaches. Most of the changes in practice originate from the internal frustrations and failures of the profession itself and present themselves as innovations in technology. Some of the changes, mainly those that arose in response to external pressures, may involve a different value perspective.

Perhaps the major development over the last 10 years in terms of clinical practice has been the rise (maybe the triumph) of behavior modification. In terms of the values and the political views described so far, behavior modification is paradoxical because it is both the embodiment and the natural consequence of the conservative trend in psychology and the only psychological school that is openly and consciously value laden. We should commend our colleagues who practice behavior modification for their honesty in stating their value preferences. Most of them are quite open about the fact that they work to enforce social norms and prevent deviance. Contrary to other therapists, they are ready to tell us exactly what kinds of changes they want to bring about in their clients, and why. Their starting point is always what is socially desirable and socially acceptable. One of the promoters of behavioral approaches to deviance, J. V. McConnell (1970), suggested the following:

> We should try to regulate human conduct by offering rewards for good behavior whenever possible instead of threatening punishment for breaches of the law. We should reshape our society so that we all would be trained from birth to want to do what society wants us to do [p. 74].

The behavioral approach recognizes the importance of the environment in shaping the individual, but attempts to reshape the individual, rather than the environment, when dealing with human suffering and dissatisfaction. In this respect, behavior modification is in total agreement with most other clinical approaches. Philosophically, behavior modification is the logical result of the "end of ideology" and the "end of values" movement in clinical psychology, the reduction of all moral questions to technical ones. "End of ideology" theories, in both psychology and politics, are promoted by conservative forces. The assumption behind them is that of a broad consensus, which unites most members of society and most members of the profession, so that only "technical" problems remain to be solved (cf. Halmos, 1970). Behavior modification is the logical result of our emphasis on "functioning" and on our "big machine" model of society. We start with the belief that the big machine is all right; we just have to keep it oiled and running. Individuals, who can best be conceptualized as little machines, should adjust to the big machine, and they too should be kept oiled and running. Psychologists help people adjust to the reality of the big machine.

Other changes in practice present more of a challenge to the narrow ecological view of the causation of human suffering because they go beyond the individual and attempt wider system intervention. The newer developments of family therapy and network therapy show that the preoccupation with individual causation can be constructively overcome. These newer approaches also give due recognition to what is going on within the family at present, and not only what was going on within the family 20 and 30 years ago. Dealing with systems may lead psychologists to look at wider and more encompassing ones. It is impossible to ignore the continuity between the family and society as a whole, and so psychologists are already looking at whole communities and may find themselves looking at whole societies.

More serious challenges to the traditional model of the person in psychology come from those who attack the basic definition of deviance. The drive toward politicalization of private problems offers us concrete examples of an alternative way of viewing our clients. The emphasis on what human beings

have in common internally is countered by the new emphasis on what people have in common externally, such as race and sex. We are used to dealing with individual and universal insecurities involved in the process of growing up in human society. The new approaches emphasize the insecurities involved in growing up as a member of a social group. The differences between growing up powerful and growing up powerless, growing up upper class and growing up working class, growing up white and growing up black, all lead to differences in psychological experiences and self-concept.

The experiences of every woman, black, and Chicano are affected by their social positions. Belonging to an oppressed group in these cases is one of the major determinants of individual experience. Politicizing women's problems is significant because women make up the majority of our clients. The feminist viewpoint implies that there are external forces and necessities that shape women's behavior into forms that are then regarded as deviant or pathological. The external social forces are those that determine what appears to be prevalent forms of psychopathology. The hysterical, depressed housewife has some pretty good reasons to be depressed, and identity conflicts in college women are a result of reality pressures. When we look at minority groups, the radical viewpoint emphasizes reality factors in the causation of personal difficulties and underscores the relativity of clinical concepts.

The lessons of "community mental health" and studies in psychiatric "epidemiology" are quite clear. They show that the poor and minority groups are "high-risk" populations in terms of "mental health," which means that members of these groups are more likely to become inmates in mental hospitals and prisons. One of the daring suggestions of the recent Nader's Raiders' report on community mental health was that employment opportunities and better housing could do more for the "mental health" of the poor than could community "mental health" centers. This suggestion was properly ignored by the "mental health" professionals because it obviously indicates defensive externalization.

Many psychologists have become sensitized to the problems of women and are less likely to take a totally individualistic position when looking at the problems of individual women. Many are recognizing more and more the effects of the social situation on members of minority groups and are willing to examine critically the social consequences of psychological measurements and definitions. We can then generalize and suggest that there are general social conditions affecting most members of this society, not only minority groups. Growing up and living in a society that regards and treats people as commodities affects every individual's self-concept, confidence, and abilities (Marcuse, 1964, 1966). In working with individual clients and in our attempts to generalize from work with individuals, we have to recognize social and political realities. Both the psychologist and the client are subject to enormous social pressures. Clients come to us often when they have to make real-life decisions, and all too often we tend to ignore the impact of political and economic realities to which they respond. What we are all interested in, or, that is, what we say we are interested in, is the maximal actualization of human potential. The question before us is whether such an actualization is possible without a major transformation of society. Most psychologists today are committed to a viewpoint which sees self-actualization as totally dependent on the individual and are totally satisfied with a role definition which demands that we leave society intact, while changing only the individual. Those who are committed to a different choice of values are in the unenviable position of reminding the majority of psychologists of its repressed responsibilities.

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Gold Medal Award

The Trustees of the American Psychological Foundation invite members of the American Psychological Association to nominate candidates for the Gold Medal Award. The Award is given to an American psychologist in recognition of a distinguished and long-continued record of scientific and scholarly accomplishments. The Award is limited to psychologists 65 years of age or older and is also limited to those residing in North America. Nominations should be accompanied by a statement highlighting the accomplishments of the nominee. They should be addressed to: Secretary, American Psychological Foundation, 1200 Seventeenth Street, N.W., Washington, D.C. 20036, to be received no later than March 15, 1974. Decisions will be made by the Trustees of the Foundation.

The History of Psychology Revisited

Or, Up with Our Foremothers

MAXINE D. BERNSTEIN *Richmond College, City University of New York*
NANCY FELIPE RUSSO *American University* [1]

Test yourself (no peeking):

Q1. Who were the first persons to use the term *projective technique* in print?

Q2. Who was the first person to develop child analysis through play?

Q3. Who developed the Cattell Infant Intelligence Test Scale?

Q4. What do the following have in common?
Bender-Gestalt Test (Bender, 1938); Taylor Manifest Anxiety Scale (Taylor, 1953); Kent-Rosanoff Word Association Test (Kent & Rosanoff, 1910); Thematic Apperception Test (Morgan & Murray, 1935); Sentence Completion Method (Rohde, 1946).

Q5. The following are the last names of individuals who have contributed to the scientific study of human behavior. What else do these names have in common?
Ausubel, Bellak, Brunswick, Buhler, Dennis, Gardner, Gibson, Glueck, Harlow, Hartley, Hoffman, Horowitz, Jones, Kendler, Koch, Lacey, Luchins, Lynd, Murphy, Premack, Rossi, Sears, Sherif, Spence, Staats, Stendler, Whiting, Yarrow.

Now for the answers:

A1. Lois Murphy and Ruth Horowitz (1938).
A2. Hermine von Hug-Hellmuth (1921).
A3. Psyche Cattell (1947).
A4. A woman is either the senior author or the sole author of each work.
A5. They are the surnames of female social scientists.

When these questions were asked of an amicable male colleague, his response was to smile, shrug politely, and answer "That's nice—uh—so what?"

One answer to the question "So what?" can be found in an incident that occurred in the Cornell Psychology Department not so very long ago. Some psychology graduate students were concerned about departmental governance. They circulated a memo demanding that the faculty reevaluate departmental procedures. Included in the memo was the suggestion that the faculty revise its admission policies in light of the high proportion of women accepted for graduate study in psychology. "After all," it was pointed out, "women just drop out or go on to get married and waste their education. *Besides, what have women ever contributed to psychology, anyway?*"

That graduate students at a prestigious Ivy League school would hold so little respect for the contributions of women to their chosen field demonstrates that the male orientation of psychology goes beyond discrimination in employment (Astin et al., 1972), bias in selection of subjects (Carlson, 1971; Schultz, 1969), overgeneralization of findings from males to all persons (Dan & Beekman, 1972), or the formulation of concepts and theories (Broverman et al., 1972; Chesler, 1972; Weisstein, 1968). Male bias pervades the very essence of the profession—the historical definition of the field of psychology itself.

In addition, the fact that two of the originators of the memo at Cornell were attending a seminar on perceptual development given by Eleanor Gibson shows that this sexist attitude will not be

[1] Requests for reprints should be sent to Nancy Felipe Russo, Department of Psychology, American University, Washington, D.C. 20016.

overcome by exposure to one or two famed "exceptions." Change will require a reorientation of the professional socialization process. Psychology must rediscover the contributions of its women and give them equal time and space with those of men.

The benefits of sensitizing psychologists to the contributions of women to the field are many. Two of the most important ones are provision of role models and service as a source of pride and inspiration. In addition, women's history can refute the stereotypes and myths that often form the foundation for discriminatory practices. If psychologists are taught to acknowledge, appreciate, and respect the contributions of women to the profession, their image of women cannot help but be altered, and behavioral change can thus be facilitated.

The lack of historical insight into the contributions of women psychologists is due to a multiplicity of causes, one of the simplist, and most insidious, being our method of documentation. In some publications only initials are used to indicate first names in bibliographies, while in others initials are used for men and women's names are spelled out in full. This inconsistency leads one to assume that the lack of a first name in a citation indicates that the work was done by a man. Unless they are directly involved in a research area, using original sources, psychologists are dependent on informal means (personal stories, anecdotes, etc.) to tell them that "X" was a woman. Confusion is increased when women marry and change their names, losing continuity of documentation. As the years increase between the time a woman first publishes her research and the current moment in time, there is a decrement in the likelihood that someone will be around who "remembers" that this work was done by a woman.

The tendency to cite research by referring only to surnames carries over to informal conversation. Many people, for example, know Freud (1946) first reported on the defense mechanism of intellectualization, but few realize that in that case Freud's first name was Anna. When there are two psychologists of the same name—one male, one female—and research is cited by use of the last name only, it is often assumed that the work referred to is that of the male. If we begin to discuss the work of the social psychologist, Sherif, for example, how many people think of Carolyn? What a shock it was to go through six years of studying perception and then to find out that Zeigarnik's first name was Bluma.

Without a commitment by the profession to historical research and consciousness raising, the early contributions of women to psychology will be lost. We will probably never know how much work was done by women but credited to men: how many footnotes of appreciation should rightfully have been coauthorship, how many times junior authorship should have been senior authorship, or how many times it was the male coauthor who should have received the footnote. But we can, at least, become sensitive to the contributions that we do have record of and develop a better appreciation of them. Perhaps the question "What have women ever contributed to psychology" would not have been asked at Cornell had the students been aware that it was at this very University that Titchener's first doctoral student, Margaret Floy Washburn (1908), put together a compendium of animal psychology, which not only became a classic in the field, but which provided a partial impetus for the development of behaviorism.

Once one starts looking, contributions of women are found everywhere. Indeed, it is a source of inspiration that despite the discrimination, the hardships, and the restrictions, so many women did so much. Yet their work is not given proper recognition. For instance, it is not generally known that the principal author of the Thematic Apperception Test was Christiana D. Morgan (Morgan & Murray, 1935), or that the current revision of the Stanford-Binet Intelligence Test was coauthored by a woman (Terman & Merrill, 1937). In a time when women are searching for role models and new identities, it is exciting to discover that Taylor of the Taylor Manifest Anxiety Scale is named Janet, that Bender of the Bender-Gestalt test is known as Lauretta, and that the Kent of the Kent-Rosanoff Word Association Test was called Grace.

Women's contributions to the theoretical development of psychology have had a major impact on the development of new fields of inquiry, both in psychology and in related social sciences. Nancy Bayley (1933) and Anne Anastasi (1937) are recognized leaders in the field of testing, but one must also acknowledge the efforts of lesser known persons such as Grace Fernald, who, with William Healy, did pioneer work with delinquent children, developing an early series of performance tests (Healy & Fernald, 1910). Grace Fernald is not to be confused with the experimental psychologist Mabel Fernald (1912), who is known for her classic work on imagery and memory.

In visual perception, Magdelena Vernon (1937) has been a major influence, and Mary Henle (1961) is a principal figure in Gestalt psychology. Melanie Klein (1948), Anna Freud (1946), and Karen Horney (1939) have each influenced new directions in psychoanalysis. While Mead (Margaret, not George) (1939) and Benedict (1934, 1938) are known for their central roles in the development of the new fields of culture and personality, and psychological anthropology, lesser known figures include Martha Wolfenstein (1950), Florence Kluckholn (Kluckholn & Strodbeck, 1961), and Dorothea Leighton who coauthored two of the classic books in psychological anthropology with a prominent male anthropologist to whom these works are sometimes attributed (Kluckholn & Leighton, 1946; Leighton & Kluckholn, 1947). Also important (but neither last nor least) in these areas is Eggan (1949), who first suggested that dreams could be sources of valuable cross-cultural data.

In the field of social psychology, the studies of conformity are generally associated with the name of Asch, but a little-known woman (Berenda, 1950) was doing identical work at the same time. The group studies in cooperation and competition were first inspired by the work of a woman (Hurlock, 1927). The classic study on the authoritarian personality was the product of the central contributions of a woman (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950; Frenkel-Brunswik, Levinson, & Sanford, 1948), as were the comprehensive works on the need for social approval (Strickland & Crowne, 1962) and Machiavellianism (Christie & Geis, 1958). Two women contributed to a classic experimental study of aggression (Bandura, Ross, & Ross, 1961), and the first successful test of a psychoanalytic concept was the work of yet another woman (Goldman-Eisler, 1953). As Rosenthal (1966) noted, one of the earliest works that led to the uncovering of the Rosenthal effect was that of Edith Lord (1950). Finally, it should be mentioned that a woman was responsible for the current revision of the code of ethics on experimentation with human subjects by the American Psychological Association (Baumrind, 1964).

Classic texts in all fields of psychology have been written by women, or contributed to by women as second authors, including the works of Arnold (1960), Anastasi (1937), Benedict (1934), Bruner, Goodnow, and Austin (1956), Buhler (1939), Goodenough (1945), Jahoda, Deutsch, and Cook (1951), Maccoby, Newcomb, and Hartley (1958), Murphy (1937), Rickers-Ovsiankina (1960), Sherif and Sherif (1953), Shirley (1933), and Tyler (1956).

All psychologists are familiar with the Terman studies on genius, but not many know that two of the volumes presenting the results of those studies were written by women (Burks, Jensen, & Terman, 1930; Cox, 1926), or that John B. Watson's famous Albert experiment was coauthored with a woman (Watson & Rayner, 1920).

These examples are not just *curiosa*, nor are they meant to be comprehensive. They just represent the tip of a very large iceberg.

The current practice of using initials for all citations will only serve to obscure the contributions of women to the profession. As long as the English language uses pronouns of male and female gender so that we must say "she found this," or "he discovered that," psychologists must have a systematic means to designate the contributions of women to its journals. This is not incompatible with blind reviewing. Blind reviewing is essential so that females do not take to submitting articles with only their initials for fear of rejection due to prejudice. If such a practice became widespread, we would not even have the original sources to preserve the woman's identity. The contributions of contemporary women should not be lost. We need them as well.

Practices of discrimination and prejudice will not be overturned by adequate record keeping, however. Adequate documentation is a necessary, but certainly not a sufficient, means to upgrade the status of women in psychology. The professional socialization process itself must undergo a basic transformation.

The leadership of the American Psychological Association has recently been moving to assess and advance the position of women in the profession. The Task Force on the Status of Women in Psychology has done some very fine work toward these ends and has submitted a report which acknowledges the need for a professional reorientation. It recommends, among other things, that "appropriate subspecialties should inquire in depth into the psychology of women. New courses should be created to explore the psychology of women: e.g., developmental psychology of women, social psychology of women's status [Astin et al., 1972, p. 15]."

We affirm the exigency of such curriculum revision. However, psychologists must not limit

themselves to the study of the psychology of women—they must also study the women of psychology. Without historical insight, without an intellectual history ("herstory?") women will never be truly integrated into the profession. The current approach seems to be "let us in." We think the approach should be "recognize that we are here—and we have been here for quite a while. See what we've done despite adverse circumstances and incredible odds. Think of what we could do if we had an equal chance."

So we say, "Up with our foremothers." Teach women in psychology their intellectual history and preserve the contributions of contemporary women. Women have a proud tradition of accomplishment in the profession. Let it be recognized—by both women and men.

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Contributions to Education in Psychology Awards

The American Psychological Foundation invites nominations for the 1974 Contributions to Education in Psychology award. The annual award is in the amount of $1,000 to be given to the recipient for his achievements. It is hoped that the awardee's institution will contribute a matching sum, thus providing the recipient with a modest "grant" to be used by him as he wishes to "improve the teaching of psychology." Thus, the objective of the award program is to enhance the local instructional program, not just to recognize "master teachers." Because the awards are to be presented at the APA Annual Convention in September 1974, *nominations must be completed and sent to the Committee by March 31, 1974*. A nomination form with a statement of the guidelines and suggestions for documentation can be obtained by writing to Wilse B. Webb, Chairman, APF Teaching Awards Committee, Department of Psychology, University of Florida, Gainesville, Florida 32601.

New Members and Associates of the American Psychological Association

The Board of Directors announces that the following 2,906 persons were elected to initial membership in the American Psychological Association as of January 1, 1974. Of these, 1,929 were elected as Members, and 977 as Associate members. Though not all of them have validated their election by payment of initial dues, all but a few will eventually do so.

In addition, in accordance with Article II, Section 8, of the Bylaws, 346 Associate members were transferred to Member status as of January 1, 1974.

Members

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Abraham, Joseph Arnold
Abrahams, Joel Peter
Abrams, Gary
Abusaidi, Mohammad Saleh
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Ackland, Vergie Lee
Adams, David Bruce
Adams, William A.
Adler, Elliot Mathew
Adler, Lois E.
Adler, Nancy Elinor
Afanador, Josef Carter
Ahern, Elsie Hashimotu
Ahmed, Mohiyddin
Ahrons, Constance Ruth
Akamatsu, T. John
Alba, Enrique
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Alexander, Kenneth Ross
Alf, Herbert A.
Allaway, Thomas Adams
Allen, James Richard
Allen, Juanita Louise
Allen, Mary J.
Allen, Natividad M.
Alonso, Martha Rita
Alpert, Geri
Altenburg, Bonnie Patricia
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Altshuler, Richard J.
Ames, Russell Eugene, Jr.
Amicucci, Edward
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Auerbach, Carl Fredrick
Austin, John Atwood
Ayman, Tooraj

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Badre, Albert Nasib
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Barrett, James Edward
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Barrish, Irving Jay
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Bartol, Geoffrey Hemmenway
Bartsch, Thomas William
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Basow, Susan Anne
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Baukus, Erwin John
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Beach, Garnet
Bean, Joan P.
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Becker, James L.
Becker, Jeffrey Lee
Becker, Walter William
Becking, Edward Phillip
Beckman, Jerome Francis
Beerbohm, Bertram
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Beets, Mary Mitchell
Begg, Ian Maynard
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Goehring, Dwight John
Goer, Marvin H.
Goerwitz, Kurt
Goldberg, Elaine Marcia
Goldberg, Frank
Golden, Frederic
Goldenberg, Edward Elliott
Golding, Stephen Lee
Goldman, Irle M.
Goldman, Lawrence Allen
Goldman, Mark Stuart
Goldmann, Trudie Silman
Goldmintz, Martyn Mazarus
Goldsmith, Lisa A.
Goldstein, Marvin William
Goldstein, Robert Howard
Golinkoff, Roberta Michnick
Golladay, Wendy McKee
Golmieh, Marwan Shawki
Good, Kenneth John
Goodfellow, Reginald A. H.
Goodlett, Ruth Cole
Goodman, Leonard Henry
Goodwin, Alan Richard
Gordon, Alice Marie
Gorman, Ira Ronald
Gorovitz, Judith B.
Gottesman, Milton
Gottlieb, Jay
Gottlieb, Kenneth Ralph
Gottlieb, Michael C.
Gould, Warren Robert
Goyette, Charles Henry
Graeber, Raymond Curtis
Graham, Effie Irene A.
Graham, William Wallace
Grant, Frank E.
Gray, Murray L.
Greco, Tommy Samuel
Green, Charles B.
Green, Duane
Green, Marilyn Hunter
Green, Paul Casey
Green, Thomas Kerr
Green, Walter B.
Greene, Les R.
Greenwood, Charles Reed
Gregory, Mary Kathryn
Gregory, Robert James
Grellong, Bruce Arvid
Greth, David Lewis
Gries, Leonard Todd
Griffiths, Roland Redmond
Gripka, Frank J.
Grippin, Pauline Carl
Groeschel, Benedict J.
Gross, David Edward
Gross, Susan
Grossman, Marlyn
Grow, Richard T.
Gruber, Alan Robert
Guarnaccia, Vito John
Guenzel, Quentin Gregory
Guinagh, Barry Joseph
Gurney, Robert Peter
Guten, Sharon Marcia
Gutkin, Daniel Choda

Haber, Lawrence Charles
Haddock, Raf
Hadersberger, Gunter Rudolf
Hafer, Marilyn
Hagerman, Robert Albert
Hain, Tony
Haines, James Richard
Haladyna, Thomas Michael
Haley, Hugh John
Hall, Richard Barton
Hall, Richard Van
Halpin, Gelnnelle
Halpin, Gerald
Hamby, Russell R., Jr.
Hamlin, Paul Herbert
Hammer, Henry Burton, III
Hammes, Richard George
Hammond, Ralph B.
Hamner, Walter Clayton, Jr.
Hamrick, Diane D.
Hands, Donald Raymond
Hannay, Hilda Julia
Hannon, Noelle Patricia
Hansen, Lorraine Sunny
Hansteen, Ralph William
Harbaugh, Garry Leon
Hariton, E. Barbara
Harold, Beatrice Joy
Harreld, Jerry Douglas
Harrell, Ernest Harvey
Harris, Marilyn E.
Harris, Stephen G.
Harrison, Dorothy Toomey
Harrison, Patrick Ryan
Hart, Lance Roderic
Harvey, Eric R.
Harvey, Mary Ruth
Hatcher, John Chris
Hatt, Mary Jane
Haugk, Kenneth Charles
Haven, Howard J.
Haxel, Bobby Joe
Hayden, Brian Charles
Hayes, Nancy A.
Haynes, Stephen Neil
Hecker, Benson
Hedgeman, Betty Syracuse
Hedlund, Carolyn Sue
Heilman, Madeline E.
Heisler, William John
Heitler, James Benjamin
Heller, Jack Freeman
Hellman, Betty

Helwig, Howard Drew
Hemmes, Nancy Shaw
Hendel, Darwin Dale
Henderson, George L., Jr.
Henderson, Harold L.
Hennessy, James Joseph
Hensley, Michael Lee
Herman, John Harvey
Herschman, Philip Lee
Hertzog, Jane
Hess, Lee Raymond
Hesson, John Edward
Hicks, Charles J., Jr.
Hicks, Jeffrey Louis
Higgins, William Hart
Hill, Rachel Tallen
Hill, Robert Emerson, Jr.
Hillabrant, Walter John
Hilliard, Ronald Devere
Hilliard, Ted Richard
Himmelsbach, Joseph Thomas
Hines, David Arnold
Hinkel, Thomas Joseph
Hinrichs, Theodore Lynn
Hippe, Douglas Lee
Hiroto, Donald Shiro
Hirschman, Richard David
Hnatczuk, Taras
Hoblit, Pamela Reynolds
Hock, Ellen Irene
Hodges, Patricia M.
Hoeckele, Carol R.
Hoff, Louis Alexander
Hoffman, Alan Montel
Hoffman, Charles David
Hoffman, Irwin Z.
Hogsett, Stanley Gordon
Hokenson, Maura Esther
Holden, William Joseph
Hollan, James Davidson
Holland, Terrill Robert
Holmes, Helen Lucile
Holmes, Paul Whitten
Holmstrom, Valerie Louise
Homa, Donald Lee
Hommen, Donovan LeRoy
Honor, Stephen H.
Hopkins, Neil Robert
Hopkins, Samuel Wallace, Jr.
Hoppe, Richard Brian
Horn, Charles Heckert
Hornik, John Alan
Houlihan, John Patrick
Hoyer, Francis Wilcox
Huang, Lily Chu
Hubbert, Ardelle K.
Huber, R. John
Huff, Gerald Wallace
Huff, Kathleen
Hughes, Daniel Anthony
Hughes, Fergus P.
Hughes, Rosemary Anne
Hull, Margaret
Hultman, Kenneth Edward
Hunt, Stanley Moore, Jr.
Huntsman, Nancy Joy
Huston, Ted Laird
Hutchison, Bruce Delbert
Hutson, Barbara A.
Hyde, Elizabeth Meyers

Ijams, Donald Sheldon
Iman, Stephen Charles
Imber, Steve C.
Ingle, Ronald R.
Ingwell, Richard Hamlin
Ireland, Robert Ellis
Irey, Larry D.
Isen, Alice Marian
Isett, Robert David
Israel, Sherry R.
Iverson, Sonya Rae
Iwai, Charles T.

Jablonski, Eugene Michael
Jackson, Lee Anderson, Jr.
Jacobs, Bertram H.
Jacobs, James Walter
Jacobson, Marsha Bruckner
Jacobson, Robert Kerry
Jacoby, Larry Leroy
Jaffe, Michael L.
Jalkanen, Arthur Wilhelm
Jalowiec, John Edward
James, Charles Robert
Janda, Louis Hugo
Jandt, Fred Edmund
Janisse, Michael Pierre
Jansky, Jeannette Jefferson
Jansma, Theodore John, Jr.
Jeffs, J. Dale
Jeghelian, Alice
Jenkins, Barbara B.
Jepson, Peter
Jesteadt, Walt
Joanning, Harvey H.
Joe, Victor Clark
Johanson, Chris Ellyn
John, Miriam
Johnson, Douglas Warren
Johnson, Elmer LeRoy
Johnson, Eugene Bernard
Johnson, Eugene H.
Johnson, James Harding
Johnson, Jean Elaine
Johnson, Kline Wilson
Johnson, Lois Powell
Johnson, Patrick Joseph
Johnson, Raymond Howard
Johnson, Ronald Bruce
Johnson, Thomas Frank
Jones, D. Whitman
Jones, Duane Lyle
Jones, Jeanne Louise
Jones, Philip Arthur
Jones, Randel Royce
Jones, Wylie Coleman, Jr.
Jordan, Jesse Joseph
Jorgensen, Gerald Thomas
Joslyn, Wallace Danforth
Julia, Gwen Woodiwiss
Juliano, Daniel B.
Juola, James Frans
Jurich, Anthony Peter

Kachorek, John Joseph
Kaftan, Robert Eugene
Kagey, John Robert
Kahan, Betsy Berk
Kahn, Irwin
Kalish-Landon, Nancy

Kambach, Marvin Carl
Kane, Robert Louis
Kanov, Jeffrey Francis
Kanzler, Alfred Wilhelm
Kaplan, Paul Stanley
Kaplan, Robert Malcolm
Kapur, Veena
Karagan, Nicholas James
Karan, Val Elliot
Karlson, Alfred Lennart
Karpowitz, Dennis Hanks
Kaslow, Florence Whiteman
Kass, Warren Albert
Katrin, Susan Ellen
Katz, Jeremy Milton
Katz, Michael Martin
Kaufman, Kenneth Phillip
Kayser, Brian Duolem
Keahey, Scott Paul
Keating, John Patrick
Kegley, John Franklin
Kellett, Deborah Sharon
Kello, John Edward
Kellogg, Robert S.
Kemler, Deborah Glaser
Kemmerling, Robert G.
Kennedy, James Lester
Kent, Martha
Kidder, Louise Hoch
Kier, Rae Jeanne
Kilburg, Richard Robert
Kime, Ronald Grant
King, Glen David
King, Robert James
King, Virginia Marie
Kinghorn, Carol Ann
Kinney, Phyllis Elaine
Kirkpatrick, Sue Wilson
Kirshner, Sheldon G.
Kissler, Gerald Ray
Klass, Ellen Tobey
Klatzky, Roberta Lou
Klau, Linoa
Kleiber, Douglas Alfred
Klein, Harvey Allen
Klein, Richard Stanley
Klein, Stephen Barry
Kleinpeter, William Jerome, III
Kliger, Clifford M.
Kline, Donald William
Klinger, Nancy N.
Kloss, Marie Guzell
Kluger, Joseph Herman
Klukken, Philip Gary
Kluttz, Jean Swink
Knapp, Susan Carol
Knour, Arthur
Knowlton, William Ruemke
Knox, George Willard
Koenig, Frances Salman
Koenigs, Robert James
Koenigsberg, Riki Shareman
Kokes, Ronald Francis
Konanc, Judy Ann
Kondas, Michael Joseph
Kondrasuk, John Norton
Konecni, Vladimir J.
Kopel, Kenneth Fred
Kopp, Richard Royal

Kops, Victor Barry
Koran, John Joseph, Jr.
Koski, Charles Henry
Koslowsky, Meni
Kovach, Joseph K.
Koven, Marvin Vernon
Kozloff, Martin Alan
Kozma, Frederick, Jr.
Kradel, Paul Franklin
Kraft, Thomas D.
Kraushaar, Alan H.
Kress, Gary
Kressel, Kenneth
Kribs, Nancy E.
Kroeker, Leonard Paul
Krohn, Alan Stephen
Krupski, Antoinette
Kuhner, Susan Mary
Kuldau, Janice Elaine M.
Kuldau, Von Dean
Kulik, James Alan
Kulkin, Alan Leonard
Kurtz, Paul David
Kusick, James Joseph

Laabs, Gerald Joseph
LaBach, Patricia Avery
Labrentz, Helmut L.
Ladouceur, Robert
LaFave, Francis Earl
Lambert, E. Warren
Lambert, Ruth
Lamkin, Billy Dan
Landes, Herbert Rodney
Landon, Philip Bruce
Lane, Patrick Richard
Lang, Jo
Lantz, Alma Esther
LaPat, Patricia Cheryl
Lappert, Richard E.
Lasky, Judith Faye
Lasky, Richard Donald
Latane, Julie Gatewood
Lathey, Jonathan Wells
Lauricella, Salvatore Daniel
Lautin, Devora
Lavin, Michael John
Lawrence, Charles Edward
Layne, Michael
L'Ecuyer, Rene
Ledyard, Mary Jane
Lee, Albin L.
Lee, James Marvin
Lehman, Jerry Dean
Lehman, Robert Edwin
Leider, Jerome
Lemond, Luther Charles
Lenchitz, Kenneth
Leon, Janet Estelle
Leonard, Hamlin Skipton
Leonard, J. Calvin
Lepore, Franco
Levendusky, Philip George
Leventhal, Leslie Y.
Levin, Ellen Margaret
Levin, Harvey Steven
Levine, Michael M.
Levine, Steven Eliot
Levy, Marvin Michael
Lewis, Judy Diane

Lewis, Kirsti
Lewis, Lawrence David
Lewis, Martha R.
Lewis, Mary Ann
Lewis, Ronald Frank
Liberson, Cathryn Walters
Lieberman, Jack Noel
Lieberman, William
Liebert, Ann Marie
Lin, Shie-fang
Lindberg, Frederick Hjalmer
Lindoerfer, Dennis Lee
Lindskoog, Donald Philip
Lindsley, James Romaine
Lipoff, Dennis Alan
Lipsitz, David Alan
Lisman, Stephen Alan
Lissitz, Robert Wooster
Little, Douglas Marion
Littman, Isabelle
Liu, An-Yen
Livingston, James Wesley
Lloyd, James Bernard
Lloyd, Kenneth Louis
Loe, Barbara
Lombardi, Joseph S.
Longin, Harold Elliott
Lopez-Roig, Lucy Enid
Lord, Sharon Burmeister
Lorenz, Jerome Robert
Lorion, Raymond Paul
Loshak, Lee Joel
Lott, Daniel Ralph
Lowe, Nancy Jane
Lowe, William R., Jr.
Lowenhar, Jeffrey Allen
Lower, William F.
Lowman, Robert Paul
Lowther, Barbara Doty
Lubershane, Melvin Michael
Lubin, Marc I.
Lucow, William H.
Ludwigsen, Kris Rahbek
Luek-Keen, Susan Penelope
Lunceford, Ronald D.
Lusterman, Don-David
Luther, Grace Ann
Lutzker, John Robert
Lycette, William Hugh
Lyle, William H.
Lynch, James J.
Lynch, N. Timothy
Lyon, Carla Susan
Lyon, Rebecca Saxon

Mac, Roddy F.
Mach, Richard Stuart
MacPhail, Robert Campbell
Madden, John Thomas
Mahan, James Maurice
Mahler, Clifford R.
Mahr, Ila Ruth
Maish, James Ivan
Maitland, Suzanne DePree
Makarowski, Louis Michael
Maki, Ruth Hipple
Malcolm, Daniel Bruce
Malin, Jane Tanner
Mallenbaum, Victor
Mallory, William Allen
Malnati, Richard John
Malon, Scott Vernon
Malsbury, Charles Wendell, Jr.
Mandelcorn, Berenice Secter
Mangelsdorff, Arthur David
Mann, Howard H.
Marchbanks, Gabrielle E.
Marcus, Lee Melvyn
Mardell, Carol D.
Marek, Edward William
Marinelli, Robert Patrick
Markiewicz, Dorothy
Markle, Allan
Markowitz, Howard Jay
Marks, Lawrence E.
Markson, Jordan Quentin
Marler, Peter Robert
Marolla, Francis Anthony
Marquette, Carl Henry
Marshall, Karol L. Anderson
Martin, Cynthia Dyee
Martin, Dale Alison
Martin, Donald Felix
Martin, Garry Leonard
Martinez-Urrutia, Angel C.
Martorano, Raymond Dennis
Marx, David James
Mason, Elizabeth Jane
Mass, Roslyn
Massar, Barbara Anne
Massey, Ronald John
Masterson, Jill
Mathews, Kenneth E., Jr.
Matranga, James Thomas
Matthews, Charles Odell
Maudal-Twedt, Gail R.
Maurer, Charles David
Maurer, Patti Ann
Maxwell, Mary Gay
Mayer, Richard Edwin
Mays, Roy Mark, Jr.
McAllister, Ann Dunn
McCants, Alice Jane
McCarthy, Mary Anne V.
McCarthy, Mary Elizabeth
McCarthy, Michael
McCarthy, Robert James
McCarver, Ronald Bevill
McCary, Patrick Wellington
McCauley, Clark Richard, Jr.
McClannahan, Lynn E.
McClennen, Sandra Elaine
McCracken, N. Patrick
McDonald, Charles Thomas
McDonald, Thomas Fletcher
McGaffey, Thomas Neal
McGinnis, Ruth L.
McGourty, David Phillip
McGowan, Raymond Peter
McGurris, Martin Charles
McHale, Carol Rishel
McHenry, Thomas Bruce
McHugh, William Thomas
McKain, Ann Elizabeth
McKeown, Douglas O'Neal, Jr.
McMahan, Ian Douglass
McMahon, Carol Ellen
McMahon, Judith Wantland
McMillan, John C.
McNamee, Sharie
McNeil, James Henry
McNeil, Michael
McNeill, Earle Daniel
McTague, Kenneth Edward
McVey, Ronald F.
Meacci, Frank, Jr.
Meacham, John Allen
Mead, Philip Griswold
Mehlman, Anita Cooper
Mehnert, William Otto
Meid, Elizabeth S.
Meier, Robert David
Melamed, Leslie
Melby, David John
Meliker, Martin Larry
Melnekoff, Philip
Mendel, Raymond Max
Mendell, Dale M.
Mermis, Bernie Joseph
Merritt, MacAllister
Merryman, Coleman T.
Meskin, Bonnie Beth
Messmore, David William
Metcalf, Frederick Ulmer, Jr.
Metelli, Fabio
Meyer, Mary Ann Therese
Meyer, Robert Leonard
Meyers, Bonnie Marie
Meyers, Marvin D.
Meyers, Mary Joyce
Miczek, Klaus Alexander
Middleton, Brian Arthur
Millar, Richard Dykes
Miller, David Bennett
Miller, Duane Ivan
Miller, Felice Susan
Miller, Jill M.
Miller, L. Keith
Miller, Lawrence Robert
Miller, Max Donald
Miller, Michael Foster
Miller, Peter Nathan
Miller, Robert William
Miller, Shirley Anne
Miller, Thomas Joseph
Miller, William Gene
Millham, Jin
Minnigerode, Fred A.
Minton, Judith Marion Haber
Mintz, Paul M.
Miron, Nordechai
Misner, Marilyn Spitler
Misra, Rajendra Kumar
Mitchell, David Eaton, III
Mitchell, Marlene
Mitroff, Ian Irving
Moeller, Theodore Allen
Moely, Barbara E.
Molineux, J. Bailey
Monaco, Donald Alphonse
Monahan, John Thomas
Monat, Jonathan S.
Monk, Marguerite R.
Montgomery, Doil D.
Moore, Joseph A.
Moore, Mark H.
Morado, Carolyn
Morgan, Cyril Percy, Jr.
Moriarty, Daniel D., Jr.
Morris, John E.
Morrison, Richard Lynn
Morrison, Thomas L.
Moser, Richard John
Muchinsky, Paul Michael
Muhich, Dolores Rose
Muhlenkamp, Ann F.
Mukherjee, Subash Chandra
Muktarian, Herbert H.
Mulligan, William Lawrence
Mullins, Rebecca Jean
Mullozzi, Anthony Dominick, Jr.
Mundorff, Jan Edward
Munford, Paul Rochester
Munger, Arlene Lorraine
Munoz, Leo
Murphy, Wilton W.
Murray, Catherine Savino
Murray, John Ralph
Muti, Matteo Renato
Myers, John Arthur, Jr.
Myers, Stanley Baker
Myrow, David Louis

Naffziger, Claudeen Cline
Nardi, Anne H.
Nathan, Susan Wimmer
Naus, Mary Jeanne
Naviaux, LaRee DeVee
Nebeker, Delbert M.
Nebes, Robert David
Nedd, Albert Neville Byron
Nedelsky, Michael George
Nelson, Barbara Ann
Nelson, Kenneth Omer
Nelson, Rosemery Orlene
Nemeth, Darlyne Gaynor
Nester, Mary Anne
Neuringer, Martha DiNardo
Nevotti, Joseph Richard, II
Newby, Robert Wayne
Newman, Alexander
Newman, Barbara Miller
Newman, John Edwin
Newton, Elizabeth Elley
Nichols, Michael Paul
Nichols, Robert Charles
Nickel, Ted W.
Niewiadomski, Michael Z.
Nisenson, Roy Arthur
Njaa, Lloyd Johan
Nobles, Brenda Hudson
Noeth, Richard Jerome
Norbert, Nancyann Mardin
Norman, Kent Leigh
Norton, A. Evangeline
Norvilas, Algis A.
Nygard, Melissa Farley
Nystrom, Paul Clifdon

Oakes, Elizabeth Street
Oaklander, Harvey
O'Connor, Donald Russell
Oden, Chester Wayne, Jr.
O'Donnell, Joseph Neil
Oeltjen, Paul D.
Offir, Carole Eve

Ogland, Vanetta S.
Ognibene, Gerald L.
Ohlson, Edward LaMonte
O'Keefe, Beth Egan
O'Keefe, Garrett James, Jr.
O'Keefe, Kevin Joseph
O'Keefe, Rip L.
Olczak, Paul Vincent
O'Leary, Brian Shaemus
O'Leary, Sandra E.
Olejnik, Anthony B.
Oliver, Rose
Olivero, Gerald
Olmedo, Esteban Lucas
Olson, Lawrence Richard
O'Mahoney, Michael Terrence
Onwuegbu, Obed Isigwuzo
Orenstein, Louis Jay
Orenstein, Rosanne
Organist, James E.
Osborn, William Powell
Osborne, Francis Hoy
Osterlund, Blair Lee
Otaala, Barnabas
Overton, Tom Dave
Owen, Thomas R.
Owen, Wayne L.

Paelet, David
Page, Robert Arthur
Painter, Genevieve
Paloutzian, Raymond Frank
Paris, Scott George
Parish, Thomas Scanlan
Parker, David E.
Parker, Janat Fraser
Parker, Lawrence Craig, Jr
Parker, Tom Solomon
Parkman, John M.
Parks, Eldon Roy
Parrish, Robert Neil
Patterson, Arthur Howard
Pattie, Currell Lee
Patton, Thomas Charles
Patty, Rosemarie Anderson
Paul, Daniel A.
Pearne, Sally Ann
Pearson, Robert Edwin
Pennington, Florrie McCollum
Pentecoste, Joseph C.
Pereboom, Margaret G.
Perlowski, Stephen Francis
Perrie, Ben
Perry, David George
Pert, Agu
Peskay, Joel
Peter, Hollis Michael
Peters, Bernard Albert
Peters, David Steven
Peters, Richard Douglas
Petersen, Anne C.
Petersen, Peter Barron
Peterson, Gerald L.
Peterson, Judith Austin
Petri, Herbert L.
Pettinelli, James Douglas
Pettit, Iren Bernadicou
Pfeifer, Charles Michael, Jr.
Pfeifer, W. Dean
Phelan, Thomas Warren
Phifer, Mary Kaye
Phillips, Florence Louise
Phillips, James Killis, III
Phillips, John MacKenzie
Phillips, William Daniel
Pierce, Robert Allyn
Pierson, Stephen Charles
Pines, Harvey Allan
Pittman, Thane Sibley
Pizer, Stuart Alan
Platt, Alice Brill
Platt, Jane Habiger
Plaut, S. Michael
Politzer, Rita Jean
Polonsky, Ira Eugene
Poon, Leonard Winston
Porzemsky, Jerry Marvin
Posnansky, Carla Joy
Postman, Robert Joel
Poulos, Rita Wicks
Powell, James Everett
Powell, Jay R.
Powell, Joann
Powell, Peter Odgen
Powers, Sandra Matthews
Pratap, Vijayendra
Pratt, Linda K.
Pressman, Dorothy Elaine
Price, Frances Fogel
Price, Judy M.
Price, Roslyn Marilyn
Pricer, Ryness Scott
Prigatano, George Patrick
Priggie, Nan Kathryn
Pringle, Marlene Bence
Pritchard, David A.
Pucel, John Charles
Purviance, Carlton William
Pusser, Henry Ellison
Putterman, Allan Howard

Quinn, Philip Francis
Quirk, Michael Patrick
Quiroga, Isidro Ramon

Raftery, John P.
Raia, James Richard
Rall, Marilyn Louise
Ralph, George S.
Ramayya, Divakaruni P.
Raminez, Augustine J.
Ramos, Franklin
Ramp, Eugene A.
Randazzo, Andrew F.
Randhawa, Bikkar Singh
Randolph, John Gordon
Ransom, Donald Charles
Raque, David Warren
Rath, Frank H., Jr.
Rathbone, Patricia S.
Rathjen, Diana Pickett
Ratto, Charles Richard
Rausch, Lyle J.
Ravensborg, Milton Robert
Reckseit, Joan Fleischer
Redding, Gordon M.
Redmon, Annie Louise
Reed, Cheryl Lee
Reeder, Darrell Allan
Reeves, Joann Ondrovik
Reich, Michael J.
Reiff-Ross, Eleanor Roslyn
Reilly, Charles Vincent
Reiner, Charles Alan
Reinheimer, George Edward
Reisinger, James John
Reisner, Phyllis
Reiss, Steven
Renwick, Patricia Ann
Rest, James Ronald
Rhodes, Jon Glenn
Ricci, Anthony Michael
Rich, Manny
Richards, Ann Bruegel
Richardson, Leroy Parker
Richardson, Mary Sue
Richert, Alphons John
Richman, Lynn Charles
Riley, Terry Michael
Roberts, Ada Lorean
Roberts, Gilbert L.
Roberts, Gustave William
Roberts, John Milton
Robertson, Elizabeth Anne
Robinson, John David
Robinson, Margery LeVern
Robinson, Valda Mock
Robison, Susan Miller
Rodeheffer, Martha Ann
Rodgon, Mari Carole Monitz
Rodin, Judith
Rodway, Margaret Rebecca
Roehlke, Helen Jean
Roff, James David
Rogers, Mary Brown
Rom, John Donald
Ronen, Simcha
Rose, Donald Bruce
Rose, Gerald L.
Rose, Lucien Devon
Rose, Robert Gaylord
Rose, Roger Dean
Rosen, Ira Joel
Rosen, T. John
Rosenbaum, Robert Mark
Rosenfield, Richard Ullman
Rosenthal, Bailie Ann
Rosenthal, Irwin
Rosner, Phillip Ernest
Rosoff, Nina
Ross, Michael Arthur
Rothman, Golda Reena
Rothstein, Sergio
Rotton, James Gale
Rouse, Bobby Gale
Roy, Michael Aaron
Ruby, Larry Lee
Ruch, John Clifford
Ruderman, Gerald H.
Rudman, Seymour Lipman
Rudow, Edward H.
Rueth, Thomas William
Russ, Sandra Walker
Russell, John Curtis
Russell, Katherine Endress
Russell, Richard Keith
Rust, James Over
Rutner, Ivan Toby
Rywick, Thomas
Sabatasso, Anthony Peter
Sabourin, Michel Edouard
Saccuzzo, Dennis Paul
Sachs, Donald H.
Sack, Sallyann Amdur
Sadler, Orin Winslow, IV
Safran, John S.
Sakitt, Barbara
Salamy, A.
Salas, Dominador Justiniano
Salisbury, Jerry Lee
Sallade, Jacqueline Belle
Sallows, Glen Owen
Salmon, Lenin Eduardo
Samaan, Anne Jorjorian
Samaan, Makram Khalil
Sambursky, Joel H.
Samsky, Jeremy Jacob
Samuelson, Wayne Donald
Sandberg, Salek
Sandel, Lenore
Sandler, Howard Mark
Sanford, Mary Eve
San Luis, Roberto Ruiz
Santoro, Samuel, Jr.
Sarafino, Edward P.
Sarver, Gary Steven
Sasson, Ralph Yves
Saunders, Antoinette
Savitsky, Jeffrey Charles
Savlov, Steven Michael
Sawtell, Rodney Oral
Sayner, Richard Brian
Scagnelli, Paul
Scarpetti, William Louis
Schack, Mary Lou
Schadler, Margaret
Schad-Somers, Susanne P.
Schaedler, Thomas J., Jr.
Schaefer, Susan Davidson
Scharf, Kathleen R.
Scharles, Winifred Warnat
Schauer, John Richard
Scheaf, William Allen
Scheckenbach, Albert F.
Schenkel, Susi
Scher, Gloria Simon
Schevill, Helen Shaner
Schlossberg, Harvey
Schmeltz, Robert
Schmidt, Lanalee Carol
Schneider, Thomas Erwin
Schoenenberger, Joseph Christian
Schofield, Leon Joseph, Jr.
Schulman, Martin Alan
Schumacher, Ruth B.
Schwartz, Theodore William
Schwenk, Mary Ann
Scorzelli, James Francis
Scott, Craig Stanley
Scott, John F.
Scott, Joseph Augustine
Scott, Richard D.
Scott, W. Paul, Jr.
Search, Paul F.
Seder, Paul
Sedgwick, Harold Arthur
Seeley, Mark Stanley
Seeman, Stephen J.

Sell, John Michael
Semb, George B.
Sepe, Robert Francis
Setne, Verlis Louise
Schaffer, Ronald William
Shallman, William Samuel
Shannon, Lael Whiting
Shanteau, James C.
Shapiro, Alvin Harvey
Shapiro, Bernadette Nelson
Shapiro, Lillian P.
Shapiro, Michelle K.
Shapiro, Ralph
Shappell, Dean Leroy
Share, Joseph H.
Sharigian, Kenneth Joseph
Sharma, Satanand
Sharma, Vijay
Sharpe, Theodore Miller
Shatz, Marc D.
Shaw, Sandra Jane
Sheehan, Peter Winston
Shegina, Frank Strauss
Sheldon, Sharon Sweeney
Shepela, Sharon Toffey
Sher, Abigail Benton
Sheras, Peter Loren
Shere, Stephen Howard
Sherman, Paul Stephan
Sherman, William Mark
Sherrod, Kathryn Brown
Shields, Dianne Sue
Shipley, Charles Raymond
Shipley, Robert Hopkins
Shippen, Samuel Joseph
Short, Gordon R.
Shove, Georganne R.
Shtasel, Thelma Flanders
Shulman, Arthur Joseph
Siebenthall, Curtis Alan
Siebold, James Robert
Siegel, Louis
Sieka, Frank Louis
Siggers, Kathleen W.
Sikes, Walter Wallace
Silberberg, Alan Mark
Silver, Avrum Irwin
Silver, Carl
Silverman, Wayne Paul
Silverstein, Leonard
Silzer, John C.
Simeon, Jovan G.
Simminger, Steven E.
Simmonds, David Wayne
Simmons, Jack Warren
Simos, Alexander Thomas
Singer, Anne
Singer, Marc Geoffrey
Siperstein, Gary N.
Sitton, Sarah Clydette
Skotdal, Nicki Marlene
Skovlolt, Thomas Meyer
Skultin, Richard
Slavin, Malcolm Owen
Sletten, Paul Melvin
Sloggett, Barbara Burkitt
Slosky, Ronald Jay
Smith, Darrell
Smith, Edward Douglas
Smith, Katharine Chapman
Smith, Patricia Mary
Smith, Richard Michael
Smith, Robert L.
Smith, Sandra L.
Smith, Sara Dawn
Smutz, Edwin Randall
Snapper, Kurt John
Snett, Phillip Irwin
Solhkhah, Nariman
Somerville, Kevin Edward
Songer, Elaine Cathcart
Sorem, Anthony Milton
Sorkin, Robert Daniel
Sosis, Ruth Hyland
Space, Lawrence Grant
Spangler, Robert Stephen
Speare, Jonathan
Speeth, Sheridan Dauster
Speidel, Gisela Elisabeth
Spencer, Lyle M., Jr.
Spencer, W. Boyd
Sperber, Doreen Faye
Sperling, Kenneth
Spielberg, Deanna B.
Spielbichler, Otto
Spierling, Thomas Wayne
Spiess, Eugene Robert
Spinetta, John Joseph
Spink, James Patrick
Spitalnik, Robert Steven
Spitzer, Tam Mackenzie
Springer, Sally Pearl
Sroges, Richard Walter
Stang, David James
Stanley, Charlton Sumner
Stanley, Larry Daniel
Stansbury, Clayton Cresvell
Stapleton, LeRoy Earl
Stark, James Jehu, Jr.
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Statt, David Alexander
Steele, Galen H.
Steffen, John Joseph
Steger, Joseph Michael
Stein, Earl S.
Stein, Gerald Michael
Stein, R. Timothy
Stern, Myron Bud
Sternberg, Daniel Paul
Sterneck, Rosalie L.
Stevens, Ashley
Stevens, Thomas Granville
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Stewart, Robert John
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Stick, Sheldon Leon
Stiggins, Richard John
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Stockton, Rex A.
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Stone, Gerald L.
Stone, Patricia A.
Storm, Roger Lee, Jr.
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Sullivan, Roy F.
Suls, Jerry M.
Super, Charles McAfee
Surwit, Richard S.
Suslovich, Eli
Sussman, Marion Baum
Sutherland, Steven Harry
Swallow, Carolyn S.
Swan, Gale Allen
Swan, James Albert
Swanson, Terry Rusk
Swap, Susan McAllister
Swartz, Virginia Diane
Swift, Carolyn Frances

Taber, Thomas Dean
Tamayo, Alvaro
Tanney, Mary Faith
Taplin, John Eaton
Tapp, George S.
Taylor, Gary Alan
Taylor, Gary G.
Taylor, Mary Glen
Tchack, Estelle B.
Teal, Janice Rebecca
Tedesco, John Fredric
Tedoff, Ira Lewis
Telaak, Kathleen Ann
Temple, Kathe D. L.
Tennyson, Robert Duane
Tepper, Donald T., Jr.
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Thall, Marcella Martha
Thomas, Clifton Davis, Jr.
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Thomas, Jack D.
Thomas, John Bruce
Thompson, Katherine Seric
Thompson, Lois Lavender
Thurman, Clarence
Tice, Thomas Evert
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Todd, Theriault Jeanne
Tokayer, Sidney S.
Toner, Ignatius Joseph
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Toto, Samuel Eugene
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Travers, Jerome A.
Tresemer, David Ward
Trimakas, Kestutis A.
Trimble, Joseph Everett
Trimboli, Frank
Trost, Ronald Charles
Truxaw, Kathleen R.
Tryon, Georgiana Shick
Tucker, Michael Floyd
Tucker, William Harris
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Typpo, Marion Henrikson
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Tzeng, Ovid Jyh-Lang

Udick, William Simms
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Unger, Hanne
Urda, Michael Andrew

Vail, Ilona M. M.
Van Couvering, Nancy Victoria
Vanderveer, Richard Brent
Vannicelli, Marsha-Lee
Van Slambrouck, Paul Francis
Van Slambrouck, Suzanne Lee
Van Son, Levinus George
Van Wye, Rodman F.
Varadi, Marilyn Meyrow
Varley, William Hugh
Vaughn, John Porter
Velnosky, E. Charles
Venham, Larry Lee
Vidler, Derek Charles
Viehman, Evelyn Huffman
Vitale, Michael R.
Vockell, Edward Louis
Vogel, Irene Susan
Vogel, Juliet Marcia
Vogel, Roberta Burrage
von Glasersfeld, Ernst Christian
Vu, Catherine Thu-Huong

Waag, Wayne Leslie
Wack, Dennis L.
Wackwitz, John H.
Walker, James Paul
Walker, Lenore E.
Walker, Lilly Julia Schubert
Walker, Steven Harold
Wall, Joan Barrett
Wallach, Steven David
Walseth, Hannah Kristine
Walsh, Harold Haldeman
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Walter, Donald A.
Walter, Timothy Lewis
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Wang, YuTsun George
Ward, James Stuart
Ware, Joseph Roger
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Warner, Joan Marie
Warren, Brian Edgar
Waterman, Donald Arthur
Waters, Robert Michael
Watson, Robert Kimball
Wayne, Marvin R.
Weber, Stephen John
Weber-Levine, Margaret L.
Webster, David Roger
Weidemann, Sue
Weiner, Elayne J.
Weiner, Hubert
Weinrach, Stephen G.
Weinstock, Andrew I.
Weinstock, Comilda Sundeen
Weintraub, Joseph R.
Weisman, Robert Lee
Weiss, Arthur N.
Weiss, Thomas
Wellens, Albert Rodney
Wener, Albert E.
Werblo, Dorothy Funk
Werner, Jan A.
West, Cathy Maynard

Westman, Alida Spaans
Westrate, Ronald Martin
Wetter, Berwyn Duane
Wexler, Michael
Whaley, Donald Lee
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Wheeler, Gloria Eileen
Wheeler, Kirk Edwin
White, Brent Clark
White, Charles Borden
White, Geoffry D.
White, Helen Muriel
White, William Joseph
Whitehead, George Ira, III
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Whitman, Russell Douglas
Whittaker, James Kevin
Whittle, Reed L.
Widseth, Jane Christina
Wiesen, Lawrence E.
Wilcove, W. Gregg
Wilcox, Anne Herndon
Wilcox, Sandra Galdieri
Wildblood, Robert William
Wilde, Roger Kenneth
Willard, David Lee
Williams, Charles Milton
Williams, Claudia Cowan
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Williams, Norwood Lewis
Williams, Robert Forest
Wilson, Charles Chase
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Wilson, William Herman
Win, U. Kyaw
Windmiller, Myra Bailey
Wine, Jeffrey J.
Wiseman, Neal Lawrence
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Wodarski, John Stanley
Wolfe, Marshall
Wolfinsohn, Lawrence S.
Wolfson, Abby
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Wolk, Stephen
Wolowitz, Howard Martin
Wolz, William T.
Wombacher, Kristin
Wong, Paul Tsai-Pao
Woods, Donald Joseph
Woodward, M. June
Woolfolk, Robert Lee, IV
Work, Murray Smith
Wynne, Bayard Edmund
Wynne, Ronald David
Wyrick, Richard Arthur
Yaffe, Fred Larry
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Yu, Miriam
Yunker, Gary Warren

Zabriskie, Joseph William
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Zepelin, Harold
Zerface, James Phillip
Zevin, Barbara S.
Ziegenfuss, Penny Low
Zimmerman, H. Dale
Zimmerman, Joel Stephen
Zuckerberg, Joan
Zussman, Shirley
Zwibelman, Barry Bruce

Associates

Abarbanel-Cremona, Irene
Abbondanza, James Michael
Abikoff, Howard Barry
Agarwal, Mary Glenn
Aharonov, Nily
Ahmad, Khwaja Mahmood
Ahrens, John B.
Allison, Vernon Lee
Amato, Grace Helene
Anand, Shashi
Andersen, Brent Lewis
Anderson, Laura Kate
Andrews, Charles Richard
Andrews, Frank Maxwell
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Antonelli, Patrick Thomas
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Armstrong, James William
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Autio, Diane Jean
Axel, Robert A.

Baker, Gail Lewis
Bakin, Gloria W.
Bakshy, Judy
Balaschak, Barbara Ann
Balko, Wilda Parthenia
Bank, Stephen Howard
Baranski, Z. Alexander A.
Barnard, William Kenneth
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Baron, Richard Leo
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Baumbach, H. Dale
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Bengston, John Konstant
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Berman, Peter Jay
Bermudez, John Manuel
Bernhard, Bernice B.
Bernstein, Beth Anspach
Bertilson, Hal Scott
Biberman, Gerald
Bickman, Louise Danny
Bien, Ellen Carol
Billingsley, Felix Foster
Binder, Barry
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Bloch, James Phillips
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Bloom, Leonard
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Bourke, Frank John Patrick
Bowditch, Dierdra Elizabeth
Boyle, William Francis
Boynton, Dorothy Thompson
Bracy, James Harrison
Bradley, Lois LaRue
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Brickman, Jean Berzon
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Brodsky, Sylvia Arlene
Brown, Janet Marie
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Burger, Jack Albert
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Bush, Robert Joseph
Butler, Timothy F.
Byers, Patricia Jean

Cacy, Jimmy Ray
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Callahan, James Francis
Camargo, Robert John
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Capaldo, Theodora Frances
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Casimono, Judy Roberts
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Charles, Edward S.
Childers, John Harold, Jr.
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Citrenbaum, Charles Michael
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Clark, Karen Noelle
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Cohen, Phyllis Friedman
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Colbert, Anatole Kolodny
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Connors, William Francis
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Conte, Hope R.
Cook, Walter Charles
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Coviello, George Stephen
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Cox, Mary Anne Kellenberger
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Curran, Carole B.
Cusack, Gerald Patrick

Daviet, Charles William
Davis, John Michael
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Davis, Mary Ann
Decker, William Allen
Deinhardt-Mawson, Carol L.
deJarnette, James Edward
Delgado de Torres, Cory
De Martinis, Elizabeth A.
De Mauro, Albert Vito
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Demyun, Stephen Michael
Denis, Ronald A.
Derrickson, Vivian Mae
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Dick, Anita
DiCuio, Robert Frank
DiDio, Daryl P.
Diehl, Luther Albert
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Dilliard, Maxine Kirst

Dillon, Charlotte Ann
Di Masi, J. Theodore
Dimitroff, Michael Lee
Domkus, Lucy R.
Donenfeld, Thelma Levine
Donnelly, Keven J.
Donovan, Patricia
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Downs, John Charles
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Driver, Jerrell Lee
Drozda, Stephen F.
Duffey, James Bernard
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Dufur, Walter H.
Dukin, Timothy W.
Duncan, Io-Aurelia Holland
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Dunn, Grace Smith

Easton, David J.
Edgerly, Robert Francis
Eisenberg, Bonnie Sheila
Eisman, Elena Joy
Ekstrom, Maurice Fred, Jr.
Ekstrom, Robert David
El Ghatit, Zeinab M.
Elias, Eileen Phyllis
Elleby, Ellouise Amelia
Ellis, Eric Harold
Emerson, Patricia G.
Engel, Ned Leonard
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Erk, Robert Richard
Ernst, Ann Robinson
Esparza, Ricardo
Espinoza, Jorge
Esterling, Sandra Miller
Estes, Karen Lewis
Eun, Bong-soo
Evans, Robert Albert
Extrum, Raymond Claude

Falkenstein, Robert Holmes
Fallica, Anthony Joseph
Fannin, Patricia M.
Farley, Susan Greenbaum
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Fenster, Bessie Esther
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Fisher, Alan Thomas
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Fisher, Robert
Fisk, Samuel Hoar
Fitzpatrick, John Christopher
Flaxman, Judith
Floor, Lucretia Jordan
Flor, John Fulton
Flores, Jorge Cruz
Flynn, Virginia Marie
Foat, Carolyn S. Lepper
Fogelman, Diane Lee
Foht, Kym
Foley, Robert Matthew
Follini, Maida Barton
Follini, Paul Louis
Fonseca, Otilia E.
Force, Herman E.
Forester, Wayne Andrew
Forman, Bruce David
Foster, Margaret Elgin
Fox, Robert Howard
Framer, Edward Marc
Francis, Barbara Brown
Franklin, Paula Anne
Frazier, John G., III
Frederiksen, Sharon Jenine
Freiberg, Karen Louise
Frengut, Robert John
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Friedman, Robert Paul
Friedman, Sarah Landau
Funk, Eleanor N.

Gaborick, Carole G.
Gage, Michelle Carey
Gagné, Ellen Dalton
Gallardo, Edgar Joseph
Gama, Elizabeth M. P.
Garemchuk, John Charles
Garfinkel, Toby
Gartenberg, Jeanette M.
Gartman, Naomi J.
Garwood, Samuel Gray
Gary, L. Vivian
Gates, Jerry Delmar
Gavalas, Nicholas
Gaydos, Paul Martin
Gebelein, Eric Steven
Gibson, Ronald Gordon
Gilbert, Lloyd George
Gill, Kathleen Mary
Gilmore, Mildred Gilman
Gilroy, Vivienne Silora
Giovinco, Susan Grace
Glaser, Howard
Goas, J. Allen
Godow, Annette Gamm
Godwin, Penelope Ann
Gold, Barbara Stott
Goldberg, Arthur I.
Goldberg, Daniel David
Goldfarb, Stephen Michael
Goldson, Marvin
Golub, Fred David
Gomez, Beltran Rafael
Goodman, Allen
Goodwin, Ann Jennings
Gordon, Marshall Donald
Gordon, Mathilde
Gordon, Scott Robert
Gore, Elliot M.
Gorman, Paul Gerard
Gottfurcht, James William
Gould, Elaine
Govine, Barbara Freudenthal
Gowen, Jean Wixson
Green, Kathryn Ann
Green, Marcia Nieder
Greenspun, Ira Frank
Grey, Peter Glenn
Grieve, Josephine H.
Griffin, Susan Elizabeth
Gripper, James Kaiser
Grody, Deborah
Groner, Dennis Mark

Haber, Phillip B.
Haddad, Lawrence B.
Hadley, Trevor R.
Hafer, William Keith
Hagenbuch, Lucy Carroll
Halcomb, Jane Frances
Hallett, Kathryn J.
Hamill, Bruce W.
Hamilton, Margaret C.
Hammelman, Morris Ray
Hanby, Richard Thirman
Hand, Brian Chandler
Handlin, Velma Jane
Hanly, Peter Thomas
Harris, Orpha Sherman
Harris, William Howard
Hart, Lorraine A.
Hartmann, Barry E.
Hatcher, Milton Wright
Hausman, Arthur Leonard
Hayden, Teresa
Hayes, Charles David
Haynes, Jack Phillip
Heilbronn, Marybeth
Held, Robert Mark
Henkin, Paul Henry
Henry, James Michael
Henry, Roger
Herbster, Gene A.
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Hershey, Steven Joel
Herzich, Paul Luka
Heydt, Carole Joyce
Hickman, John William
Higginbotham, Howard Nichlos
Higgins, Joseph Patrick
Hirano, Toshitsugu
Hirsch-Frengut, Renee F.
Hittel, Zelda
Hochstadt, Neil J.
Hofferth, Roger Alan
Hoffman, Ludmila Winter
Hoffman, Mark Robert
Hoffman, Michael Peter
Hoffman, Roger G.
Hoffman, Sylvia Joyce
Hoffmann, Manfred W.
Hogue, Glenn E.
Hopkins, Thomas Francis
Horstman, Preston L.
Howard, Oscar Freeman
Hunt, Peter James
Hunter, Beverly Bindseil
Huntington, Jane Mitchell
Hunyady, Joan Helen
Hutchison, Richard Arthur
Huzinec, Carolyn P.
Hyett, Norman Paul

Imar, Terry Ramsey
Irish, Norman Carl
Isaacson, Shirley
Itkin, Edward Samuel
Itkin, Martha Glenn
Ivanoff, Joanne Marie
Izzo, Robert A.

Jacobowitz, M. Alan
James, Jane Emerson
Jamison, Harry Joseph
Janszen, Pearl
Jarvis, Lawrence George
Javian, Margaret
Jaynes, Robert Walter
Jensen, Eleanor Grace
Jensen, Thomas Robert
Jerant, Anthony P.
Johnson, Dennis L.
Johnson, George Carl
Johnson, James Edward
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Jones, Marion Arthur
Joosse, Anamarie Lea
Jordan, Byron Neal
Joslow, Gary Dressler
Joyce, Carole Ann

Kabithe, Daniel Wanjohi
Kahn, Gay Miller
Kaiser, Deborah Arthurs
Kameyer, Norma Charlene
Kamfonik, Allan Anthony
Kaplan, Phyllis Ann
Karafin, Gail R.
Karan, Gloria S.
Karan, Orville Carl
Karlen, Eric Booth
Katz, Joseph M.
Kaufman, Judy Lynn
Kay, Marion Lank
Kaye, Melvin Michael
Keenan, Eithne
Kelleher, William Joseph
Keller, Frances Marie
Kelley, Maureen T.
Kelly, Doris E.
Kennedy, John W.
Kennedy, Robert Steven
Kerr, Norman Joseph
Kesler, Steven Shaw
Khan, Kanwar Habib
Khan, Lois I.
Kiel, Larry
Killilea, John Michael
King, David Warren
King, Gene Walker
Kipgen, Joyce Evelyn
Kirschenbaum, Neal
Kirshenbaum, Doris
Kirshenbaum, Elaine Barbara
Kisch, Ron Magnus
Klein, Robert David
Klima, Kenneth Joseph
Knapp, Nancy Hay
Kneavel, Thomas Charles, Jr.
Knepper, Warren R.
Knight, James Ralph, Jr.
Knox, Barbara Snyder
Knull, Thurman Dale
Kohlrieser, George Albert
Kolarik, James Milan

Koller, Terrence James
Komers, Ronald W.
Konig, Irene
Koonce, Jefferson Michael
Koontz, Dorothy Ann
Korner, Steven
Kosack, Allan Francis
Kramer, Kenneth Elliot
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Krauser, Charlotte Claire
Krebs, George M.
Krell, Frederic
Krenkel, Noele
Krizmis, Rena
Kroll, Larry Justin
Kruger, Robert S.
Kuhl, William Robert
Kunkle, Thomas P.
Kunkle, Toni Jeanne
Kurtz, Theodore Stephen
Kushman, Karen Michelle
Kutner, Sol
Kwak, Barbara J.

Lacey, Stephen Edward
Ladd, Lisa Katherine
Lain, Margaret Elizabeth
Lajousky, William, Jr.
Lake, Lesley S.
Lakowitz, Raphell Sims
Landis, Luella Daniels
Langer, Ginette B.
Langer, Lawrence
Lanzisera, Philip John
Larsen, H. Stephen
Lauer, Barbara Ann
Lauria, Anthony Louis
Laverty, Kristine Hall
Lawrence, Patrick G.
Lazar, Zoe L.
Ledelbur, Gary William
Lee, Helen Shores
Lee, Linda Jane
Leibensperger, Amber Baldwin
Lennox, Robert Snowden
Levin, Elaine L.
Levine, Laurence Sheldon
Levine, Stephen Mark
Levy, Joel Martin
Lewis, Brian John
Li, Livia Kidmei
Libertun, Lidia Beatriz
Licopoli, Francis L.
Lieberman, Deborah Katz
Lifson, Allan Howard
Lifson, Nathan
Lill, James Frank
Limongello, C. Michael
Linarducci, Robert Pasquale
Linder, Helene B.
Linn, Stephen Eugene
Livingood, James Russell
Livingston, Joe D.
Livingston, Roger Harry
Lloyd, Paul Joseph
Loeffler, Henry Kenneth
Logan, Patricia Burns
Lonborg, Greta R.
London, Albert S.
Lord, Gary Lewis
Lorentz, Raymond John
Lorenz, Andrea Helen
Lowery, Charles Lee
Lowery, Mary Jane
Loye, David Elliot
Luby, Larry W.
Lucero, William Joseph
Lyman, David A.
Lynch, Timothy Dennis

MacCardell, Leslie Hart
Maciborka, Barbara
Mack, James Michael
Maes, Charles D.
Maggio, Wanda Festini
Mahoney, Frances Ann
Majovski, Lawrence Vladimir
Makower, Naomi C.
Malamud, Thomas Judson
Malley, J. David
Malmstrom, Frederick Vincent
Malotte, Wayman E.
Mancano, Michael Joseph
Mandryk, Thomas Russell
Manganello, James A.
Mangum, Paul Daniel, Jr.
Manheim, Alan Anselm
Mann, Suzanne Bette
Manni, John Leonard
Mapes, Bruce Edward
Marburger, Steven
Marinelli, Frank Anthony
Marino, Martin F.
Mark, Sandra Fay
Marquardt, Lloyd David
Marsh, Mary Louise
Marshall, David Douglas
Martella, Joseph Anthony
Martinson, Lucia Elizabeth
Masih, Gulzar Barkat
Massey, Robert Franklin
Materazzi, Augustus Victor
Mayclin, Dan K.
Mayers, Barbara Susan
Mazzagatti, Nicholas J.
McCarthy, Robert Edward
McCoy, Clarence Edward, Jr.
McCrady, Barbara Sachs
McCrone, William Patrick
McCullough, Thomas Michael
McCusker, Kenneth
McDermott, William V.
McGarrey, Patricia Ann
McGee, Joanabeth Ballard
McGovern, Lois Maryanne
McHugh, Dolly Ann
McKillip, John A.
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McMillan, Grant Rochfort
McNair, Edward Glaston
McNeill, Malvina Rosat
McSurely, Alan
McWilliams, Sheldon Thomas, Jr.
Meehan, Robert H.
Meltzer, Gloria
Merrill, Susan Lee
Mesches, Leanore Weinberger
Meskauskas, John A.
Metoskie, John Arthur
Michaelson, Ronnie Login
Migdol, Kenneth M.
Miller, Harold
Miller, Harriet Siegel
Miller, Lester E.
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Minch, Jane
Mini, Louise Ann
Miraglia, Phillip Joseph
Mishler, Susan Anzivino
Mitchell, Edward Lee
Mitchell, Edwin Lee
Moir, John Brownlee
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Monfredo, Benjamin Francis
Montague, Pamela Ann
Montijo, Jorge Antonio
Mood, Darlene Weisblatt
Moore, Andrea Jean
Moore, Joanne Francine
Moore, Robert Franklin
Moore, Robert R.
Morahan, Janet M.
Moran, Janice Mary
Moran, Melanie Knapp
Moran, Robert Thomas
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Morguelan, Fred Neal
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Most, Robert King
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Murajda, Helen Jean
Murdoch, Eleanor M.
Murphy, Walter V.
Murray, Thomas Henry

Nagy, Vivian Tong
Naide, Albert
Namenek, Therese Marie
Napora, Joseph P., Jr.
Nason, Dennis A.
Nathanson, Carol
Nealey, Vicki Green
Neighbour, John Henry
Nelson, Gayle G.
Nelson, Joan Elizabeth
Nemtusak, Mathew Anthony, Jr.
Nerney, Mary
Newman, Neal Peter
Newton, Sarah A.
Nichols, Clarence Eugene
Nicoletta, Thomas Michael
Nicoud, Jonathan
Nies, Charles M.
Nimmo, David C.
Norman-Jackson, Jacquelyn
Norris, David LeRoy
Norton, Catherine G.
Novey, Theodore Burton
Nowakowski, Marie Elizabeth
Nowell, Lawrence Alexander
Nuce, Laura Jane
Nuñez, Antonio Eduardo

O'Dell, Stan Lee
Ogburn, Keith David
Ogusky, Ronald Charles
Osherson, Adele S.
Ospa, Joel D.
O'Sullivan, Michael Joseph
Otterbein, Leo Eugene

Paine, Whiton Stewart
Pantano, Lynn Theresa
Parker, Harvey Cary
Parker, Kermit Eugene, Jr.
Parr, Vincent Ernest
Paul, Ved Parkash
Pavelsky, Robert Lee
Payne, Glen Charles
Payton, Tommy Ira
Peak, Grahame John
Pearce, Linda Brown
Pearson, Barbara Feen
Peck, Raymond C.
Pelletier, Alcid Milton
Pelletier, Cheryl Stanton
Percelay, Sheela
Perilstein, Joanne Prise
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Permut, Steven E.
Perry, Marva J.
Peters, Virginia Stauffer
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Peterson, Joseph Patrick
Peterson, Robert Schaefer
Peterson, Ronald Emanuel
Petros, Usam Aziz
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Pfohl, William Forman, Jr.
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Phillips, Jack Howard
Pickens, Marleen K.
Piech, Mary Lou
Pinson, Kathryn Walker
Pinto, Louise Rita
Pinto-Marques, Heradio C.
Pitts, Helen Haines
Pizano-Thomén, Margarita Maria
Plachetta, Kurt Eric
Poling, Tommy Hugh
Polisky, Randy J.
Poll, Bernice G.
Pollock, Richard L.
Pool, David Cregar
Popoff, Edna Spieler
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Porchelli, George McCall
Posner, Israel W.
Potter, Nancy Dutton
Powell, Barbara
Power, Elaine Perpetua
Pratt, Earle Wilson, Jr.
Probst, Sandra K.
Proctor, Derrick L.
Proctor, Diane Cole
Provins, Robert Glen
Pruden, Claroy W.
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Schulman, Susan Ruth
Scott, Ronald Lee
Seaman, John Michael
Seeley, Odile Marie
Segal, Cecile Pollack
Segal, Roberta Hasan
Seibold, Jon
Seidita, Nicholas Vincent
Serfass, Carolyn P.
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Seveland, John W.
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Shepherd, Walter Philip
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Shiel, Thomas Vincent
Shifren, Irene E.
Shotts, John R.
Shuchter, Carolbeth Rosner
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Shuttleworth, William Charles
Siering, George David
Signorelli, Anthony S.
Silberstein, Ruth
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Skeel, Charlaine Joy
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Sloan, Marc E.
Slocumb, Frances Gilliam
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Smith, Charles A.
Smith, David Allen
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Snyder, Arden L.
Snyder, Margaret P.
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Soat, Douglas Marc
Sofield, Loughlan
Soutor, Ronald Eugene
Speir, Herbert Allison, III
Spence, William Edward, Jr.
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Spikler, Rochelle Ellen
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Spindler, James Nelson
Spiro, Drucilla A.
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Staples, Sharon Bruce
Steinberg, Fred A.
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Sternberg, Richard Ira
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Stitzer, Dianne Flint
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Stuck, Murlowe Leon
Sturgis, Samuel Paine, III
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Swartz, Allen Irwin
Szpadzinski, Raymond Stanley

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Tees, Sandra Martin
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Tentoni, Stuart Charles
Terdine, Richard Gerard
Tesoro, Rustica Torrijos
Thebolt, George Lee
Thoe, Patricia Carol
Thomas, Carroll Ray
Thomas, Charles S.
Thomas, Katherine Rupert
Thomas, Lucinda Ellen
Thomas, Randall Lloyd
Thompson, Lynne Nichols
Thompson, Tyler
Thorsen, Signe Ann
Titgemeyer, Roger William
Tomany, Shirley Ann
Tonesk, Xenia
Torbert, James Brison
Trucchio, Thomas Michael
Tucci, Nicholas
Tucker, William Reed
Tyson, Mark Edward

Ugent, Warren D.
Uhlemann, Max Reinhold

Vaccarella, Gerard, Jr.
Valla, Samuel Maurice
VanderWege, Terrence Alan
Van Fleet, Jay Nelson
Vassallo, George Castner
Vastano, Carole
Venema, Henry B.
Vesper, Joyce Horwich
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Vredenburgh, Donald John

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Wallach, Ellen J.
Walsh, Thomas Anthony
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Webster, Raymond E.
Wedell, Jacelyn R.
Weil, Robert Arthur
Weirbeky, Carol Donna
Weiss, David Charles
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Weiss, Leslie Bloch
Weiss, Marc Franchot
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Wells, David Thomas
Wengrow, Jack
Werner, Gary S.
Werner, Paul Dennis
Wessman, Quentin F.
West, Barbara
Westphal, Frank George
Wheeler, Charles, III
Wheeler, Mary Louise
Whelihan, William Michael
White, Frances Bonds
White, Garry William
White, Nicole H.
Wideman, Marian Patricia
Wilkinson, Lamar V.
Williams, Allan Rawson, Jr.
Williams, Jean Beth
Williams, Ronald Owen
Willis, John Herman, Jr.
Wingblade, Loren Charles
Winkler, Henry Bevan
Wirt, Roger Alan
Wohl, Jonathan Alan
Wolf, Robert Phillip
Wolinsky, Sara
Wonderlic, Charles Fredrick
Wong, Herbert Z.
Wong, Keith K. H.
Wood, Gary Seth
Wright, Marsha Armstrong
Wright, Merle Levine
Wubbenhorst, John Michael
Wurster, Sharon Ann

Yaffe, Maurice
Yates, Hal Gene
Yates, Nancy Carole
Yorn, Ira Paul
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Zadig, Alfred Thomas Kurt
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Abraham, Carol
Ace, Merle E.
Adelman, Howard
Allen, Ronald E.
Ammons, Rose Mary
Astill, Dolores Ellen

Balch, Philip
Barash, Robert I.
Barry, Robert R.
Bauste, Larry G.
Beatty, James R.
Beere, Donald B.
Beiser, Jacob P.
Belsky, Muriel B.
Bencomo, Armando A.
Benton, F. Warren, Jr.
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Bierbryer, Bruce
Birnbaum, Michael H.
Black, F. William
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Blinder, Max
Bloom, Jack H.
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Burke, Leocadia
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Cannella, Ronald M.
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Carroll, Halcyon H.
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Christos, Constantine
Clarren, Sumner N.
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Coleman, Sandra B.
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Costello, Robert T.
Cox, W. Miles
Cromer, Ward
Crouse, Gerrit
Crowe, Patricia B.
Curley, James F.

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Dank, Martin I.
Davis, Carole J.
De Marzo, Baldwin M.
Denison, Joseph W.
Dennis, Paul M.
Derrick, Noel E.
De Vita, Victor R.
DiLalo, Louise
Dischel, Phyllis I.
Dolins, Joseph L.
Drude, Kenneth P.
Duncan, Robert L.
Dusansky, Barbara

Ellman, Norman K.
Ellsweig, Phyllis L.
Equi, Peter J.
Esterson, Pat S.

Faison, Bert
Falkenberg, Don
Fanelli, Gerard C.
Farber, Joan C.
Fay, Henry J.
Fellerath, John T.
Ferriolo, Michael
Fineberg, Beth Louise
Fineman, Kenneth R.
Fiss, Jeanette S.
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Forman, Rhea H.
Francis, Evelyn W.
Frank, James F.
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Friedman, Mark J.
Friedman, Steven
Frieze, Irene L.
Fristoe, Macalyne
Fullerton, Wayne S.

Garrity, Carla B.
Garsee, Jarrell W.
Gellman, Florence
Gergen, Paul C.
Gianutsos, Rosamond
Gibson, Kenneth R.
Gilbride, Thomas V.
Ginsberg, Stanley
Girgus, Joel J.
Goby, Marshall Jay
Gold, Alice R.
Goldberg, Leonard
Goldberg, Marsha
Goldstein, Stanley Melvin
Golemba, Michael E.
Gonzalez-Reigosa, Fernando
Goode, Paul
Goodyear, Rodney K.
Gradwell, William A.
Grant, Thomas N.
Green, Mary Grace
Grossman, Della S.
Grote, Frederick W., Jr.
Grzesiak, Roy C.
Gualderon, John G.
Guidry, Lawrence S.
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Hall, Peter H.
Hardy, James M.
Harkavy, Alan
Hart, Stuart N.
Havens, Ronald A.
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Heffler, Joel Barton
Heilman, Kenneth A.
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Henson, Ramon
Hepburn, Andrew W.
Hersch, Ronald G.
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Hoover, Jon Gary
Horowitz, Roslyn
Horwitz, Morton
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Husted, June R.

Israel, Alan R.

Jamail, Michael A.
James, Edassery V.
Jennings, James E.
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Jensen, Richard E.
Johnsen, Stephen F.
Johnson, Paul Alan
Johnson, Warren Munro

Kaiser, Johanna
Kane, Mark J.
Karp, Barry P.
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Kay, F. DeWitt, Jr.
Kelley, David M.
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Kennedy, Robert S.
Kierniesky, Nicholas C.
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Klinedinst, Harold R.
Klosterman, Donald F.
Knutson, Jeanne N.
Koch, Manferd Dee
Konya, Bruce A.
Kossuth, Gina L.
Krain, Burton F.
Kremnitzer, Susan
Kuckleburg, Ray J., Jr.

Lagos, John M.
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Lenssen, Barbara G.
Lentchner, Lawrence H.
Lerner, Albert
Lever, Michael David
Libby, Myrna E.
Loigman, Barry M.
Lupiani, Donald A.
Lyle, Orcena E.
Lynch, Marianne Cook

Mabli, Jerome
Machowsky, Herbert
Mahler, H. James
Marcum, Howard B.
Martin, Paul J.
Matter, Darryl E.
Matthews, Richard
Mattis, Noemi Perelman
McBride, Byron A.
McCahan, George R.
McElvain, Judith Lee
McGrory, Joseph E.
McIntire, Jon W.
McRae, Douglas J.
Mealy, John Burke
Mednick, Murray S.
Miller, Stephen D.
Millis, Willard E., Jr.
Minishiello, William E.
Moser, Adolph J.
Moser, Kathryn Ann
Murphy, Linda L.
Mynatt, Harold Lee

Nearing, Robert J.
Nelson, Arnold E.

O'Connor, A. Gail
O'Halloran, Richard J.
O'Neill, Andrea Seiver
Orsillo, Donald G.
Ostrand, Janet Louise
Owens, James M.

Pachtman, Edith
Palef, Sandra R.
Palmer, Anthony J.
Paolella, John M.
Parsons, Ralph F., Jr.
Paskewitz, Daniel F.
Patrick, Audrey W.
Patrick, Jayne
Payne, Dan
Payne, John W.
Perlman, Lawrence M.
Petzelt, John A.
Polakoff, Sally E.
Primavera, Louis H.

Quattrocchi, Michael R.

Raben, Charles Savas
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Regan, Carole B.
Reiter, Gregg F.
Resnick, Jacquelyn Liss
Resnick, Michael B.
Richards, W. Bruce
Ritchie, Richard J.
Robert, Nan
Ronnebeck, Reinhard W.
Rosenberg, Jack L.
Rudolph, James E.

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St. John, David E.

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Schildhaus, Andrew
Schippers, Louis
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Schwartz, Allan James
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Schwitzgebel, Robert L.
Scopetta, Mercedes A.
Scott, William C.
Seamons, Terry R.
Sebest, Edwina
Selig, Sidney
Sepez, Peter V.
Shepherd-Look, Dee L.
Sherrick, Michael F.
Shimoni, Nissim
Shimrat, Niusia
Silver, Dianne S.
Silver, Robert Jay
Simmons, Robert Jay
Singh, Joginder Paul
Sinowitz, Melvin
Smith, Caryl Halper
Smith, Gene F.
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Smith, William Ray
Sodetz, Alan R.
Soethe, James W.
Spencer, Barbara J.
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Steele, Richard L.
Sterner, Gary Alan
Sterner, Gary Arvid
Sternes, Glenn F.
Sussman, Gilbert
Swezey, Robert W.
Szura, John Paul

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Thomsen, Stephen J.
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Vecchiotti, Robert A.
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Witkin, Mildred Hope
Woodrow, Jane Zartman
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Zimmerman, Jerry L.
Zimmerman, Sephanie M.

Comment

Response to Rogers' "Challenges"

Carl Rogers' article in the May 1973 issue was very interesting. As the only professional radio broadcaster in the APA, over the 25 years that I have broadcast "Psychologically Speaking" I have tried continuously to interest the program chairmen of the national conferences to have some meetings on the public attitude toward psychologists: the public's general suspicion of the freakish demonstrations in psychologists' so-called "therapy" and some measure of control over the completely nonvalidated outpourings in psychology's so-called literature for the public. And consistently the bureaucracy has not even had sufficient interest to answer my mail. The culmination was at the San Francisco meeting where the balcony of the Fairmont Hotel was inhabited by such goings on in the name of "humanism" that even a modern insane asylum would have been ashamed of it.

What pains me most is to ask Rogers to remember back to when he started most of this. Whatever he might have felt was a "scientific" approach to "client-centered counseling," he started the movement of listening only and also the notion that all one had to know in "practising psychology" was to listen and that "advice" is a dirty word. But all of this is the psychologist's notion. The client is not at all interested in doing all the paying and all the talking. He would like some advice, but surely he will not receive it in weekend nude encounters, or encounter groups with bedrooms, or touch and squeeze the muscle groups, or publications that extoll the use of sex relations with the therapist. We, as a group, are so afraid that we will be criticized that we have lost our identity with the community as a group that either sets standards or has any interest in them.

When I want a good book that discusses human problems, I choose one written by a layman rather than by a psychologist because the layman is interested in human problems. I am sorry to say that the least effective broadcasters on human problems are psychologists, who are chiefly interested in aggrandizing their reputations and purses.

May I suggest to Dr. Rogers that at the next annual conference he have a "symposium" of psychologists who meet the public. We are never represented. Why? I suspect that the members are afraid of what we will say.

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Lee R. Steiner
New York, New York

Subject Cooperation in Experiments

At first glance the results of Resnick and Schwartz (February 1973) greatly disturbed us. The students in their experiment appeared to cooperate more under the "unethical" than under the "ethical" condition. This seemed to imply that undergraduate students will cooperate more with a relatively unethical professor or experimenter who behaves according to expectations than with an experimenter who is completely honest and open.

We feel that the effect of the independent variable (ethical behavior) in this experiment was badly confounded by length, complexity, and interest level of instructions. There was also possible confounding by experimenter characteristics, for example, prestige, confidence, bias, and degree of professional "know how" conveyed by the experimenter. Furthermore, in order to shape trusting behavior, it will take more than telling people that psychologists will no longer practice deception.

We wonder if the concern for differences between the two ethical codes in this experiment does not mirror the conflict between the two philosophies coexisting in psychology and, in fact, in our society. One philosophy emphasizes total responsibility by authority, and the other places value on individual responsibility and decision making. In the first case, the experimenter is in control and gives the subject minimal information. In the second case, the experimenter shares his or her hypotheses and hence shares control with subjects. The latter experimenter encourages independent decision processes in subjects (students). These processes may of course lead the subject to resist the influence of experimenters (authority figures). Honesty on the part of both subject and experimenter seems feasible only under the latter condition. We agree with Resnick and Schwartz that the comparative "ecological validity" of these two systems or codes merits serious consideration.

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Karen D. Laughlin
University of Washington
Edda I. Schweid
Children's Orthopedic Hospital
Seattle, Washington

Editor's note. The Resnick and Schwartz article was based on an early

draft of the research ethics report. The final report is very different (see "Ethical Principles in the Conduct of Research with Human Participants," *American Psychologist*, 1973, 28, 27–30).

Is Violence Itself Necessarily Bad?

I must take issue with several points in John P. Murray's (June 1973) highly biased article on television and violence. First, the evidence which he cites consists almost entirely of experiments on children in highly artificial situations, the results of which are hard to generalize to real-life settings, and correlational studies of adults which allow for a variety of cause–effect interpretations. Even the so-called "crucial" study by Lefkowitz et al. (1972) did not examine or control for such crucial factors as the child's self-concept or self-esteem. Nor did Murray cite or explain any of the negative findings on this subject (e.g., Feshbach & Singer, 1971).

Second, the definitions of *aggression* and *violence* (and "antisocial" behavior) are so broad and arbitrary as to include arguing, pushing, breaking toys, pushing "hurt" buttons, and delinquency. Does this imply that it is always bad for children to argue? (If so, does this mean that it would be better for them to simply "follow orders" when others tell them to do something they disapprove of?) Is pushing necessarily bad, say, for example, if one has been pushed around by a bully whom adults will not discipline? Is violence itself necessarily bad, or is it important to identify the full context before making such a judgment? For example, what about the difference between the initiation of force and self-defense? Is there a moral difference between an Eisenhower and a Hitler or Stalin?

Third, the author fails to recognize the possibility that watching violent programs might have certain beneficial consequences. For example, could watching *The Untouchables* show a child that (*a*) there is nothing romantic or admirable about being a criminal, (*b*) justice will be done to evildoers ("crime does not pay"), (*c*) the way to oppose evil is by a heroic, noncompromising attitude on the part of good men.

One gets the distinct impression from Murray's article, including his conclusion that television violence should be reduced, that his ideal child (or adult) would consist of a schmo: a being who wants nothing, will fight for nothing, including his own life or property, and will stand up for nothing regardless of the circumstances.

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Edwin A. Locke
University of Maryland

Supports Crisis Phone Services

With respect to Lester's (May 1973) criticisms that hotline telephone counseling is unsupervised, executed by poorly trained individuals, and potentially dangerous in that such agencies may violate professional ethics, I must take issue. This attitude toward paraprofessional efforts represents an elitism within the mental health field that posits nonacademically trained individuals as being unable to lend constructive support and help to other human beings. When a friend helps another friend, he is applauded. When caring individuals organize a service to help others in a crisis situation, a cry of impropriety is made by those professionals with power in the field. In my opinion, this attitude and underlying assumption is both dangerous and fallacious: dangerous in maintaining a small professional elite, and fallacious in assuming that a PhD is necessarily more able to provide crisis "help" than a non-PhD.

In the face of such a shortage of manpower in the mental health field, such efforts must be supported. Training sessions for volunteers are necessary, but the demand for supervision by psychologists sounds more like a demand for control rather than a genuine offer to help paraprofessionals. Let us not forget that those who are trained, untrained, and in need of help make up part of the community who are both therapists and patients, really nothing more or less than people who want to help and who seek help. The ability to empathize, listen, offer suggestions, and simply to "be there" in a time of crisis are not "trainable" qualities, nor qualities that need "supervision" as Lester implies, but rather constitute the best of being human.

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Paul Schoenfeld
California School of Professional Psychology

Lester Replies:

In reply to Schoenfeld's comment, there are several points worth making: (*a*) Schoenfeld implies that supervision of paraprofessionals is not necessary. By extension, then, are internships, supervision, licensing, and codes of ethics unnecessary for psychologists? (*b*) Schoenfeld equates supervision with control. I would equate it with both teaching and control. (*c*) Paraprofessionals do not act in the way that "friends" do. It is for this reason that selection and training are so critical for counselors of all kinds. (*d*) Those who have worked in crisis

services have experienced the results of lack of supervision. I have seen one counselor fall asleep while a client talked on the telephone to him. I have heard another respond by laughing when a client began a call by stating her suicidal intent. I have found counselors acting as therapists to clients after the counselors have left the crisis service. (*e*) We can no longer choose between control versus no control. The issue now is becoming "Who shall control?" It would be useful for the psychology profession if psychologists were given equal status with psychiatrists in the licensing of hotlines and crisis services.

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DAVID LESTER
Richard Stockton State College

What Constitutes a Social Good?

John Platt's (August 1973) article "Social Traps" was a clear exposition of a conceptual basis for applying behavior modification principles to social problems. Hidden, however, were assumptions on what constitutes a social good and the definition of long range, particularly as it implies an eventual locking-in of a system. Also ignored were the adverse side effects that might be generated by using devices he advocated, such as establishing a superordinate authority. Nor is there clear-cut evidence for some of the assertions he made. For example, he related how Indiana and Ohio, by setting up toll road corporations, converted short-range pay and return on investment, R_S^+, to an eventual benefit, R_L^+, that would accrue to the state and all the drivers. The odds are that had the toll roads not been built, the development of free interstates would not have been so impaired. Thus, because of threats to the revenues of the Indiana Toll Road, it took over 10 years to complete a short leg of I-94 between Gary, Indiana, and the Michigan state line. In the meantime, motorists were forced to use two-lane, crowded, and dangerous state roads. The "long-range" benefits lasted only a few years, but the "long-range" punishments (tolls) will last another 10.

In Illinois, the Toll Road Commission appears to have an eternal life. Rather than pay off the bond issues as the profits accrue, a new sparsely used toll road is being built. Again, how long is "long"? Yet, toll roads do have a benefit for a minority segment of motorists in that the costs dissuade many who would otherwise contribute to its congestion. Hence, the long-range benefits do accrue to those willing and able to make the financial expenditure, but the concept also locks out those who are not. Are we then not discriminating against the poor?

Medicare is another example of mixed blessings. In the short range, the medical delivery system seems to have been strained, even though one major segment of our society appears to have greatly benefited. But in the long range, changes in the system induced by Medicare can have broader benefits.

Still another example is that many economists partly attribute the high costs of beef to the expanded welfare system whereby those formerly unable to purchase this item are now competing for scarce supplies. What are the positives and negatives to whom and for how long? And which social accountant is to determine these valences?

Did we lock in our television lineage standards too early, or should we have waited before standardizing, as Europeans have done? How much longer should we have waited to enjoy clearer color programs? No social venture, regardless of how noble the underlying philosophy, is devoid of adverse effect on some segments of the population nor is it "good" indefinitely. Similarly, none is "bad" indefinitely. Constant readjustments in the social, political, and economic systems do take place, often alternating between "good" and "bad"; and this is the central problem, not arbitrarily specifying an oversimplistic long range without carefully examining the intricacies of the effects and the specific segments affected.

Other examples which Platt cited (e.g., Special Drawing Rights, European Common Market) are not clear-cut examples of social good. Neither did Platt explain why the demise of hundreds of leading newspapers over the past 20 years is necessarily bad or why media should not strive for higher audience ratings. (I am not even attempting to delve into the question as to whether violence depicted on television breeds violence.)

The technology of behavior modification, whether on an individual or a group basis, will probably constantly improve. But the major question is whether and what behavior should be modified, and the question must take into account not only the personal values of the technologist, but his awareness of the intricacies of the system and the balances between the "good" and "bad" at numerous points along the time scale.

REFERENCE

PLATT, J. Social traps. *American Psychologist*, 1973, **28**, 641–651.

JOSEPH M. KAMEN
Indiana University Northwest, Gary

Prefers Hardin's Solution

John Platt (August 1973) used Garrett Hardin's (1968) "The Tragedy of the Commons" as one example of the "social traps" with which he is concerned. After examining some suggestions for ways out of these traps, Platt suggested that a Skinnerian "reversal-of-reinforcers" approach is one that might help, and he added: "Some of these methods have been in use for hundreds or thousands of years, and much of our fashionable despair today comes

from a kind of willful blindness to the methods that society developed long ago [p. 648]."

Following this introduction to his suggestion of a solution he stated, simply,

> the Tragedy of the Commons is essentially a problem of the allocation of scarce resources. And a half-hour's thought will turn up a dozen mechanisms that we use every day for dealing with such problems. [And] Hardin . . . made out his New England cattle owners to be a good deal more stupid than they actually were. The problem is not a problem of thoughtless competition, but rather the problem of setting up a superordinate authority to handle reinforcement mechanisms . . . for getting out of these traps [p. 648].

As one who found Garrett Hardin (and his New Englanders) neither stupid nor simplistic, I was troubled by Platt's characterization. Moreover, the problem with which Hardin was struggling seems not to be comprehended by Platt. Hardin's concern, and mine, is that of scarce resources in a *free society*. Platt's methods, those that have been in use for hundreds or thousands of years, may well be successful in dealing with a problem of the allocation of scarce resources. They are methods that have had the luxury of not dealing with a free people. Platt is right: Set up a superordinate authority. But that solution is one Hardin saw as incongruous with a free society.

Platt's presentation brings sharply to the fore a concern that many psychologist-libertarians have about consequences of the Skinnerian scientific approach to human problems. At times the problem of freedom is recognized by Skinnerians before it is discarded. In this case, however, Platt seems not to have seen the freedom issue, even though it was central in Hardin's thinking. And Platt's answer—a superordinate authority using our scientific knowledge and methods!—is frightening. I, for one, far prefer Hardin's social trap and his dilemma to Platt's help to a way out.

REFERENCES

Hardin, G. The tragedy of the Commons. *Science*, 1968, **162**, 1243–1248.

Platt, J. Social traps. *American Psychologist*, 1973, **28**, 641–651.

Arthur L. Kobler
Seattle, Washington

Recruitment of Minority Group Students and Women

This Comment describes recruitment efforts of fully approved APA doctoral programs in clinical psychology. More specifically, the study described herein attempted to determine how many programs actively recruited members of minority groups and women and, in addition, how the actual recruitment procedures were handled in the application materials.

In recent years, professionals in psychology have been recognizing that members of minority groups as well as women have been underutilized in psychology; there is little agreement, however, about how to rectify the imbalance. One approach is to maximize the number of applications sent in by members of minority groups and women so that there would be a wider base of qualified applicants from which to choose. If recruitment of more applications from minority group members and women is a potentially effective method of democratizing psychology, it is important that psychology departments be aware of what is the current status of recruitment and how various departments actually have recruited applicants.

Method

The list of fully approved APA programs in clinical psychology as of July 1, 1972, was obtained from the manual *Graduate Study in Psychology for 1973–74* (APA, 1972). Clinical psychology programs were chosen as a target group for two reasons: (*a*) the relatively large number which could yield potentially more data and (*b*) the recent concern of applied fields in psychology for diversifying the cultural composition of their students. Although the total number of programs was 69, 4 programs did not send their application materials. Consequently, the total number of programs was adjusted to 65.

I sent to each program a brief letter which stated an intention to go to graduate school and a request for application materials. I felt such a deception was necessary because I was interested in analyzing recruitment procedures as they appeared in the regular application materials. I wanted to view the recruitment policies from the vantage point of an applicant, rather than from the vantage point of the program, for how the program views its policies may be quite different from how an applicant perceives the policies.

Next, the literature from each school was read, and those programs that attended to minority group members or to women were identified. Those programs recruiting were then rated on a 7-point recruitment continuum. The recruitment categories were the following: (1) statement of encouragement to apply; (2) statement of interest; (3) statement of the program's commitment to minority groups or women; (4) statement that scores, etc., are considered in the context of cultural background or special consideration given; (5) statement that a certain number of admission openings were available for minority members or women; (6) statement of cooperation with the Black Students' Association and/or the Association of Black Psychologists; and (7) statement of specific inducements, that is, fees waived, financial support, test scores optional.

The recruitment scale was not an interval scale, but rather a rough rank order of recruitment procedures that were ordered on the basis of an increasing amount of information offered to applicants about the program's commitment to minority group and women's admissions. For example, one sentence encouraging an applicant to apply did not seem as strong a message as did an offer of specific inducements to apply. However, the middle range became more interchangeable and it was possible to score on several categories. In addition, *stating a commitment* was scored only when the literature stated "commitment" or clearly indicated commitment with another choice of words. However, those programs which recruited in another form seemed to be illustrating some type of commitment in practice, and this should be taken into account when reviewing the results. Finally, statements of nondiscrimination were not considered recruitment.

Results

Two basic questions were of interest: (*a*) How many clinical psychol-

ogy programs recruited (according to the definition used here) minority group members and women, and (*b*) what were the various forms of recruitment?

Of the 69 clinical psychology programs, 65 schools responded to the letter by sending their application materials so that there was a 94.2% return rate. While 22 or 33.8% of the programs recruited members of minority groups, 2 programs or 3% recruited women. Consequently, 43 or 66% of the programs did not recruit minority group members and 63 or 96.9% did not recruit women.

The next point of interest concerned the various forms of recruitment chosen by the clinical psychology programs. Concerning the recruitment of minority group members, 15 of the 22 programs scored on the first half of the continuum. More specifically, 7 programs offered statements of encouragement, 4 stated interest, 4 stated their commitment to increasing the participation of minority group members, and 7 stated that special consideration would be given. The remaining 13 programs scored on the latter part of the continuum which involved stronger recruiting measures. Of those 13, 9 offered special inducements. The most frequent inducement which was used by 5 schools involved special financial support. Regarding the other inducements, 2 schools waived application fees and 2 schools stated that test scores were optional. Only 1 school told the applicant that a certain number of admission openings were available, while 3 schools indicated cooperation with the Black Students' Association and the Association of Black Psychologists. Of the 22 schools, 9 scored on more than one category, with 4 schools using three different recruitment strategies.

Only 2 schools recruited women, and the form was one of encouragement to apply. One university wrote, "Women and minority groups are encouraged to apply for admission." Another school wrote, "We find that holders of the M.A. and M.S. who enter this department do well in graduate work here. Their applications, like those of women, and of members of minority groups, are especially welcome."

Discussion

The analysis of recruitment strategies had several limitations. First, the study did not measure the programs' intentions, but rather the written indicators of such intentions. In addition, application materials may not represent the actual quality of supportive relationships existing in any program. Consequently, the result cannot be construed to indicate actual practices of each clinical psychology program. Second, an element of personal judgment was necessarily involved when rating the forms of recruitment. However, there were only two cases when the author felt in some doubt.

It seems fairly clear from the results that the recruitment of minority groups and especially of women was not widely practiced. Of those programs that did recruit, many gave little information to the applicant about the program's stand on the issue, the program's willingness to support a member of a minority group or a woman both emotionally or physically, and the program's understanding of why they want to increase applications from minority groups and women. In other words, many schools gave little reason for the applicant to take the recruitment seriously.

There was a noticeable lack of attention to the female applicant. One school informed applicants that the school provided many jobs for the wives of graduate students but did not mention job opportunities for husbands of graduate students. The current attempts to recruit women have not included the following potentially useful incentives: (*a*) special scholarships for women, (*b*) jobs for husbands, (*c*) day care, (*d*) part-time graduate study, and (*e*) study through correspondence.

The results of this study may encourage those responsible for admissions to clarify their position on the recruitment issue and to learn from the practices of other programs concerning the elements of a clear and strong recruitment strategy. At this time, the professional staff of clinical psychology programs seem ambivalent about recruitment of minority group members and women.

REFERENCE

American Psychological Association. *Graduate study in psychology for 1973–74.* Washington, D.C.: Author, 1972.

Nancy Dutton Potter
University of Missouri, Columbia

Announcements

1974 Regional Placement Activities

You are invited to use the placement services at the regional psychological association conventions this spring. Employers who wish to attend or to list positions with any individual regional convention must contact the appropriate placement official listed below. In addition, APA will publish the *APA Regional Placement Bulletin* (positions only) for distribution at the regional conventions. This provides a single source of information that is available at all seven meetings, as well as through APA. It does not take the place of registering with any of the individual Placement Offices. Employers thus have two options:

1. Listing in the *APA Regional Placement Bulletin*, distributed to all seven regional conventions ($5 fee required for each printed listing—FEE MUST ACCOMPANY COPY). *Deadline: March 22, 1974.*

2. Separate and/or additional listings at any of the seven regional conventions (check local Placement Chair as to individual fee requirements).

Southern Society for Philosophy and Psychology: April 11–13, 1974; International Inn, 4800 W. Kennedy Boulevard, Tampa, Florida. *Placement Chair:* L. B. Cebik, Department of Philosophy and Religion, University of Georgia, Athens, Georgia 30601.

Eastern Psychological Association: April 18–20, 1974; Sheraton Philadelphia Hotel, Philadelphia. *Placement Co-chair:* Joseph DuCette and Emil Soucar, Department of Educational Psychology, Temple University, Philadelphia, Pennsylvania 19122.

Western Psychological Association: April 25–28, 1974; Sheraton-Palace Hotel, San Francisco, California. *Placement Chair:* Judi Komaki, Psychology Department, San Jose State University, San Jose, California 95114

Southeastern Psychological Association: May 2–4, 1974; Diplomat Hotel, Hollywood, Florida. *Placement Chair:* Michael Epstein, University of Miami, Box 8185, Coral Gables, Florida 33124.

Southwestern Psychological Association: May 2–4, 1974; Paso del Norte Hotel, El Paso, Texas. *Placement Chair:* Paul Whitmore, HumRRO Division 5, Post Office Box 6057, Fort Bliss, Texas 79916.

Midwestern Psychological Association: May 2–4, 1974; Conrad Hilton Hotel, Chicago, Illinois. *Placement Chair:* David W. Bortree, Box 476, South Holland, Illinois 60473.

Rocky Mountain Psychological Association: May 8–11, 1974; Cosmopolitan Hotel, 18th and Broadway, Denver, Colorado. *Placement Chair:* John Hocking, Veterans Administration Hospital, Mental Hygiene Clinic, 1055 Clermont Street, Denver, Colorado 80220.

Applicant and general inquiries about placement facilities *should be directed to the convention officials whose names and addresses are listed above.*

Please note the first regional convention is April 11, necessitating the *APA Regional Placement Bulletin* closing data of March 22 (earlier announcement to employers was made by letter and the January, February, and March *APA Employment Bulletin*).

The printed *APA Regional Placement Bulletin* will be available at the conventions for $1.00 per copy, or may be ordered by mail (available April 2) by sending $1.50 to Regional Conventions, Placement Services, American Psychological Association, 1200 17th Street, N.W., Washington, D.C. 20036.

American Board of Professional Psychology

The *American Board of Professional Psychology*, during the period September 12–December 12, 1973, awarded its diploma in the specialties of Clinical and Industrial and Organizational Psychology to the following:

CLINICAL

Rose E. Boyarsky
Allen Mark Feinberg
Lane Anthony Gerber
Mack Raymond Hicks
Floyd S. Irvin
Paul Kessel
Theodore Landsman
Joel Morton Liebowitz
Shirley Sanders
Bruce T. Saunders
Freda Beth Stone
Robert H. Woody

INDUSTRIAL AND ORGANIZATIONAL

Ronald O. Lippitt

Corrections

In the listing of the Industrial Psychology II paper session on page 825 (September), the affiliation for *Stephen L. Cohen* is incorrect. His correct affiliation is University of South Florida.

In the ABPP diplomate listing on page 1017 (November), *Jefferson M. Fish* is incorrectly listed as Morris Jefferson Fish.

Two names were inadvertently omitted from the list of 1973 consultants for the *American Psychologist* on page 1136 (December). They are *D. James Dooling* and *Robert Perloff*. Their contributions are greatly appreciated.

In the first Comment in the January issue ("Some Comments on Hersch," pp. 54–55), the second author's name was omitted. He is *Leslie G. Brody* of the Indiana Department of Mental Health. We regret the error.

Deaths

Katherine E. Baker, November 16, 1973

Walter Cohen, December 20, 1973
Charles B. Truax, December 10, 1973
Fred D. Whelan, date unknown

Fellowships; Predoctoral and Postdoctoral Programs

Leadership Program in Community Mental Health, School of Public Health, University of California, Berkeley: Now accepting applications for 1974–1975 academic year. An interdisciplinary curriculum providing skills in planning, administration, consultation, community organization, and research is integrated with innovative field experience. Requirements are a master's degree in a mental health related field and suitable work experience. An MPH is awarded after one year of study. Some stipends available. *Final application deadline is March 1, 1974.* For further information write to Robert Z. Apte, School of Public Health, Earl Warren Hall, University of California, Berkeley, California 94720.

Division of Psychology in the Department of Psychiatry at the Neuropsychiatric Institute of the University of Illinois: Now accepting applications for postdoctoral fellowships in community psychology. The program is planned to enable students to spend a year with the supervision and instruction of experts in the field. They will be developing specialized knowledge and skills for organization, planning, execution, and evaluation of community mental health programs. Through seminars and supervised field experience, the fellow is exposed to such areas as community organization, epidemiology, and ecology. Specialized training is also given in the techniques of community organization, consultation, and crisis intervention. Trainees will be selected with backgrounds in social and clinical psychology. The stipend is $9,240 per year. To apply or obtain further information write to Wade H. Silverman, Community Psychology Program, Psychology Division, University of Illinois Medical School, P.O. Box 6998, Chicago, Illinois 60680.

Child Psychology Section of the University of Iowa Psychopathic Hospital: Now offering a postdoctoral fellowship in child-clinical research, consultation, and service. Applicants whose degrees are in clinical, developmental, experimental, school psychology, or related fields will be considered. Appointments are for one year, beginning no later than October 1, 1974, with a possible second year. First-year stipend: $10,500 (taxable). Send letter of application and curriculum vitae to Jan Loney, Department of Psychiatry, College of Medicine, 500 Newton Road, Iowa City, Iowa 52240.

Division of Pediatric Psychology, University of Maryland School of Medicine: Now accepting applications for 1974–1975 internships in clinical child psychology presenting comprehensive field training in clinical child, pediatric psychology, and developmental disabilities. The program has an interdisciplinary focus, and there is close association with the Department of Child and Adolescent Psychiatry. Applicants with background in clinical, counseling, and school psychology considered. *Deadline for completed application is March 11, 1974.* For more detailed information write to Thomas Kenny or Rudolph Bauer, Division of Pediatric Psychology, University of Maryland Hospital, Baltimore, Maryland 21201.

College of Law and the Psychology Department of the University of Nebraska–Lincoln: Announce the law–psychology graduate studies program leading to both a regular law degree, the Juris Doctor, and a PhD in psychology. This program will typically take a student five years to complete both degrees. The student may take his PhD in any of four areas: general-experimental, social-personality, community-clinical, or psycholegal studies. For further information contact Bruce Dennis Sales, 209 Burnett Hall, University of Nebraska–Lincoln, Lincoln, Nebraska 68508.

Psychiatric Associates of Tidewater, Inc.: Now accepting applications for predoctoral internships in clinical psychology to commence either June or September 1974. The internship is a rotating one and will offer opportunity for intensely supervised experiences in psychological assessment, individual and group psychotherapy with children, adolescents, and/or adults. Interns will be required to participate in group training experiences with residents in psychology as well as in individual supervision. Rotation will be through outpatient and inpatient facilities as well as through child–adolescent and adult services. The group is interdisciplinary and each intern will have an opportunity to associate with and learn from social workers and psychiatrists as well as psychologists. Application is rather informal and should be accompanied by a vita, a transcript, and recommendations. Stipend is $7,000. Applications should be made to Joseph D. Warner, 500 Wainwright Building, Norfolk, Virginia 23510.

University of Wisconsin Center for Health Sciences: Postdoctoral fellowship in clinical neuropsychology. Two-year postdoctoral training program with emphasis on neuropsychological assessment of children and adults with known or suspected CNS dysfunction, and research in brain–behavior relationships, including application of biofeedback and conditioning techniques to neurological patients. Applicants must have PhD in psychology. Stipend: $8,000 for 11 months, $3,600 of which is tax exempt. Applications for September 1974 now being accepted; *deadline is May 1, 1974.* For additional information write to Charles G. Matthews, Neuropsychology Laboratory, Department of Neurology, University of Wisconsin Center for Health Sciences, Madison, Wisconsin 53706.

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Eastern Psychological Association: April 18–20, 1974, Philadelphia; April 3–5, 1975, New York City

For information write to:

Murray Benimoff
Department of Psychology
Glassboro State College
Glassboro, New Jersey 08028

Western Psychological Association: April 25–28, 1974; San Francisco

For information write to:

Milton R. Blood
Department of Psychology
University of California
Berkeley, California 94704
or
Georgia Babladelis
Department of Psychology
California State University
Hayward, California 94542

Southeastern Psychological Association: May 1–4, 1974; Hollywood, Florida

For information write to:

Edward H. Loveland, Secretary-Treasurer
School of Psychology
Georgia Institute of Technology
Atlanta, Georgia 30332

Midwestern Psychological Association: May 2–4, 1974; Chicago

For information write to:

James H. McHose
Department of Psychology
Southern Illinois University
Carbondale, Illinois 62901

Southwestern Psychological Association: May 2–4, 1974, El Paso, Texas; April 17–19, 1975, Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Rocky Mountain Psychological Association: May 8–11, 1974, Denver; 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque

For information write to:

Irwin Cohen
Veterans Administration Hospital
Mental Hygiene Clinic
1055 Clermont Street
Denver, Colorado 80220

Southern Society for Philosophy and Psychology: April 11–13, 1974; Tampa, Florida

For information write to:

Michel Loeb
Department of Psychology
University of Louisville
Louisville, Kentucky 40208

Symposium on Dyssocial Behavior Control (Psychosurgery): March 7–8, 1974; Coronado, California

For information write to:

Lynn Smith
Porter Memorial Hospital
2525 S. Downing
Denver, Colorado 80210

Council of Psychoanalytic Psychotherapists: March 17, 1974; New York City

For information write to:

Mrs. Ruth Marcus
Administrative Secretary
162-05 89th Avenue
Jamaica, New York 11432

National Association of School Psychologists: March 17–21, 1974; Las Vegas, Nevada

For information write to:

Mrs. Elizabeth Day
Convention Chairperson
1111 34th Avenue
Sacramento, California 95822

American Association of Sex Educators and Counselors: The Process of Sex Counseling: March 20–23, 1974; Washington, D.C.

For information write to:

AASEC
3422 N Street, N.W.
Washington, D.C. 20007

Conference on Professional Issues in Behavior Analysis: The Evaluation of Behavior Analysis Personnel and Programs: March 28–29, 1974; Des Moines, Iowa

For information write to:

Anne Connolly
Conference Coordinator
Department of Psychology
Drake University
Des Moines, Iowa 50311

Conference on Evaluation in Alcohol, Drug Abuse, and Mental Health: April 1–4, 1974; Washington, D.C.

For information write to:

Berna Koren
462 Grider Street
Buffalo, New York 14215

Conference on Structure and Cognition: April 13, 1974; Geneseo, New York

For information write to:

Gerald Erchak or Russell Judkins
Department of Anthropology
SUNY College at Geneseo
Geneseo, New York 14454

Interdisciplinary Meeting on Structural Learning: April 20–21, 1974; Philadelphia, Pennsylvania

For information write to:

Joseph M. Scandura
University of Pennsylvania
3700 Walnut Street
Philadelphia, Pennsylvania 19174

American Society of Group Psychotherapy and Psychodrama: April 25–28, 1974; New York City

For information write to:

James M. Sacks
97 Columbia Heights
Brooklyn, New York 11201

American Society of Adlerian Psychology: May 24–26, 1974; Chicago, Illinois

For information write to:

Eugene J. McClory
Executive Secretary
110 South Dearborn Street
Suite 1400
Chicago, Illinois 60603

International Conventions

Sixth International Round Table for the Advancement of Counseling: April 7–11, 1974; Cambridge, England

For information write to:

Mrs. Lee Wichelns
Forwood Junior High Schools
2000 Westminster Drive
Wilmington, Delaware 19810
or
D. A. L. Hope
Brunel University
Kingston Lane
Uxbridge, Middlesex, England

First Mexican Congress on Behavior Analysis: April 8–10, 1974; Xalapa, Veracruz

For information write to:

José Enrique Díaz Camacho
Primer Congreso Mexicano Análisis de la Conducta
Juarez 81, Apdo. 270
Xalapa, Veracruz, México

Second Annual Congress of the European Sleep Research Society: April 8–11, 1974; Rome, Italy

For information write to:

Mario Bertini
Chair of Local Organizing Committee
Università Cattolica S. Cuore
Via della Pineta Sacchetti, 644-00168
Roma

German Society for Phenomenological Research: April 24–28, 1974; Berlin, Germany

For information write to:

Gerd Brand
Steinplatz 2
Wissenschaftszentrum Berlin
1 Berlin 12, Germany

Eighth International Congress of the International Association of Workers for Maladjusted Children: April 29–May 3, 1974; Lausanne, Switzerland

For information write to:

Secretariat: 8° Congres IAWMC
Avenue de Valmont 35
1010 Lausanne, Switzerland

Cheiron: The International Society for the History of Behavioral and Social Sciences: May 31–June 2, 1974; Durham, New Hampshire

For information write to:

Rand B. Evans
Department of Psychology
Conant Hall
University of New Hampshire
Durham, New Hampshire 03824

Fourth Annual Conference of the European Association for Behaviour Therapy: July 11–15, 1974; London

For information write to:

Lawrence E. Burns
Department of Psychology
Birch Hill Hospital
Rochdale, Lancaster, OL12 9QB, England

Eighteenth International Congress of Applied Psychology: July 28–August 2, 1974; Montreal

For information write to:

Secretariat of the 18th International Congress of Applied Psychology, Inc.
C. P. 242
Station Youville
Montreal, Quebec, Canada

Eighth Congress of the International Association for Child Psychiatry and Allied Professions: July 29–August 2, 1974; Philadelphia, Pennsylvania

For information write to:

Albert J. Solnit
Yale University
333 Cedar Street
New Haven, Connecticut 06510

Second International Conference of the International Association for Cross-Cultural Psychology: August 6–10, 1974; Kingston, Canada

For information write to:

J. W. Berry
Psychology Department
Queen's University
Kingston, Ontario, Canada

Fourth International Conference on Social Science and Medicine: August 12–16, 1974; Elsinore, Denmark

For information write to:

P. J. M. McEwan
Centre for Social Research
University of Sussex
Falmer, Brighton
Sussex, BN1 9RF, England

International Conference on Dimensions of Anxiety and Stress: September 2–5, 1974; Athens, Greece

For information write to:

Irwin G. Sarason
Department of Psychology NI-25
University of Washington
Seattle, Washington 98195

First International Congress on Obesity: October 9–11, 1974; London, England

For information write to:

Kim Solly
1st International Congress on Obesity
22 Montagu Street
London, W1H 2BR, England

Fifteenth Interamerican Congress of Psychology: December 14–19, 1974; Bogotá, Colombia

For information write to:

Liuz F. S. Natalicio
Secretary General
Interamerican Society of Psychology
P.O. Box 88 UTEP
El Paso, Texas 79968

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29–January 1, 1975; Nairobi, Kenya

For information write to:

S. H. Irvine
College of Education
Brock University
St. Catharines, Ontario, Canada

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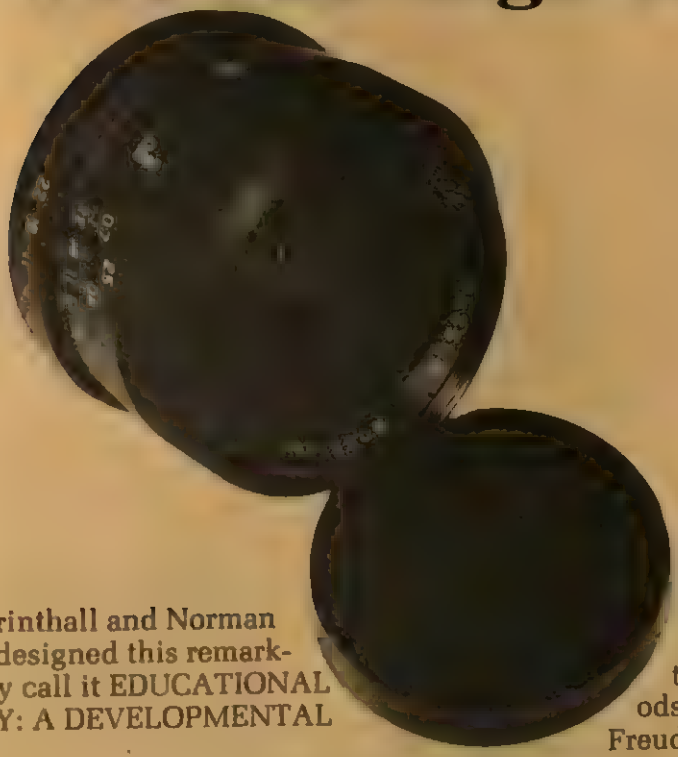

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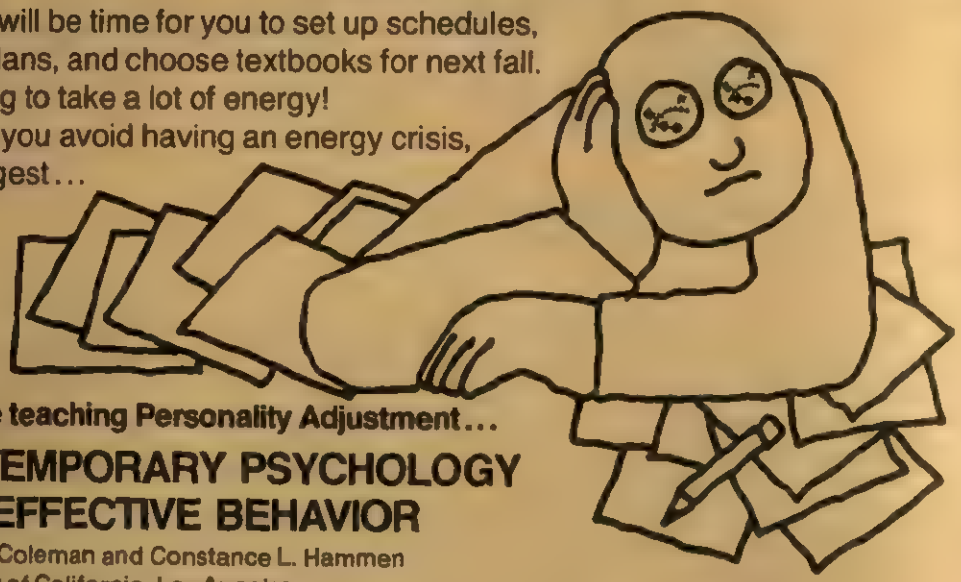

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Freedom and Dignity

A "Functional" Analysis

J. DENNIS NOLAN *Ohio State University* [1]

Behavior can be studied from a number of perspectives, and a "functional" [2] analysis of behavior could, in principle, be concerned with the relationship between behavior and a number of other variables. An approach that focuses primarily on the relationship between behavior and its effects on the environment has been called the *experimental analysis of behavior*, or sometimes, *operant analysis* (e.g., Skinner, 1969). An operant analysis of behavior centers not only on the topography or structure of a response, but also on the effects or consequences of that behavior. Behavior defined in terms of its consequences is said to be functionally defined (e.g., Ferster & Perrott, 1968). A technology developed by people working from an operant analysis perspective has been identified as behavior control technology, and interventions that draw upon this technology are often globally referred to as *behavior modification*.

In practice, functional analyses of behavior attempt to identify both the effects of the environment on behavior and the effects of behavior on the environment. When a functional analysis is directed toward "undesirable" [3] behaviors, it may help to identify environmental variables that could be modified if attempts to modify the undesirable behaviors were considered justified. However, in itself, a functional analysis does not imply that modification should be attempted, nor does it implicate any specific behavior as being the most appropriate alternative if it *is* decided that modification will be attempted. Once it has been decided that intervention will be attempted and, in addition, that the intervention will be based on behavior control technology, there is concern for behavioral alternatives to the undesirable behavior because specification of such alternatives is generally required in applications of that technology (e.g., Bandura, 1969). As a rule, specifications of behavioral alternatives are fairly precise, though in some cases they may not seem to be so (e.g., "reinforce anything else but that"). Even in the latter cases there is usually de facto specification, because some behaviors are more likely to occur than others and hence are more likely to be followed by "reinforcement." Since functional analysis is neutral with respect to the desirability of behavior, it necessarily cannot specify the alternatives. Therefore, such specification must be based on a value system, whether implicit or explicit (e.g., Bandura, 1969). Before a behavior modification program is begun, the designer of that program should be able to specify both the rationale for intervention and the outcome toward which the intervention is geared.

Skinner (1972) suggested that the use of behavior control technology can and should be extended to accomplish the design of a culture. Like any behavior modification proposal that involves the use of behavior control technology, this proposal requires the behavioral specification of the goal (in this case, a modified culture [4]) toward which the technological application is directed, as well as the rationale for intervening. Ideally, such a proposal would include a description of the ex-

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[1] The author is deeply grateful to Jaques W. Kaswan, Harold B. Pepinsky, and Joseph H. Spitzner for extensive criticism of an earlier draft, and to an anonymous reviewer for several very helpful suggestions.

Requests for reprints should be sent to J. Dennis Nolan, Department of Psychology, Ohio State University, 1945 N. High Street, Columbus, Ohio 43210.

[2] In this article, the term *functional* is used in a "Skinnerian" sense. For a consideration of ambiguities in the use of this term by social scientists, see, for example, E. Nagel (1961, pp. 520–535).

[3] The critical issues of "by whom" and "on what grounds" are not dealt with in this article. The interested reader is referred to T. Szasz (1960) and to D. Ausubel (1961) for two contrasting views.

[4] The difficult question "What is a culture?" is not the focus of this article. Skinner's (1972) conception of culture as a social environment or a set of contingencies of reinforcement, however arguable in its own right, will remain unchallenged in the present discussion.

pected effects of the proposed application of technology on as many aspects of the environment as possible. At the very least it should articulate the value system on which the specification of goals is based. Again, though, a functional analysis per se does not provide either specification.

Skinner (e.g., 1961a) asserted that survival is the only value that can reasonably be used to justify the design of a culture. This argument rests on two critical inferences drawn from a "directional" conception of cultural "evolution" (Skinner, 1972, p. 129). Additionally, he argued that science (specifically, operant analysis) offers the best perspective from which to implement his valued outcome. His rationale for introducing behavior control technology is that it is a scientifically based technology and that it is ethically neutral (p. 150). He implied that since science's models of human nature are "value free," a technology that is based on a scientific model is also value free. Hence, a concern for value judgments is largely beside the point (pp. 101–102). Further consideration of the philosophical and inferential issues involved seems warranted.

Skinner (1972) identified the strong commitment to the values of "freedom" and "dignity"[5] that exists in our culture and presented intriguing arguments about some likely effects of maintaining these values. Nevertheless, the strength of these values in our culture is simply a fact in a functional analysis, and such an analysis would necessarily uncover both their existence and prevalence. For Skinner, however, the commitment to freedom and dignity values is *undesirable* for two related reasons. First, the presence of these two values may be precluding, at least indirectly, the acceleration of the "evolution" of our culture (e.g., 1972, p. 163). Second, if such evolution is not altered (it is unclear whether simple acceleration would suffice) our culture will develop, or perhaps is already developing, a fatal flaw, which could allow some other culture to make a greater contribution to the future (p. 183).

It may indeed be reasonable to assume, with Skinner, that commitment to the values of freedom and dignity is affecting the evolution of our culture. That assumption is not questioned in this article. However, the conception of "rate" of cultural evolution, implied in Skinner's analysis, can be seriously challenged. In the absence of an independent value system, an argument for accelerating the rate of cultural evolution implies the assumption that such evolution is necessarily moving in a desirable direction. However, unlike biological evolution, cultural evolution is at least partially revocable and is not subject to natural selection as such (e.g., Mead, 1958; Simpson, 1958). Hence, its "directionality" is highly questionable (e.g., Popper, 1964, pp. 105–119). In the absence of direction, rate is not a very meaningful parameter of cultural evolution (e.g., Popper, 1964, p. 114). Therefore, the rate of cultural evolution cannot serve as the basis for rejecting the values of freedom and dignity regardless of whether such evolution is affected by those values. Even if cultural evolution is affected by those values, it is not evident that any such influence would be undesirable.

Skinner's second objection to the prevalence of the values of freedom and dignity is related to the first. It rests on the assumption that "our" particular culture should be preserved at the expense of any other culture. Since for Skinner (1972, p. 182) a culture is defined as a set of contingencies of reinforcement, his argument is essentially that one particular set of contingencies of reinforcement should be maintained at the expense of any other set. A clear statement of the rationale underlying his preference for that set of contingencies is needed. Skinner (e.g., 1961a, p. 34) equated survival of his culture with the survival of mankind, an enormous inferential leap for which he has provided no substantiation ("rate" of cultural "evolution" clearly does not serve that purpose). Even if it were granted that the survival of mankind is the only defensible value, it still would not follow that Skinner's culture, or any other particular culture, must survive in order for mankind to survive. In the absence of an articulated set of values relating to the ways in which people "ought" to behave, no reason for perpetuating a particular set of contingencies of reinforcement (culture) is self-evident, and, as a result, no reason for rejecting the notions of freedom and dignity is apparent.

A Rationale for Adopting a Technology

If it were possible to provide a satisfactory specification of end states, an argument for systematic introduction of behavior control technology could be based on the assumption that the application of that technology would be likely to accomplish a desired goal. However, it would remain essential

[5] Again, the difficulty in satisfactorily defining such terms as these is not at issue in this article.

to avoid concluding that what is technically feasible is desirable. Technologies that may accomplish desirable ends, while producing other states that are, or can reasonably be expected to become, intolerable, should also be avoided. The parallel with the development and proliferation of mechanical-industrial technologies is direct. The just now emerging, but long predicted, threatening side effects of such proliferation (e.g., pollution, the destructiveness of weapons) should induce grave concern. Unfortunately, Skinner does not seem to be concerned. For example, he suggested that the question of values is no more salient in the design of a culture than in the design of a bomb (Skinner, 1961b, p. 545). But different designs (whether of cultures or bombs) produce different costs and different side effects, some trivial, some nontrivial. The question of values arises in both cases, rather than in neither case, as Skinner implied.

Perhaps the most undesirable "side effect" of Skinner's proposal is that, in the absence of an independent basis for specifying cultural goals, the technology itself is likely to dictate those goals. (A quote from *Walden Two* in which Frazier says, "Eventually we shall have no use for Planners at all," suggests that Skinner considers this to be a *desirable* outcome [Skinner, 1962, p. 273].) A major danger in allowing the technology to define goals is that the adoption of a technology without independent consideration of goals probably limits the ways in which problems can be considered or construed and may also limit the number of alternatives that will be considered as possible solutions. The adoption of a technology, any technology, must await the emergence of some consensus about cultural goals, or some other basis on which to specify such goals.[6] Reliance on a technology to dictate the efficient, or "efficiently reachable" (in terms of that particular technology), goal would not insure a wide variety of possible end states and could easily preclude such a variety.

Skinner proposed to examine how a society must be designed if it is to achieve its ends through positive reinforcement. Note that this proposal is to base the design of the society on the efficient use of an available technology (in this case, positive reinforcement), rather than on the specification of a particular outcome and the *subsequent* design of ways of achieving that outcome. As desirable as it is to avoid the negative side effects that can accompany the use of punishment, the a priori selection of a "technology" that precludes the use of punishment does not insure the specification of good end states. It would be preferable to include concerns about side effects in the specification of end states, prior to the selection of a technology and then to find or devise a technology that can accomplish the specified end state. Such a technology may or may not include punishment (e.g., Solomon, 1964), but in either case, its adoption would be based on the expectation that the technology could accomplish what was wanted, not on an arbitrary a priori basis.

Consider Skinner's position with respect to the elimination of unwanted behavior. Skinner (e.g., 1972) suggested that, from a purely technological standpoint, a good long-run way to eliminate unwanted behavior is to completely eliminate the positive reinforcement that maintains the unwanted behavior such that it eventually becomes extinguished. One way of facilitating this process is to provide positive reinforcement for behaviors that are incompatible with the unwanted behavior. However, this procedure requires the specification (either precisely or imprecisely) of a repertoire that is considered rewardable, a feat that societies generally seem much less capable of accomplishing than the specification of unacceptable repertoires. Societal judgments seem reasonably consistent with respect to what is not tolerable, but not with respect to what is inherently valuable. Hence, procedures designed to suppress "unwanted" behaviors are rationally more defensible than those designed to increase incompatible behaviors, since the specification of behavioral alternatives cannot be made on any grounds but expedience. If society can only specify unacceptable repertoires, pervasive reliance on a technology that is designed to produce desired repertoires would not make sense.

End states involving standardized codes for dress, hair style, etc., which exist in many of our institutions (e.g., schools), often seem to precede, rather than derive from, the "value" with which they are identified when challenged (e.g., "propriety," "neatness," "custom"). Sometimes it seems as if these values are identified only when the end state has been challenged. Skinner (e.g., 1972, p. 105) could suggest that such values are simply classifications of things in terms of their reinforcing effects; they are simply "facts" determined by evolution and do not need to be related to some independent value system. However, there is an-

[6] Even the emergence of such consensus would not necessarily validate pervasive cultural design (e.g., Popper, 1964, pp. 152–159), but that issue is beyond the scope of this article.

other plausible interpretation. Behavioral standardization may be likely to be specified as a goal simply because it is technologically feasible to attain that goal. For example, it would seem plausible for a school to try to maximize individual differences rather than to standardize behavior and to search for a technology that could be of help in achieving that goal if the current technology either is not capable of doing so, or cannot efficiently do so. If a behavior control technology were adopted on a priori grounds, and if no independent basis existed for the specification of end states, the specification of end states like maximizing individual differences would be less likely, since, as Skinner himself implied (e.g., 1972, p. 162), these goals are less efficiently reached by such technologies.

Regardless of whether science is ethically neutral, a particular technology at any given stage of development will not be equally efficient at accomplishing all goals. In the absence of an independent basis for specifying goals, the technology is likely to dictate those goals which it can most efficiently accomplish. Such goals are not necessarily ethically neutral. It may be noted that Skinner (e.g., 1972, p. 162) also criticized institutions like schools for breeding uniformity, and he argued for planned diversity. Yet he did not articulate a rationale for specifying diversity as a goal. In the absence of such a rationale, it is not likely that goals like diversity will be dictated by behavior control technology itself.

Skinner suggested that our current technology, however unsystematic it or its applications may be, also controls behavior—whether or not such control is planned. It may be true that the technologies currently employed do produce outcomes that are predictable, at least in principle. Even if this assertion is valid, it does not follow that we should therefore *value* an increased ability to make such predictions, nor does it follow that we should value the specification of end states or the ability to control them. Such values do not derive from functional analyses. In effect, the specification of end states is only required *if* behavior control technology is adopted. It is a technological requirement rather than a scientific requirement, and in the absence of the technology no reason for valuing the specification of end states would be self-evident. As Freedman (1972) pointed out, it is possible to use scientific principles similar to those employed by Skinner to devise a model of a good society quite different from the one that Skinner proposed. In order to do so, one need only specify end states on the basis of a value system different from Skinner's unarticulated value system. If such specifications were made, behavior control technology might indeed be useful. However, there is no warrant for assuming that reliance on technology for the specification of end states would be more likely to result in a "good" culture than our current haphazard (from Skinner's perspective) procedure.

In summary, there are at least three good reasons at present to avoid strong reliance on behavior control technology for "improving" human societies. First, Skinner's assumption that a good end state will result from an accelerated rate of cultural evolution can be rejected on logical grounds, thereby rendering the adoption of the technology pointless. Second, although behavior control technology might accomplish some desirable goals, if it were adopted in the absence of an independent value system it would also produce undesired side effects in the process, as well as unintended and unexpected effects (e.g., Popper, 1964, pp. 64–70). Finally, the precise specification of end states may not be valued (and may even be devalued). If so, the adoption of behavior control technology would lead to an undesirable goal, because that technology will itself dictate end states.

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Motivation and the Cognitive Revolution

WILLIAM N. DEMBER *University of Cincinnati*[1]

A great deal has happened to psychology in general and motivation in particular in the decade since I prepared the article "The New Look in Motivation" (Dember, 1965). The most striking development—significant enough to be termed *revolutionary*—is one that I will attend to exclusively in this article: Psychology has gone cognitive, and so has motivation. Indeed, part of the impetus for the movement of general theory and research toward cognition was provided by dissatisfaction among motivational theorists with the prevailing stimulus–response (S–R) behavioristic model that had chararterized American psychology for several decades.

The purpose of the present article is twofold: (*a*) to elaborate on the assertion above that motivation has actively participated in the cognitive revolution and (*b*) to point out some behavioral phenomena, more from the "real world" than from the laboratory, that are not yet satisfactorily handled even within modern, cognitively oriented motivational models. These are phenomena, nevertheless, that demand a cognitive interpretation. What I would like to accomplish here, above all, is to prod future theorists into a "newer look" in motivation, one which can cope with these compelling, but elusive, phenomena.

This article is organized according to its dual aims. First, I will discuss the cognitive revolution both generally and also specifically as it pertains to motivation. Then, I will indicate some kinds of behaviors which I believe must be accommodated by any motivational model that strives to be comprehensive. The theme of this article, then, might be, "You've come a long way, baby. But the trip isn't over yet."

Cognition in Psychology

One of the assets of science is its cumulative nature. New data supplant old; new ideas replace outworn ones. The growth of scientific theories may be continuous, through a process of modification and accommodation, or it may at times be discontinuous, through the introduction of radically new concepts or, in Kuhn's (1962) term, *paradigms*. In either case, there is in the history of science a sense of linear progression.

The history of psychology certainly contains its share of growth. New data typically are better than old, largely a result of improved techniques and methodology. And new theories generally are at least more sophisticated than their predecessors, by virtue both of the existence of better data and of access by theorists to more highly developed symbolic (often mathematical) structures on which to hang their ideas. Nevertheless, there is also in psychology a disappointing lack of conceptual progress: Instead of developments which are incremental, we seem all too often to witness a kind of circularity. Our newest ideas are terribly reminiscent of our oldest ones; they carry us around the board and back to "Go." Hopefully, these paths will ultimately turn out to be expanding spirals rather than circles, and conceptual progress will be made.

It is too early to know whether what I am calling the cognitive revolution is really that or whether psychological theory has just come full circle. Be that as it may, what is quite clear is that cognitive concepts have begun to pervade many areas of psychological theory.

Psychology was said to have "lost its mind" with the advent of Watsonian behaviorism in the second decade of this century. In his attempt to rid scientific psychology of data achieved through nonobjective means (a goal with which it is hard for scientifically oriented psychologists to quarrel), Watson also cleansed the field of mentalistic concepts. In the textbooks, until very recently, psychology was no longer defined as "the science of experience," but rather as "the science of behavior."

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[1] This article is based on talks given at Xavier University and Edgecliff College in Cincinnati, at Western College in Oxford, Ohio, and at the University of South Florida in Tampa.

Requests for reprints should be sent to William N. Dember, Department of Psychology, University of Cincinnati, Cincinnati, Ohio 45221.

Problems that seemed to rely on introspection-generated data for solution were taboo; feelings, images, percepts, ideas—as concepts within a theory—were proscribed. Theory was to be constructed, if at all, out of physical "stimuli" and muscular "responses."

That confusion of methodological purity with conceptual barrenness had at least two implications. First, it deprived psychology of most of its interesting problems. Second, S–R theorizing proved inadequate to the task.

Memory and Language

S–R theory, with its close conceptual ties to Pavlovian conditioning and instrumental learning à la Thorndike and Skinner, is most readily applied to simple learning situations: for example, salivary conditioning in dogs, T-image learning by rats, key pecking in pigeons, eyelid conditioning in human subjects. Even in those settings it has its inadequacies, but they only become serious when the S–R model is stretched to fit more complex situations. In particular, the weakness of S–R theory has been most clearly exposed in two areas of research, human memory and language.

In the case of memory, to summarize a vast and complex literature in a brief paragraph, the evidence is quite compelling that in storing and retrieving large numbers of items, people employ active strategies; they do not function as passive receptacles, or *tabula rasa* on which associative bonds are imprinted and then remain available for later activation when the proper stimulus comes along. One such strategy is called *chunking*. As the word suggests, several separate items are formed into a single unit and by that transformation become less of a burden on the memory system, as, for example, when the following list of eight digits, 1, 5, 2, 0, 1, 9, 0, 6, is stored and retrieved as the four units "fifteen, twenty, nineteen, six." Another strategy that people follow in learning and remembering arbitrary associations is to employ images as mediators among the items to be associated. It turns out that weird, or bizarre, images are especially good for this purpose (e.g., see Segal, 1971).

The recent study of language behavior has had profound impact on psychological theory, much of it attributable to the contributions of the linguist Naom Chomsky and his colleagues (e.g., see Chomsky, 1959). One target of this group is an S–R analysis of language proposed by Skinner (1957). The arguments are complex and beyond the scope of this article. The outcome is quite clear, however. Language, the hallmark (if not the unique possession) of human beings, cannot be treated simply as a set of associations acquired according to the principles of operant conditioning and elicited by arbitrary stimuli. It remains to be seen whether Chomsky's own "generative grammar," with its assumption of innate "deep" linguistic structures, will prove an adequate substitute for a simple behavioristic model. In any event, neither "psycholinguistics" nor general psychology will go untouched by the Chomsky-stimulated developments. Again, perhaps grossly to oversimplify, the point here is that it has become respectable, if not essential, to restore to psychological theory those complex, "cognitive" processes that behavioristic models tried valiantly to get along without.

In the case of language it is quite evident that theoretical progress has been made. We are not simply back where we were when Watson got us off the track. First, the careful, self-conscious efforts of Skinner and others to apply S–R theory to language behavior has made it possible to identify the specific deficiencies of such an approach. Second, the recent theoretical developments are far more articulate and precisely formulated than their largely intuitive pre-Watsonian forebears. Moreover, they are based on, and have generated, a solid body of clearly conceived and executed empirical research (e.g., see Dixon & Horton, 1968).

Perception

For the most part, the field of perception has been relatively impervious to the impact of behaviorism. Researchers and theorists in perception reacted to the strictures of behaviorism primarily by attending more carefully to their methods, but the problems they investigated and their conceptual models proceeded along relatively smooth and continuous paths. It may not be irrelevant that the major figures in perceptual theory (the Gestalt psychologists, Wertheimer, Koffka, and Köhler) during the ascendancy of behaviorism totally rejected its tenets as they applied specifically to perception and cognition as well as generally to the way the brain worked (the organ which presumably controls all psychological processes).

In the sense that perceptual theory has gone its own way over the past 50 years, it has also not been much affected by the recent cognitive revolution. Indeed, one major development in perceptual

theory, spanning the 1940s and 1950s, can be said to have anticipated the trends becoming apparent in other areas. I am referring to the "new look in perception," which, as pointed out in my "New Look in Motivation" article, argued for cognitive and affective influences on such basic perceptual processes as the recognition of and attributive judgments (e.g., How large is this coin?) about stimulus objects. This approach to perception represented a reaction, not against behaviorism, but rather against the Gestalt emphasis on purely stimulus determinants of perception. Of course, the Gestaltists' conception of what constituted a "stimulus" was itself quite dynamic; it insisted on the relational nature of perception, whereby the perceived value of a particular stimulus element is determined jointly by its own physical properties *and* the properties of the total stimulus field in which it is embedded. For example, a given spot of illumination can be made to appear either black or white depending on the intensity of the surrounding illumination.

Proponents of the "new look in perception" argued, in effect, that the definition of the embedding context be extended to include properties of the perceiver, such as his motives, habits, conflicts, and expectancies. How successfully that point has been demonstrated empirically remains, unfortunately, an open issue. Nevertheless, by asserting the influence on perception of cognitive, affective, and motivational processes, the new lookers did, perhaps inadvertently, open up the possibility of influences in the other direction as well. That is, as I have argued earlier, the "new look in perception" in part paved the way for a "new look in motivation."

Motivation

There is obviously no need to retell the whole new look in motivation story. It can be summarized in a sentence: Human beings and lower animals are attracted by, and their behavior is reinforced by, the informational properties (e.g., novelty, complexity) of stimuli in much the same way that they are influenced by stimulus intensity or whatever it is that gives potency to the conventional, "homeostatic" incentives and rewards. For the present purpose, however, that sentence can usefully be elaborated. But first, I simply want to assert that an impressive body of research, obtained from a wide variety of subjects, animal and human, young and mature, conducted since my new look in motivation article was written, overwhelmingly supports the basic point in the summary statement above. That is not to say that the literature, like others, does not contain its share of confusions, contradictions, and controversies. Nevertheless, the motivational significance of information is by now firmly based in sound, empirical evidence.

It is the relation of this development in motivational theory to the "cognitive revolution" that needs further, but brief, explication. In effect, over the past three decades a profound change has taken place in the manner of conceptualizing those events considered motivational in nature (drives, incentives, rewards, etc.). In Clark Hull's (1943) highly influential *Principles of Behavior*, those events were tied directly to conditions of psysiological imbalance, or "need." Behavior was instigated by departures from optimal physiological states and reinforced, or "rewarded," by restoration of physiological balance. Hunger (operationalized as "food deprivation") and reinforcement of hunger-reducing behaviors (through ingestion of food) provided the prototype of the processes linking motivation and learning. Hull's choice of a physiological definition of motivational variables was dictated by his general biological, Darwinian orientation to psychology and his concern for objective, operational anchors for "tying down" psychological concepts.

But Hull's strictly biological approach proved too confining; it was relatively easy to find instances that contradicted the expected one-to-one correspondence between biological needs and behavioral indicants of motivation. Thus, needs could be cited which seemed to have no direct behavioral consequences, and behaviors indicative of strong, underlying motives could be identified for which there seemed to be no corresponding need. The realm of behaviors referred to as "exploratory" or "curiosity motivated" provided a rich source of compelling examples.

Hull's student Neal Miller dealt with this problem by redefining motives (*drives*, in his terms) and rewards. A drive, in Miller's scheme (e.g., see Dollard & Miller, 1950), was "any strong stimulus." Now, strong stimuli *can* arise from biological needs (e.g., the hunger pangs that often accompany prolonged food deprivation), but they are not tied exclusively to needs. Rewards, in Miller's scheme, are provided by the reduction in the intensity of strong stimulation: reward = drive reduction. Such rewards (e.g., food for a food-deprived animal) may be need reducing but, again, not necessarily.

In effect, Miller gave a *sensory* definition to Hull's basic motivational concepts, and thereby started motivational theory down the cognitive path. The crucial motivational aspect of stimulation, for Miller, was its intensity—intensity as a *physical* property of stimuli. Tying drives to physically measurable properties of stimuli in this way allowed Miller the objectivity required of his general S–R behavioristic approach. But it had the same confining effect as Hull's tying motives to biological needs. It became all too easy to find examples (in "thought experiments") of motivational events, the impact of which was related not to their physical properties per se but to their significance or meaning to their perceiver. These examples include instances in which increasing stimulus intensity is rewarding and where decreasing intensity of stimulation might increase, rather than reduce, drive level. Consider, for example, a person lost in a cave, searching for an exit. It is totally dark. Suddenly he sees in the distance a dim light; he moves toward it, and as the light gets brighter he realizes that he is seeing sunlight through an opening in the cave. Surely the increase in light intensity is a rewarding event. Now, reverse the sequence, the imagine being alone at night in a strange house; suddenly all the lights go out. Is that a rewarding event?

Clearly, stimulus intensity by itself cannot carry the whole motivational burden. Changes in intensity of stimulation can be either drive reducing or drive inducing, depending on the total context in which they occur. In other words, without knowing certain things about the perceiver of the stimulation, it is unlikely that taking physical measurements of stimulus intensity will provide a sufficient basis for inferences about motivational states.

One can remain committed to intensity as the key motivational variable and still get out of the trap Miller set for himself by giving up physicalism and turning inside the organism. What counts then is not necessarily stimulus intensity (although it may be highly relevant in many instances), but the organism's internal responses to stimulation, as evidenced perhaps by level of activation of the autonomic or the central nervous system. In short, the key variable becomes "arousal" intensity, instead of stimulus intensity. Drives, then, might be defined as states of high arousal, and rewards as reductions in arousal level. From this point of view, stimulus seeking, exploration, and so on are seen as behaviors that generate increases in stimulation in the service of reducing the high arousal level which accompanies sensory deprivation or boredom (e.g., Myers & Miller, 1954). Berlyne (1960) built an entire theory of curiosity and aesthetics around that notion.

Now, what the arousal model does in essence is to collapse all possible psychological dimensions onto the single dimension of physiological arousal. That is, it is arousal level that determines which behaviors will occur; what the determinants of arousal itself might be are of secondary interest. So, if one could tap the arousal lines and read arousal level on a voltmeter, then one would have access to all the information necessary to make accurate behavioral predictions.

Such an approach might prove valid, but even so it seems unsatisfying. Too many issues are skirted. Psychologists do care about what people perceive and think and why, for example, certain events are arousing for some people at some times. It may be that ultimately these various psychological processes do all pass through a single blender ("arousal") and come out undifferential glop at the other end, but what they were like before becoming homogenized may still be worth asking.

An alternative approach to one that employs arousal as the mediating variable is to stick with events as perceived or otherwise processed ("understood," "categorized," "associated to," "imaged," or what have you) and to search for the properties of these events that relate to the behavioral choices that people and animals make. That class of models, referred to earlier under the general rubric of "the new look in motivation," which emphasizes the information value of events, is one such alternative approach to an arousal model For present purposes the point to be made is that "new look" models clearly invoke processes that would generally be classified as "cognitive." It is in that respect that these models have moved motivational theory, along with other areas of psychology, in the cognitive direction.

The same impetus has come from other sources as well. Cognitive dissonance theory, for example, is concerned with the generation of a drivelike state through cognitive manipulations; indeed, some dissonance research has demonstrated that conventional drives, like thirst and hunger, can be influenced by cognitive-dissonance-arousing manipulations, for example, as when the level of hunger is reduced following a person's voluntary commitment to go several additional hours without eating following an agreed-on period of food deprivation (Brehm, 1969; see also Zimbardo, 1969, for a series

of related articles). Similarly, an impressive array of experiments has shown that the emotional impact of autonomic arousal is highly dependent on the context in which the arousal occurs—context provided by manipulations that clearly are mediated by cognitive processes (e.g., Schachter & Singer, 1962).

The conception of motivation in American psychology has thus considerably broadened over the past three or four decades. One effect of this development has been to allow increasingly complex and increasingly interesting behavioral phenomena to be incorporated into a scientific framework. The burden of the following section is to point to behaviors which are certainly interesting but which cannot yet be handled in any obvious way within current motivational theories, even the most cognitive of them. These behaviors suggest that "ideas" can be very potent motivators, not just mimicking, moderating, or influencing the "biological drives" but even at times overriding them.

The Motivational Potency of Ideation

Before proceeding, we need to agree on the nature and intent of what follows: (*a*) The material is presented not as evidence supporting a particular hypothesis, but rather as a collection of anecdotes and observations which, en masse, are suggestive of a hypothesis; (*b*) no anecdote or behavioral observation is subject to only one convincing interpretation; many of the items that are cited could be employed, as some have been, to make a different case from the one I am advocating; (*c*) so, my purpose here is to parade before you examples of behavior which suggest to me a serious gap in current motivational models—again not to make a case for a hypothesis but to indicate why it might be worthwhile to entertain, develop, and ultimately try to test a hypothesis of "the motivational potency of ideation." Let the parade begin.

LABORATORY STUDIES OF COMPLIANCE

Virtually every experiment in psychology is, willy-nilly, a study in compliance. Potential subjects write their names on sign-up sheets, show up with remarkable reliability at the right time and place, and then proceed to follow as best they can the instructions given to them. We typically pay little attention to this chain of compliant acts; if we had to account for it, we would most likely turn to explanations that involved such concepts as role playing ("being a good subject") or specific motives, such as "conformity," "need to please authority figures," "altruism," perhaps even "curiosity," and so on.

Of course, and more to the point, there are countless experiments specifically designed to investigate compliant behavior. In these, the centrality of compliance is highlighted because it occurs in the face of serious counterpressures. For example, in Asch-type conformity experiments (e.g., Asch, 1956), subjects go along with group consensus even if it requires making perceptual judgments which are far from veridical. More dramatic are the obedience experiments of Stanley Milgram (1963), in which subjects followed the experimenter's orders, often with considerable personal distress, to apply what they believed to be increasingly intense electric shocks to fellow subjects serving in a purported learning study. Apparently, ordinary people can be induced, by relatively uncoercive means, to engage in what would be viewed outside the laboratory as highly aggressive, perhaps sadistic, acts. But these were not sadists; they were simply normal people following orders. They could have refused to comply, but very few did.

For many such subjects, what they were doing was personally repugnant, but they did it anyway. Why? The simplest answer would seem to be "because they were told to" by a credible, persistent "scientist," to be sure. That is, people do not blindly follow all orders; the setting needs to be just right. But in the right setting, being told to do something is tantamount to being motivated to do it. The experimenter's instructions, once "planted," take on the function of powerful, internally generated drives. (I wonder how hungry a person would have to be before he would deliver what he believed to be very painful shocks to a fellow subject in order to get access to food.)

HYPNOTIC SUGGESTION

The power of hypnotic suggestion is well known. Hypnotized people can be induced to engage in a wide variety of unusual behaviors, some of them quite antisocial or self-destructive. The literature on hypnosis is vast and confusing. Indeed, there is serious dobt as to whether it is necessary to invoke a special state of consciousness to account for hypnotic behavior (e.g., see Barber & Calverley, 1963). Attempts to uncover the essence of hypnotic behavior have revealed that nonhypnotized subjects, either faking or simulating hypnosis, do much, if

not all, of what "genuinely" hypnotized subjects do. That is, people can accurately "role play" the hypnotized subject. Whether that means there is nothing more to hypnosis than serious role playing is an open question. I allude to these observations here as a reminder that people, either under hypnotic suggestion or just simulating hypnosis, can be programmed, via the hypnotist's verbal instructions, to engage in behaviors that have all the earmarks of an intense underlying motivational state. That is, if one were to observe their behavior without knowing anything of its origins, one would be hard pressed not to give it a motivational interpretation.

REAL-WORLD EXAMPLES

My own interest in the issue under discussion was aroused not by laboratory research of the sort alluded to above, but rather by examples of behaviors reported in the news media that seemed to demand a motivational interpretation, but for which the standard, "dynamic" interpretations were vaguely unsatisfying. They all involve destructive and/or self-destructive acts—not too surprisingly given the temper of the times. Consider just four of a myriad of examples:

- A young boy died a slow, painful death after ingesting some solvent he had stolen from the school shop. Apparently some of his friends had dared him to do it.
- A 24-year-old truck driver won a $500 bet from two of his friends by jumping off the George Washington Bridge—a 212-foot plunge into the Hudson River. He did not live to collect the bet (reported in the *New York Times*, May 9, 1971).
- Diana Oughton, daughter of a wealthy midwestern family, described as sensitive and warmhearted, was killed with two Weathermen companions when bombs that they were constructing in a Greenwich Village apartment exploded (extracted and paraphrased from a book review by Fred J. Cook, in the *Saturday Review*, May 1, 1971, of a biography, *Diana*, written by Thomas Powers and published by Houghton Mifflin).
- An assistant pastor and a layman of the Holiness Church of God in Jesus Name, of Carson Spring, Tennessee, died in agony after drinking a mixture of strychnine and water—testing their faith in the Bible, where in Mark 16:16–18 it is asserted of those that believe that "if they drink any deadly thing, it shall not hurt them" (described more fully in *Newsweek*, April 23, 1973).

The last two examples, Diana and the strychnine drinkers, are illustrative of what I believe is the most potent form of ideation, that is, political or religious *ideology*. I do not know whether we are dealing with a continuum or with qualitatively distinct categories, but clearly when ideology prevails, ordinary motives look pale and insignificant. Think of Mahatma Ghandi going for weeks without eating; think of countless religious martyrs and political prisoners undergoing torture and death in the service of their faith, their party, or their country. Think of what must be otherwise gentle, loving, moral men and women killing and maiming innocent men, women, and children so that their side can win out in the current conflict in Ulster.[2]

The point, if its not already obvious, is that some of the dramatic behaviors demanded by a particular ideology must occur in the face of considerable counterpressure. By analogy with the old obstruction box method for assessing the relative strengths of rats' drives (how much electric shock will a rat endure in order to get access to a given type of incentive?), the intensity of this presumed counterpressure is suggestive of the extreme potency of the ideology-based motivation.

Needed: A Good Model

How can we transform these observations and inferences into a scientifically respectable theoretical model so that, among other things, they become susceptible to empirical investigation? I wish I knew. I must confess that I do not even know what form such a model might take, more, I hope, than just a collection of carefully worded sentences.

I am ready to suggest that one lead to model building in this area is provided in D. O. Hebb's (1949) *The Organization of Behavior*. Hebb made the cogent point that the directionality of a train of thought is just as much the expression of a motivational process as are the choices a rat makes running in a maze. In the case of ideology and its less intense counterparts, we seem to have trains of thought, or rather whole "transportation systems," dominating other sources of behavioral control. Furthermore, considering ideology in particular, we have not only a dominating system of ideation, but one that is capable of overriding all other motiva-

[2] My colleague, Harold Fishbein, has called to my attention some very pertinent comments about aggression and ideology by Arthur Koestler. (See especially Koestler, 1967, pp. 225–239.)

tional influences. Thus, whereas Stanley Milgram's subjects suffered great emotional stress in delivering intense electric shocks to their victims, I suspect that blandness, or else pleasant excitement, more nearly characterizes the emotional state of the ideological assassin. That is, the conflict is minimal as ideology overrides conscience or even impulses toward self-preservation.

Override, then, is one important property of ideology. Another, I think, is analogous to the physiological dependence that is associated with drug addiction. Call it *ideational addiction*. That is, the ideological convert is captured by his ideology; it is not just dominating, but also pervasive, persistent, and insatiable. Unlike ordinary motives, which are cyclic, ideology is constant, and instead of being reduced through consummatory activity it is either unaffected, or perhaps even enhanced.

This notion of ideological addiction might be handled in the language of a model that would also accommodate the property of override, by a concept such as *locking in*. That is, once an ideological addiction has occurred, it becomes, in effect, a permanent structure in the system. As such, I would guess, ideological addictions are much easier to acquire than they are to get rid of. Indeed, it may be that a new ideology must be available to substitute for an old one, if the old one is to be dropped—very much, it seems, as is the case with Kuhn's scientific paradigms referred to at the beginning of this article.[3]

Cognition and Evolution

It may be of some interest to consider the foregoing in the context of evolution. In that context, the morphological structures through which an organism's behavioral repertoire is put into effect (the limbs, glands, and other "effector organs") serve as means to assure individual and species survival. And the same holds for the structure, the nervous system, which integrates and controls the activity of the several effector organs. In that sense, the nervous system in general and the brain and cerebral cortex in particular have developed as adaptive mechanisms. To simplify grossly in order to make the point, the brain is the servant of the stomach and the sex organs: Cognitive processes are instruments, means to an end.

One implication of the theme of this article is that the tables may have turned in the course of human evolution. Or at least the relation between master and servant has become more nearly symmetrical: The stomach exists, if you will, as much to nourish the brain as vice versa. Cognition is at times instrumental, and at other times an end in itself, with its own demands. Without adequate input, in the form of information, the cognitive system begins to deteriorate. To ward off cognitive malfunctioning, information-searching behavior is initiated in response to signals (boredom) analogous to hunger pangs, and so on, through the whole story of curiosity, information seeking, exploratory behavior, and the like.

To make the point in a somewhat different way, the brain is not like a computer, which will do its thing on demand and then sit silently until called on once again to perform. The brain, rather, is an instrument with needs of its own. And some of the processes that it mediates (that we loosely call "cognitive") may achieve states which are as demanding of *themselves* as are the organ systems that those processes were originally developed (through evolution) to serve. Indeed, as in the case of those self-destructive behaviors alluded to earlier, the demands of ideology may at times be utterly inimical to the demands of biology.

A Final Comment on Rationality

One of the aims of psychoanalytic psychotherapy, at least as Freud (1962) viewed it, is "to enable the ego to achieve a progressive conquest of the id [p. 46]." Thanks to Freud, and to many other pre- and post-Freudian writers—theologians, philosophers, psychiatrists, psychologists, novelists, playwrights—we have come to take for granted the position that evil is associated with the irrational and good with the rational. This assumption is nicely illustrated in a review of the book *Diana* (whose story was alluded to earlier) which contains the following sentence: "At some risk of oversimplification, it is nevertheless safe to say that in Diana Oughton, as in others, emotions came to rule the brain [Cooke, 1971, p. 35]." The reviewer may be right in that particular instance, but I would like to suggest the plausibility of just the opposite interpretation: Diana's ideology came to rule her emotions. More generally, I would call into question the easy assumption that the domi-

[3] I wish to thank William J. McGuire, who offered this suggestion after innocently asking me, on a ride to the airport, what I was working on these days.

nance of behavior by rational, "cognitive" processes will necessarily assure personally and socially desirable outcomes. On the contrary, there may be as much to fear from unbridled ideation (especially in its extreme form, ideology) as there is from unconscious fantasies and impulses or unrestrained emotions.

Perhaps the point is that the domination of a complex system by any one of its components—even a "good" component—may lead to highly undesirable consequences. In the days of Watergate, when these words are being written, an obvious political analogy immediately comes to mind. In personality, as in politics, the safest, if not the most efficient, system is one with multiple components, or "institutions," that exist in some kind of tense equilibrium (id, ego, superego; Congress, President, Supreme Court). And perhaps to keep the system truly healthy there is need for additional external institutions (family, friends, therapist, society; a free press, other governments) keeping an objective eye out for its integrity and giving constructive feedback when the system seems to be getting out of balance.

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Teaching Children to Read Using a Computer

RICHARD C. ATKINSON *Stanford University*[1]

For the past 10 years I have existed in two quite separate worlds. One world is that of an experimental psychologist working in the isolation of a university laboratory on problems of memory and cognition; the other is that of an applied researcher attempting to computerize the instructional process. It would seem that there should be a fair amount of commerce between these two worlds, but I am disappointed to find that very few of my colleagues in memory and learning are aware of the work on computerized instruction, and this situation is equally true for my friends in education. Therefore, this article gives me an ideal opportunity to propagandize a bit for the potential that each of these fields has for the other.

This article is primarily descriptive, focusing on work that we have been doing at Stanford in teaching reading to first-, second-, and third-grade children. Let me emphasize, however, that there is a clear link between this work and basic research on memory and cognition. I like to refer to this link as a "theory of instruction." By that phrase I do not mean a highly formalized theory, but rather a loose collection of theoretical and empirical facts that can be used in conjunction with educational methods to design optimal procedures for instruction. Having written about the ingredients for a theory of instruction, I will not spend time on that topic here.[2] Simply stated, there are examples in which psychology provides powerful tools for devising optimal procedures, particularly when instruction can be brought under computer control. I will refer to several of these examples, but will discuss only two in detail.

Computer-Assisted Instruction and the Reading Curriculum

Our first efforts to teach reading under computer control were aimed at a total curriculum that would be virtually independent of the classroom teacher.[3] These early efforts proved reasonably successful, but it soon became apparent that the cost of such a program would be prohibitive if applied on a large-scale basis. Further, it was shown that some aspects of instruction could be done very effectively using a computer, but that there were other tasks for which the computer did not have any advantages and possibly had some disadvantages over classroom teaching. Thus, during the last four or five years, our orientation has changed and the goal now is to develop low-cost computer-assisted instruction (CAI) that supplements classroom teaching and concentrates on those tasks in which individualization is critically important. A student terminal in the current program consists only of a Model-33 teletypewriter with an audio headset (see Figure 1). There is no graphic or photographic capability at the student terminal as there was in our first system, and the character set of the teletypewriter includes only uppercase letters. On the other hand, the audio system is extremely flexible and provides virtually instantaneous access to any one of 6,000 recorded words or messages.

The central computer which controls the CAI system is housed at Stanford University. Telephone lines link the computer to student terminals located in schools near the University and as far away as Florida, Oklahoma, Texas, and Washington, D.C. First-, second-, and third-grade students receive CAI reading instructions for anywhere from 15 to 30 minutes per day. Instruction begins with the student typing R for reading, an identification number, and his first name. The program responds with the student's last name and

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[1] This article was an invited address presented at the meeting of the Southwestern Psychological Association, Dallas, Texas, April 1973. The preparation of the article was supported in part by National Science Foundation Grant NSF-GJ-443X3 and in part by Office of Naval Research Contract N00014-67-A-0012-0054.

Requests for reprints should be sent to R. C. Atkinson, Department of Psychology, Stanford University, Stanford, California 94305.

[2] See Atkinson (1972), Atkinson and Paulson (1972), and Groen and Atkinson (1966).

[3] For a review of this work, see Atkinson (1968a, 1968b, 1969).

Figure 1. Student running on the CAI reading program. (The terminal consists of a Model-33 teletypewriter and earphones with an audio amplifier. The program operates on a PDP-10 computer located at Stanford University and is connected remotely to terminals in the schools by multiplexed telephone lines. Although the terminal has no graphic capability, it is a sturdy, low-cost device that provides the student with a printed copy of his interaction with the instructional program.)

automatically transfers him to the point in the curriculum where he finished on the previous day.

Reading instruction can be divided into two basic tasks which have been referred to as *decoding* and *communication*. Decoding is the rapid, if not automatic, association of phonemes or phoneme groups with their respective graphic representations. Communication involves reading for meaning, aesthetic enjoyment, emphasis, and the like. Our CAI program provides instruction in both types of tasks, but focuses primarily on decoding. The program is divided into eight parts or strands. As indicated in Figure 2, entry into a strand is determined by the student's level of achievement in the other strands. Instruction begins in Strand O, which teaches the skills required to interact with the program. Entry into the other strands is dependent on the student's performance in earlier strands. For example, the letter identification strand starts with a subset of letters used in the earliest sight words. When a student reaches a point in the letter identification strand where he has exhibited mastery over the letters used in the first words of the sight-word strand, he enters that strand. Similarly, entry into the spelling pattern strand and the phonics strand is controlled by the student's placement in the sight-word strand. On any given day a student may be seeing exercises drawn from as many as five strands. The dotted vertical lines in Figure 2 represent *maximal rate contours*, which control the student's progress in each strand relative to his progress in other strands. The rationale underlying these contours is that learning particular material in one strand facilitates learning in another strand; thus, the contours are constructed so that the student learns specific items from one strand in conjunction with specific items from other strands. In general, a student receives an amount of time in each strand proportional to the number of items yet to be completed in that strand before he reaches the next contour.

The CAI program is highly individualized so that a trace through the curriculum is unique for each student. The problem confronting the psychologist is to specify how a given subject's response history should be used to make instructional decisions. The approach that we have adopted is to develop simple mathematical models for the acquisition of the various skills in the cur-

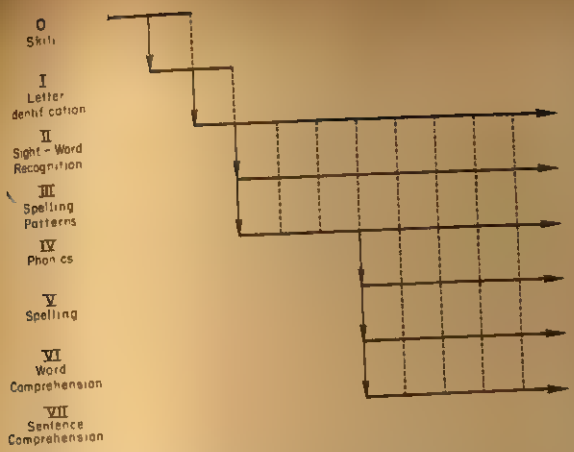

Figure 2. Schematic presentation of the strand structure. (Entry into each strand depends on a student's performance in earlier strands. The vertical dotted lines represent maximal rate contours which control the student's progress in each strand relative to the other strands.)

riculum and then use these models to specify optimal sequencing schemes. Basically, this problem is what has come to be known in the mathematical and engineering literature as *optimal control theory* or, more simply, *control theory*. The development of control theory has progressed at a phenomenal rate in the last decade, but most of the applications involve engineering or economic systems of one type or another. Precisely the same problems are posed in the area of instruction, except that the system to be controlled is the human learner rather than a machine or group of industries. If a model for the acquisition of a skill can be specified, then methods of control theory can be used to derive optimal instructional strategies.

I want to review some of the optimization procedures that have been developed, but in order for the reader to have some idea of how the CAI program operates, let me first describe a few of the simpler exercises used in Strands II, III, and IV.[4] Strand II provides for the development of a sight-word vocabulary. Vocabulary items are presented in five exercise formats, but only the copy exercise and the recognition exercise will be described here. The top panel of Table 1 illustrates the copy exercise, and the lower panel illustrates the recognition exercise. Note in Table 1 that when a student makes an error, the system responds with an audio message and prints out the correct response. In earlier versions of the program, the student was required to copy the correct response following an error. Experiments, however, demonstrated that the overt correction procedure was not effective and slowed down the pace of instruction; simply displaying the correct word following an error seems to be maximally effective.

Strand III provides practice with spelling patterns and emphasizes the regular grapheme–phoneme correspondences that exist in English. Table 2 illustrates exercises from this strand. For the exercise in the top panel of Table 2, the student is presented with three words involving the same spelling pattern and is required to select the correct one based on its initial letters. Once the student has learned to use the initial letter or letter sequence to distinguish between words, he then moves to the recall exercise illustrated in the bottom panel of Table 2. Here he works with a group of words, all involving the same spelling pattern. On each trial the audio system requests a word that requires adding an initial consonant or consonant cluster to the spelling pattern mastered in the preceding exercise. Whenever a student makes a correct response, a "+" sign is

TABLE 1

Examples of Two Exercises Used in Strand II (Sight-Word Recognition)

	Teletypewriter display	Audio message
Copy exercise		
The program outputs:	PEN	(Type pen.)
The student responds by typing:	PEN	
The program outputs:	+	(Great!)
The program outputs:	EGG	(Type egg.)
The student responds by typing:	EFF	
The program outputs:	////EGG	(No, egg.)
Recognition exercise		
The program outputs:	PEN NET EGG	(Type pen.)
The student responds by typing:	PEN	
The program outputs:	+	
The program outputs:	PEN EGG NET	(Type net.)
The student responds by typing:	NET	
The program outputs:	+	(Fabulous!)

Note. The top panel displays the copy exercise and the bottom panel the recognition exercise. Rows in the table correspond to successive lines on the teletypewriter printout.

[4] A detailed account of the curriculum can be found in Atkinson, Fletcher, Lindsay, Campbell, and Barr (1973).

TABLE 2

Examples of the Recognition and Recall Exercises Used in Strand III (Spelling Patterns)

	Teletypewriter display	Audio message
Recognition exercise		
The program outputs:	KEPT SLEPT CREPT	(Type kept.)
The student responds by typing:	KEPT	
The program outputs:	+	
Recall exercise		
The program outputs:		(Type crept.)
The student responds by typing:	CREPT	
The program outputs:	+	(That's fabulous!)

printed on the teletypewriter. In addition, every so often the program will give an audio feedback message; these messages vary from simple ones like "great," "that's fabulous," "you're doing brilliantly," to some that have cheering, clapping, or bells ringing in the background. These messages are not generated at random, but depend on the student's performance on that particular day. If his performance is above that of the preceding three days, it will be so recognized with frequent audio messages.

When the student has mastered a specified number of words in the sight-word strand, he begins exercises in the phonics strand; this strand concentrates on initial and final consonants and consonant clusters in combination with medial vowels. As in most linguistically oriented curricula, students are not required to rehearse or identify consonant sounds in isolation. The emphasis is on patterns of vowels and consonants that bear regular correspondences to phonemes. The phonic strand is the most complicated one of the group and involves eight exercise formats; only two of the formats will be described here. The upper panel of Table 3 illustrates an exercise in which the student is required to identify the graphic representation of phonemes occurring at the end of words. Each trial begins with an audio presentation of a word that includes the phonemes, and the student is asked to identify the graphic representation. After mastering this exercise, he is then transferred to the exercise illustrated in the bottom panel of Table 3. The same phonemes are presented, but now the student is requested to construct words by adding appropriate consonants.

Optimizing the Instructional Process

This has been a brief overview of some of the exercises used in the curriculum. The key to the curriculum is in the optimization schemes that control the sequencing of these exercises; these schemes can be classified at three levels. One level involves decision making within each strand. The problem is to decide which items to present for study, which exercise formats to present them in, and when to schedule review. A complete response history exists for each student, and this history is used to make trial-by-trial decisions regarding what instruction to present next. The second level of optimization deals with decisions about allocation of instruction time among the various strands for a given student. At the end of an instructional session, the student will have reached a certain point in each strand and a decision must be made as to the distribution of time among the strands in the next session. The third level of optimization deals with the distribution of instructional

TABLE 3

Examples of Two Exercises from Strand IV (Phonics)

	Teletypewriter display	Audio message
Recognition exercise		
The program outputs:	-IN -IT -IG	(Type /IG/ as in fig.)
The student responds by typing:	IG	
The program outputs:	+	(Good!)
The program outputs:	-IT -IN -IG	(Type /IT/ as in fit.)
The student responds by typing:	IT	
The program outputs:	+	
Build-a-word exercise		
The program outputs:	-IN -IT -IG P--	(Type pin.)
The student responds by typing:	PIN	
The program outputs:	+	(Great!)
The program outputs:	-IG -IN -IT F--	(Type fig.)
The student responds by typing:	FIN	
The program outputs:	////FIG	(No, we wanted fig.)

time among students. The question here is to allocate computer time among students to achieve instructional objectives that are defined not for the individual student but for the class as a whole. In some global sense, these three levels of optimization should be integrated into a unified program. However, our understanding of these matters is still very incomplete, and we have been satisfied to work with each in isolation, hoping that later they can be incorporated into a single package.

I want to describe some aspects of optimization on the first level and then go into detail on an example from the third level. Problems at the second level were touched on earlier when I described the strand structure of the curriculum and the use of maximum-rate contours to allocate time among strands. In some respects, this optimization program is the most interesting of the group, but it cannot be explained without going into considerable detail.[5]

Optimization within a strand, what has been called Level 1, can be illustrated using the sight-word strand. The strand comprises a list of about 1,000 words; the words are ordered in terms of their frequency in the student's vocabulary, and words at the beginning of the list tend to have highly regular grapheme–phoneme correspondences. At any point in time, a student will be working on a limited pool of words from the list; the size of this working pool depends on the student's ability level and is usually between 5 and 10 words. When one of these words is mastered, it is deleted from the pool and replaced by the next word on the list or by a word due for review. Figure 3 presents a flow chart for the strand. Each word in the working pool is in one of five possible instructional states. A trial involves sampling a word from the working pool and presenting it in an appropriate exercise format. The student is pretested on a word the first few times it is presented in order to eliminate words already known. If he knows the word, he will pass criterion for the pretest and it will be dropped from the working pool. If the student does not pass the pretest, he first studies the word in a series of trials, using the copy exercise, and then in a series of trials using the recognition exercise. If review is required, he studies the word again in what is designated in Figure 3 as Exercises 4 and 5.

[5] See Chant and Atkinson (1973) for a discussion of the problem and applications.

As indicated in the figure, a given word passes from one state to the next when it reaches criterion. And this presents the crux of the optimization problem, which is to define an appropriate criterion for each exercise. This has been done using simple mathematical models to describe the acquisition process for each exercise and the transfer functions that hold between exercises.[6] Basically, these models are simple Markov processes that have been extensively investigated by learning theorists and are known to provide reasonably accurate accounts of performance on our tasks. Parameters of the models are defined as functions of two factors: (*a*) the ability of the particular student and (*b*) the difficulty of the particular word. An estimate of the student's ability is obtained by analyzing his response record on all previous words, and an estimate of a word's difficulty is obtained by analyzing performance on that particular word for all students ever run on the program. The student records are continually updated by the computer and are used whenever necessary to compute a maximum likelihood estimate of each student's ability factor and each word's difficulty factor.[7] Given a well-defined model and estimates of its parameters, we can use the methods of control theory to define an optimal criterion for each exercise. The criterion will vary depending on the difficulty of the item, the student's ability level, and the precise sequence of correct and incorrect responses made by the student to the item. It is important to realize that the optimization scheme is not a simple one-stage branching program based on the last response, but rather depends, in a complicated way, on the student's complete response history.

Optimizing Class Performance

Now to turn to an example of optimization at what has been called Level 3. The effectiveness of the CAI program can be increased by optimally allocating instructional time among students. Suppose, for example, that a school has budgeted a fixed amount of time for CAI and must decide how to allocate that time among a class of first-grade students. For this example, maximizing the effec-

[6] For a discussion of the learning models, see Atkinson and Paulson (1972); for a discussion of the transfer models, see Dear and Atkinson (1962).

[7] See Atkinson and Paulson (1972) and Laubsch (1970) for a discussion of these methods.

tiveness of the CAI program is interpreted as meaning that we want to maximize student performance on a standardized reading test administered at the end of the first grade. Although other dependent measures can be used, this one provides a convenient benchmark against which to judge effectiveness.

On the basis of experimental data, the following equation has been derived that predicts performance on a standardized reading test as a function of the amount of time that a student spends on the CAI system:

$$P_i(t) = \alpha_i - (\alpha_i - \beta_i)e^{-\gamma_i t}.$$

The equation predicts Student i's performance on a standardized test as a function of the time, t, spent on the CAI system during the school year. The more time spent on the CAI program, the higher the level of achievement. The parameters α_i, β_i, and γ_i characterize Student i, and vary from one student to another. The parameters α_i and β_i are measures of Student i's maximal and minimal levels of achievement, respectively, and γ_i is a rate of progress measure. These parameters can be estimated from scores on reading readiness tests and from the student's performance during his first hour of CAI. After estimates of these parameters have been made, the above equation can be used to predict end-of-the-year test scores as a function of the CAI time allocated to that student.

To summarize, it is assumed that a school has budgeted a fixed amount of time T on the CAI system for a first-grade class of N students; further, students have had reading readiness tests and a preliminary run on the CAI system so that estimates of the parameters α, β, and γ can be made for each student. The problem then is to allocate time T among the N students so as to optimize learning. In order to do this, it is first necessary to have a model of the learning process. Although the above equation does not offer a very detailed account of learning, it suffices as a model for purposes of this problem, giving all the information that is required. This is an important point to keep in mind. The nature of the specific optimization problem determines the level of complexity that needs to be represented in the learning model. For some optimization problems, the model must provide a relatively complete account of learning in order to specify a viable strategy, but for other problems a simple descriptive equation often will suffice.

In addition to a model of the learning process, we must also specify our instructional objective. There are several objectives that seem reasonable, but only the following will be considered here:

A. Maximize the mean value of P over the class of students.

B. Minimize the variance of P over the class of students.

C. Maximize the mean value of P under the constraint that the resulting variance of P is less than or equal to the variance that would be obtained if no CAI were administered.

Objective A maximizes the gain for the class as a whole; Objective B reduces differences among students by making the class as homogeneous as possible; and Objective C attempts to maximize performance of the whole class, while insuring that differences among students are not amplified by CAI. To start, we will select Objective A as the instructional objective. If t_i is the time allocated to Student i, then the problem of deriving an optimal strategy reduces to maximizing the function

$$\phi(t_1, t_2, \cdots, t_N) = \frac{1}{N}\sum_{i=1}^{N}\left[\alpha_i - (\alpha_i - \beta_i)e^{-\gamma_i t_i}\right]$$

subject to the constraint that $t_1 + t_2 + \ldots + t_N = T$. This maximization can be done using the methods of dynamic programming. To illustrate the approach, computations were made for a first-grade class for which the parameters α, β, and γ had been estimated for each student. Employing these estimates, computations were carried out to determine the time allocations that maximized the above equation. For the optimal policy, the predicted mean performance level of the class on the end-of-the-year tests was 14% higher than a policy that allocated time equally among the students (i.e., a policy where $t_i = T/N$ for all students). This gain represents a substantial improvement; the drawback is that the class variance is roughly 15% greater than the variance for the class using an equal time policy. This means that if we are only interested in raising the class average, we

Figure 3. Partial flow chart for Strand II (sight-word recognition). (The various decisions represented in the bottom part of the chart are based on fairly complicated computations that make use of the student's response history.) ⟶

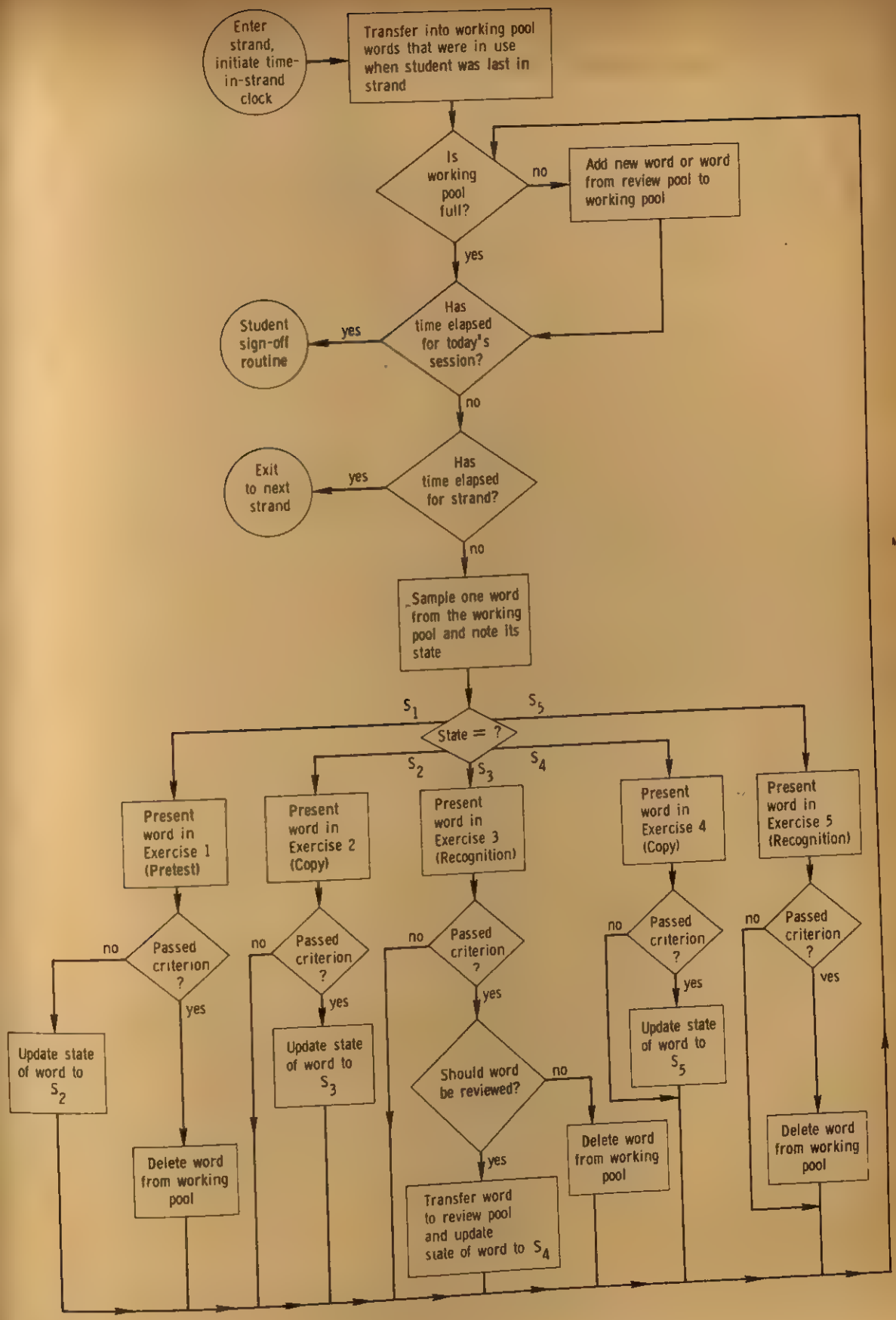
Enter strand, initiate time-in-strand clock
Transfer into working pool words that were in use when student was last in strand
Is working pool full?
no
Add new word or word from review pool to working pool
yes
Has time elapsed for today's session?
yes
Student sign-off routine
no
Has time elapsed for strand?
yes
Exit to next strand
no
Sample one word from the working pool and note its state
State = ?
S_1
S_2
S_3
S_4
S_5
Present word in Exercise 1 (Pretest)
Present word in Exercise 2 (Copy)
Present word in Exercise 3 (Recognition)
Present word in Exercise 4 (Copy)
Present word in Exercise 5 (Recognition)
Passed criterion ?
no
yes
Update state of word to S_2
Delete word from working pool
Update state of word to S_3
Should word be reviewed?
Transfer word to review pool and update state of word to S_4
Delete word from working pool
Update state of word to S_5
Delete word from working pool

TABLE 4

Percent Gain in the Mean of P *and in the Variance of* P *When Compared with an Equal-Time Policy*

	Instructional objective		
	A	B	C
% gain in mean of *P*	14	−15	8
% gain in variance of *P*	15	−12	−6

Note. *P* is an end-of-the-year performance score on a standardized reading test. In general, a policy that leads to a positive gain in the mean and a negative gain in variance is preferable.

will have to give the rapid learners substantial time on the CAI system and let them progress far beyond the slow learners.

Although a time allocation that complies with Objective A did increase overall class performance, the correlated increase in variance suggests that other objectives need to be considered. For comparison, time allocations also were computed for Objectives B and C. Table 4 presents the predicted gain in average class performance as a percentage of the mean value for the equal time policy. Objective B yielded a negative gain, and so it should, since its goal was to reduce variability, which is accomplished by holding back on rapid learners and giving a lot of attention to the slower ones. The reduction in variability for Objective B is 12%. Objective C, which strikes a balance between Objective A, on the one hand, and Objective B, on the other, yields an 8% gain in average performance and yet reduces variability by 6%.

In view of these results, Objective C seems to be the preferred one. It offers a substantial increase in average class performance while maintaining a low level of variability. These computations make it clear that the selection of an instructional objective should not be done in isolation but should involve a comparative analysis of several objectives, taking into account more than one dimension of performance. Even if the principal goal is to maximize the class average, it is inappropriate in most educational situations to select Objective A over C if it is only slightly better for the class average, while permitting variability to mushroom.

Effectiveness and Costs

Next, I want to make a few remarks about evaluation studies and the costs of the CAI program. Several evaluation studies have been conducted in the last few years, and yet another now is being done by the Educational Testing Service. Rather than merely review these studies, I would like to describe one in some detail.[8] In this particular study, 50 pairs of kindergarten students were matched on a number of variables, including sex and reading readiness scores. At the start of the first grade, one member of each pair was assigned to the experimental group and the other to the control group. Students in the experimental group received CAI, but only during the first grade; students in the control group received no CAI. The CAI lasted approximately 15 minutes per day; during this period the control group studied reading in the classroom. Except for this 15-minute period, the school day for the CAI group was like that of the control group. Standardized tests were administered at the end of the first grade and again at the end of the second grade. All the tests showed roughly the same pattern of results; to summarize the findings, only data from the California Cooperative Primary Reading Test will be described. At the end of the first grade, the experimental group showed a 5.05-month gain over the control group. The groups, when tested a year later (with no intervening CAI treatment), showed a difference of 4.90 months. Thus, the initial difference observed following one year of CAI was maintained, although not amplified, during the second year when no CAI was administered.

An interesting aspect of these results is that the boys appeared to benefit more from CAI than the girls. On all reading tests used in the evaluation, the girls as a group were superior to the boys. However, for the control group, the magnitude of the difference between boys and girls was greater than for the experimental group. For example, on the California Cooperative Primary Reading Test, the relative improvement for boys in the experimental group versus those in the control group was 42%; the corresponding figure for girls was 17%. These data suggest that both boys and girls benefit from CAI instruction, but that the gain is greater for boys. This observation is not unique to this study, but replicates a result from one of our earlier studies.[9] It is a common finding in

[8] For a detailed account of this study, see Fletcher and Atkinson (1972).

[9] See Atkinson (1968a) for a review of earlier studies.

research on reading that girls perform better than boys. This sex difference has been attributed at least in part to the social organization of the classroom and the predominance of female teachers. It is also argued that first-grade girls are more facile in memorization than boys and that this ability further aids the girls in curricula that emphasize sight-word procedures. If these two arguments are correct, then one would expect that placing the students in a CAI environment and using a curriculum that emphasizes analytic skills would reduce sex differences in reading. Our data tend to support this line of argument.

The results obtained in this and other studies can be used to project performance through the third grade. I will not explain the formula used to make the projection but will simply state the conclusion. For the population of students with which we have worked, the average reading level at the end of the third grade is approximately 2.9 when CAI is not used. For students who receive CAI, the grade level is 4.1. These values are to be compared with a national norm of 4.0. Thus, students with CAI are slightly above grade level by the end of the third grade, while those without CAI are about one year behind.

Can CAI be cost effective? The cost of daily sessions on our system is about $.55 per student. Based on a school year of 176 days, the yearly cost is roughly $97.00 per student. If this is multiplied by three, we have a figure of $291.00, a cost that places students at grade level by the end of the third grade who would normally be over a year behind. There is no doubt that such a cost is acceptable if future evaluations are as promising as they have been to date.[10]

Conclusion

In my view, individualizing instruction is the key factor in successfully teaching reading. This does not mean that all phases of instruction should be individualized, but often certain skills can be mastered only if instruction is sensitive to the student's particular difficulties. A teacher interacting on a one-to-one basis with a student may well be more effective than a CAI program. However, when working with a group of children (even as few as four or five), it is unlikely that she can match the computer's effectiveness over an extended period of time.

The possibilities for developing optimal instructional procedures are, of course, most promising at the elementary school level. In areas like initial reading and primary grade mathematics, we have an adequate understanding of many of the psychological processes involved. Simple models can be formulated to describe these processes, and in turn be used to derive optimal procedures. However, we know very little about the cognitive processes that underlie mastering a college-level curriculum in fields such as sociology or philosophy. In these cases, we cannot formulate models that describe learning, and thus cannot use the methods of optimization discussed here. However, more can be done at the college level than one might expect based on current work. Certainly, models can be developed for some aspects of learning in the natural sciences and second-language acquisition, even if these models are little more than descriptive equations. Research needs to be done to determine the feasibility of the approach, and much can be learned by experimenting with alternative optimization schemes, however loosely they may be related to formal models. Finding optimization schemes that work can tell us about the nature of the learning process and provide direction for theoretical analysis. A two-way exchange between the formulation of optimization procedures and development of descriptive models has not played as significant a role in psychological research as it merits.

In this discussion, I have tried to indicate some of the issues that arise in constructing a CAI program. From a broader perspective, the problem that we face in developing curricula is twofold. One aspect of the problem is to invent effective exercises for teaching particular skills and concepts; the other is to devise schemes for sequencing among these exercises. The problem of inventing and evaluating instructional exercises is an old one in psychology. There is no question that we know how to carry out the kinds of experiments needed for establishing the effectiveness of an instructional procedure. But the problem of formulating a scheme for sequencing among instructional procedures that is sensitive to the student's current state of knowledge is another matter, and one that has received very little investigation. Before the advent of the computer there was no real flexibility in manipulating the flow of instruction in a school situation; therefore, whether

[10] For a more detailed discussion of cost effectiveness, see Jamison, Fletcher, Suppes, and Atkinson (1974).

or not we understood how to individualize learning was of limited consequence. Now, with the computer, a new dimension of school learning has emerged. It is my belief that psychology's potential contributions in this area are of great practical significance. The development of a viable theory of instruction may be the most important issue facing psychology, and one that can revolutionize our conceptions of how man thinks and learns.

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Psychological Environments

Expanding the Scope of Human Ecology

PAUL M. INSEL *Stanford University*
RUDOLF H. MOOS *Stanford University*[1]

Like people, environments have unique personalities. Just as it is possible to characterize a person's "personality," environments can be similarly portrayed with a great deal of accuracy and detail. Some people are supportive; likewise, some environments are supportive. Some men feel the need to control others; similarly, some environments are extremely controlling. Order and structure are important to many people; correspondingly, many environments emphasize regularity, system, and order.

Henry Murray (1938) first conceptualized the dual process of personal needs and environmental press. He suggested that individuals have specific needs, the strength of which characterizes "personality." The environment potentially satisfies or frustrates these needs. Murray's model for studying behavior thus consisted of the interaction between personality needs and environmental press. Murray's concept of needs provided a point of entry for the development of a variety of measurement instruments to study personality; however, no parallel development in the objective measurement of environmental press was attempted until much later.

Stern, Stein, and Bloom (1956) expanded Murray's contribution. They demonstrated that behavior could be predicted much better when the setting in which the behavior occurred was clearly defined so as to include the social demands of the situation. Pace and Stern (1958) developed the concept of environmental press further by applying the logic of "perceived climate" to the study of "atmosphere" at universities and colleges. They constructed the College Characteristics Index (CCI) which measured the global college environment by asking students to act as reporters. Specifically, students were asked to answer true or false to items covering a wide range of topics about their college, such as student–faculty relationship, rules and regulations, classroom methods, facilities, etc. The general logic of this approach suggests that the consensus of students' characterizing their college environment constitutes a measure of environmental climate, and that this environmental climate exerts a directional influence on their behavior.

Thus, one might infer a general principle to the effect that the way one perceives his surroundings or environment influences the way one will behave in that environment. While this principle has a commonsense ring to it, it is not usually applied in a practical way to the routine problems and tasks with which psychologists deal. For example, personality and projective tests are frequently administered and interpreted with the assumption that results will portray permanent and enduring qualities that transcend the environment, providing information that can accurately predict behavior regardless of the setting in which the behavior is likely to take place. This assumption is pervasive in spite of much evidence showing that properties of the environment may account for more of the variance in behavior than measures of trait qualities or even biographic and demographic background data (Douglas, 1964; Mischel, 1968; Wolfe, 1966). For example, Friedlander and Greenberg (1971) studied the job performance and retention of 478 hard-core unemployed workers and found that the sole correlate of their work effectiveness and ability to retain jobs was the extent to which they perceived their work environment to be supportive. Personality and background data were found to be unrelated to work effectiveness and job retention.

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Requests for reprints should be sent to Paul M. Insel, Social Ecology Laboratory, Department of Psychiatry, Stanford University, Stanford, California 94305. Moos, also, is with the Social Ecology Laboratory, Department of Psychiatry.

Classifying Human Environments

The concept of environment has historically been somewhat ambiguous and amorphous. Formal and systematic study of environments is rooted in the biological sciences where the term *ecology* is most commonly applied to the natural habitats of animals. *Human ecology* is a more recent term extending to the domain of geographers and sociologists who are interested in the distributions of human populations. The term *social ecology* has evolved mainly from the efforts of psychologists and other behavioral scientists to direct their inquiries toward a more complete view of man interacting with both his physical and social environment (Moos & Insel, 1974).

The seeds of social ecology can be found in both ecology and human ecology. Because ecological phenomena may be considered as existing over the entire range of organisms and at a number of different levels of organization, the field of ecology has become fractionated and specialized.

Human ecology departs from animal and plant ecology in the nature of its regulatory mechanisms. Among the special regulatory mechanisms which operate in a human society are public opinion, punishment, rewards, competition, and supply and demand. Social cooperation is perhaps the most important regulatory mechanism in society, serving to mitigate the destructive and predatory elements found in the ecological evolution of other animal communities. Knowledge about the operation of the regulatory mechanisms which keep human societies in balance with the resources of their milieus is one of the concerns of social ecology.

The emerging discipline of social ecology grows out of this interaction of man with his environment. To this extent it transcends human ecology. It reflects the traditional concerns of ecology both in its emphasis on the measurement of objective physical characteristics of environments (e.g., temperatures, rainfall, air pollution, noise levels; the shapes, sizes, and physical arrangements of buildings) and in its inquiry into the short-term evolutionary and adaptive consequences of these environments. Social ecology, however, expands these concerns by systematically dealing with the social environment and its interaction with the physical milieu. And, unlike ecology and human ecology, social ecology has an explicit value orientation in that it is concerned with promoting maximally effective human functioning. Finally, social ecology touches the main currents of scientific thought in psychiatry, medicine, and epidemiology in its special emphasis on the identification of maladaptive responses and their relationship to environmental variables.

Moos (1973a) suggested six different ways of conceptualizing human environments:

1. *Ecological dimensions.* These include (*a*) meteorological and geographical variables (this view of the environment suggests that society has been shaped by climate, topography, and other geographical features of inhabited regions), and (*b*) physical design variables. Here man's behavior is influenced by architecture and physical constraints which limit or even define the range of activities in which man can be involved.

2. *Dimensions of organization structure.* This view of the environment suggests that behavior is influenced by structural dimensions such as size, staffing ratios, salary levels, span of organizational control, etc.

3. *Personal characteristics of milieu inhabitants.* This view implies that the character of an environment depends on the nature of its members and that the dominant features of an environment depend on the typical characteristics of its members, such as age, sex, socioeconomic status, abilities, group memberships, physique, and other background data.

4. *Behavior settings.* This view of the environment originated with Roger Barker (1968), who emphasized the importance of studying behavior settings as natural phenomena. Behavior settings are conceptualized as ecological units which have both an environmental and a behavioral component.

5. *Functional or reinforcement properties of environments.* This view of the environment suggests that people vary their behavior substantially from one setting to another as a function of the reinforcement consequences for particular behaviors.

6. *Psychosocial characteristics and organizational climate.* This conceptualization encompasses both psychological and social dimensions of the environment in a framework of person–milieu interaction. The approach accommodates both an inside perception of what the environment is like as well as an outside observer's impression, although most of the work to date emphasizes the importance of climate as perceived by participating members of the environment.

Measuring Environments

As suggested earlier, a limited literature exists on the development of systematic approaches to measuring environments. This is partly because

"environments" are unwieldy and thus difficult to deal with as objects of investigation. This problem has been partially resolved by such investigators as Barker (1968), who focused on specific environmental units which have both a space and a time locus, and Moos (1969), who focused on subenvironments or subunits in which milieu occupants interact with each other on some regular and familiar basis.

Moos and his associates at the Social Ecology Laboratory at Stanford University have made substantial contributions toward developing an in-depth program of characterizing and assessing the psychosocial qualities of environments. They have extensively studied eight different environments and have developed perceived climate scales for each environment: (*a*) psychiatric wards; (*b*) community-oriented psychiatric treatment programs; (*c*) correctional institutions; (*d*) military basic training companies; (*e*) university student residences; (*f*) junior and senior high school classrooms; (*g*) work environments; (*h*) social, therapeutic, and decision-making groups.

Underlying Patterns of Differing Environments

Common dimensions have emerged from studies of the eight different kinds of environments. These have been conceptualized by Moos (1974d) in three broad categories: relationship dimensions, personal development or goal orientation dimensions, and system maintenance and change dimensions. These dimensions are similar across the eight environments mentioned although vastly different settings may impose unique variations within the general categories.

RELATIONSHIP DIMENSIONS

Relationship dimensions identify the nature and intensity of personal relationships within the environment. They assess the extent to which individuals are involved in the environment and the extent to which they support and help each other. As can be seen in Table 1, examples of relevant subscales in the eight climate scales are involvement, affiliation, staff support, peer cohesion, and spontaneity.

Personal development dimensions consider the potential or opportunity in the environment for personal growth and the development of self-esteem. The precise nature of personal development dimensions varies somewhat among different environments and depends mainly on the goals of a particular environment. Examples of relevant subscales are autonomy, practical orientation, competition, and intellectuality.

System maintenance and system change dimensions assess the extent to which the environment is orderly and clear in its expectations, maintains control, and is responsive to change. Examples of the subscales one finds here are order and organization, clarity, control, and innovation. The three "basic" categories can best be seen as they fit into the framework of the four environments shown in Table 1.

The eight climate scales mentioned earlier can be classified into one of four types of environments: (*a*) treatment environments, (*b*) total institutions,[2] (*c*) educational environments, and (*d*) community environments. Treatment environment measures are the Ward Atmosphere Scale (Moos, 1974d) and the Community-Oriented Programs Environment Scale (Moos, 1974a). Total institutions environment measures are the Correctional Institutions Environment Scale (Moos, 1974b) and the Military Company Environment Inventory (Moos, 1973b). Educational environment measures are the University Residence Environment Scale (Moos & Gerst, 1974) and the Classroom Environment Scale (Moos & Trickett, 1974). Community environment measures include the Work Environment Scale (Insel & Moos, 1972) and the Group Environment Scale (Moos & Humphrey, 1973). A final technique, the Family Environment Scale, is being developed.

To give an example of the similarity of differing environments, let us compare the social environment of a psychiatric ward with the social environment of a factory. On the surface these two environments appear rather remote from each other. They have much in common, however. The instruments used to assess these two settings were the Ward Atmosphere Scale (WAS) and the Work Environment Scale (WES). The relevant relationship dimensions on the WAS are involvement, sup-

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[2] Goffman (1961) suggested that "total institutions" take over the life processes of persons who live within their physical constraints. They differ from other environments in that they require large groups of unselected members to conduct their lives in a similar fashion and on a fixed schedule. These developing life-styles are purportedly designed to fulfill the aims of the institutions. Total institutions have two distinct groups of inhabitants: staff and inmates. These two groups interact on a restricted, often formally prescribed, basis. Goffman suggested that two different social and cultural worlds develop, which move alongside one another but have minimal contact.

TABLE 1

Similarities of Social Climate Dimensions across Environments

Type of dimension	Treatment environment		Total environment		Educational environment		Community environment	
	Ward Atmosphere Scale	Community-Oriented Programs Environment Scale	Correctional Institutions Environment Scale	Military Company Environment Inventory	University Residence Environment Scale	Classroom Environment Inventory	Work Environment Inventory	Group Environment Inventory
Relationship								
Involvement-affiliation	X	X	X	X	X	XX	X	
Spontaneity-expressiveness	X	X	X					X
Support	X	X	X	X	X	X	X	XX
Cohesiveness				X			X	X
Personal development								
Autonomy-independence (personal status)	X	X	X	X	X		X	X
Practical orientation (task orientation, academic achievement)	X	X	X		X	X	X	X
Personal problem orientation (self-discovery)	X	X	X					X
Anger and aggression	X	X						X
Competition					X	X		
Intellectuality					X			
Traditional social orientation					X			
System maintenance and system change								
Order and organization	X	X	X	X	X	X		X
Clarity	X	X	X	X		X	X	X
Control	X	X	X	X		X	X	X
Physical comfort							X	
Work pressure							X	
Innovation-student influence					XX	X	X	X

port, and spontaneity. Relationship dimensions on the WES are involvement, staff support, and peer cohesion. Program involvement on a psychiatric ward refers to how active and energetic patients are in the day-to-day functioning of the ward. Involvement in a factory setting refers to the extent to which workers are concerned and committed to their jobs. Support on a psychiatric ward indicates the extent to which patients are encouraged to be helpful and supportive toward other patients and how supportive the staff is toward patients. Staff support in a factory indicates the extent to which management is supportive of workers and encourages workers to be supportive of each other. The subscales of involvement and support are roughly equivalent in both settings. Spontaneity, however, is more relevant to a treatment or therapeutic environment in which people are often encouraged to act openly and to freely express their feelings. This aspect of the climate in a factory setting can be seen more appropriately as part of both the staff support and peer cohesion components. On a psychiatric ward, staff support and peer support are one dimension since they correlate so highly with one another. But in a factory setting they are independent.

As mentioned earlier, some environments possess more than one aspect of the same property. For example, work environments have two distinctly different support elements. One element is called *peer cohesion* and accounts for the social and interpersonal relationships that develop among workers and their tendency to stick together and help each other. The second element, called *staff support*, accounts for the degree of friendship and communication between management and nonmanagement

personnel and the extent to which management encourages and helps nonmanagement personnel. These two elements identify a distinction between peer support and supervisor or staff support. In psychiatric and correctional environments, peer support tends to merge with staff support. In fact, it is difficult to find programs where these two support variables are not correlated positively. However, in a work environment, nonmanagement personnel frequently spend a great deal of time together maintaining a separate factor of cohesiveness.

PERSONAL DEVELOPMENT

The second category is personal development. Both settings have two subscales in common, namely, autonomy and practical or task orientation. Autonomy on a psychiatric ward involves how independent and self-sufficient patients are encouraged to be in making their own decisions about their personal affairs. For example, can a patient wear what he wants? Can he leave the ward without permission? In a factory setting, the issues are similar in that they are related to personal growth and independence. For example, are employees encouraged to learn more than one job? Can employees use their own initiative to do things?

The second subscale on a psychiatric ward is practical orientation. This component looks at the extent to which the patient's environment orients him toward preparing himself for release from the hospital. Such things as training for new jobs and setting and working toward goals are considered. In a factory the component of task orientation accounts for the extent to which the environment emphasizes good planning and efficiency and encourages workers to "get the job done."

SYSTEM MAINTENANCE

The third category for comparison is system maintenance, in which there are two identical subscales for both psychiatric ward and factory environments. These subscales are clarity and control. Clarity accounts for the extent to which both patients and workers know what to expect in their daily routines and how explicitly rules and policies are communicated. For example, do patients know when doctors will be on the ward? If a patient's medicine is changed, does a nurse or doctor tell him why? In a factory, do employees know when supervisors will be available? Do employees know who to see when a problem arises? The second component, control, refers to the extent to which staff or supervisors use measures to keep patients or workers under control. On a psychiatric ward, can patients call nursing staff by their first names? In a factory, can employees be absent from work without an authorized or written explanation?

The foregoing hopefully conveys the similar threads that make up the fabric of remotely related environments. The importance of these threads can be seen more clearly in a situation in which a patient is discharged from a psychiatric ward and gains employment in a factory setting. Perhaps diagnostic work-ups could benefit from this type of "total milieu" approach. For example, instead of administering the MMPI or Rorschach and trying to predict from these tests what the most suitable treatment might be for the patient, the psychologist might ask the patient to respond to the ideal form of the Ward Atmosphere Scale. If the patient indicates his need for more support or more order on the WAS, the psychologist would then be in an advantageous position in recommending a ward that had a climate that emphasized support and order. The same approach might be used in a work setting if the climate of the work environment was known.

The three basic dimensions (relationship, personal development, and system maintenance) identified in treatment environments are also found in total institutions and educational and community environments. However, some environments have a unique component within a particular category which is peculiar to them. For example, work pressure is specifically and solely relevant to work environments where things like time pressure, deadlines, strain, urgency, and speed may dominate the job milieu. Another example, falling within the personal development category, is "traditional social orientation," a subscale on the University Residence Environment Scale (URES). This variable accounts for the emphasis on dating, going to parties, and other traditional heterosexual interactions that one finds in student housing programs.

The eight social climate scales discussed earlier were all developed at the Social Ecology Laboratory at Stanford University. Can relationship, personal development, and system maintenance and system change variables be identified in other organizational climate scales? The results of eight different investigators are utilized and summarized in Table 2.

TABLE 2

Dimensions of Organizational Climate Scales

Scale	Relationship	Personal development	System maintenance and system change
Organizational Climate Index (Stern, 1970)	Closeness, group life	Intellectual climate, personal dignity, achievement standards	Orderliness, impulse control (constraint)
College and University Environment Scale (Pace, 1969)	Community	Awareness, scholarship	Practicality, propriety
Institutional Functioning Inventory (Peterson, 1970)	Institutional esprit	Intellectual-aesthetic, extracurriculum, concern for improvement of society, concern for undergraduate learning concern for advancing knowledge, meeting local needs	Freedom, democratic governance, self-study and planning, concern for innovation, human diversity
Learning Environment Inventory (Walberg, 1969)	Intimacy, friction, cliqueness, apathy, favoritism	Difficulty, speed	Formality, goal direction, democratic, disorganization, diversity
Organizational Climate Description Questionnaire (Halpin & Croft, 1963)	Esprit, intimacy, consideration, disengagement	Thrust, hindrance	Production emphasis, aloofness
Agency Climate Questionnaire (Schneider & Bartlett, 1970)	Managerial support, intraagency conflict, new employee concern	Agent independence	Managerial structure
Climate Questionnaire (Litwin & Stringer, 1968)	Warmth, support, conflict, identity	Responsibility, risk, standards, reward	Structure
Dimensions of Group Processes (Fairweather, 1969)	Group cohesiveness	Group performance	Leadership and role delineation

The basic logic and conceptualization appears to be consistent with the scales of other investigators. For example, the College and University Environment Scale (Pace, 1969) has five subscales: (*a*) *community* describes a friendly, cohesive group-oriented campus and is clearly a relationship dimension; (*b*) *awareness* describes a concern about personal, poetic, and political meaning, self-understanding and reflectiveness and is clearly a personal development dimension; (*c*) *scholar* describes an environment characterized by intellectuality, scholastic discipline, and academic achievement and is also clearly a personal development dimension; (*d*) *propriety* describes an environment that is polite, considerate, mannerly, proper, and conventional and where group standards of decorum are important. To the extent to which this variable emphasizes order and clarity within the environment it belongs in the category of system maintenance; (*e*) *practicality* describes an environment characterized by organization, enterprise, material benefits, and social activities. This is also a system maintenance dimension, since its essential aspect reflects orderly supervision and organization.

The second test reviewed is the Institutional Functioning Inventory (Peterson, Centra, Hartnett, & Linn, 1970). This instrument also lends itself to studying colleges and universities and pro-

vides 11 variables judged to be important in American higher education. Institutional esprit is clearly a relationship dimension. The following belong to the dimension of system maintenance and system change: freedom (lack of restraint on academic or personal life), democratic governance (extent of opportunity for participation in decision making), self-study and planning (emphasis on continuous long-range planning for the total institution), concern for innovation (commitment to experimentation with new ideas for educational practice), and human diversity (heterogeneity of faculty and student body in background and attitudes). The emphasis of these variables is on system change, which generally tends to be more strongly prevalent in most university environments than is system maintenance. Finally, the other five variables belong to the personal development dimension, that is, intellectual-aesthetic extracurricular interests (availability of activities and opportunities for intellectual and aesthetic stimulation outside the classroom), concern for improvement of society, concern for undergraduate learning, concern for advancing knowledge, and meeting local needs (emphasis on providing educational and cultural opportunities for adults in the surrounding area).

Table 2 indicates subscales for two other organizational climate scales relevant to educational environments, that is, the Learning Environment Inventory (Walberg, 1969) and the Organizational Climate Description Questionnaire (Halpin & Croft, 1963). The elements identified by these two scales can be conceptualized as falling within the three basic categories.

From results based on both educational and industrial environments, Stern (1970) identified two major types of second-order factor dimensions using the Organizational Climate Index (OCI). He indicated that the OCI factor structure essentially replicates his former scale, the CCI. Stern reported that the OCI has been factored three times. One analysis was based on the responses of teachers in elementary, junior high, and senior high schools; a second on Peace Corps trainees; and a third on technicians employed in three different industrial sites. Six factors were extracted in a first-order analysis and two in a second-order analysis. Stern (1970) summarized his results as follows:

> The first of the second-order factors describes a variety of press for facilitating growth and self-enhancement; the other reflects organizational stability and bureaucratic self-maintenance. These tend to confirm the hypothesized distinction drawn earlier between anabolic and catabolic press [p. 68].

Stern did not explicitly make a distinction between relationship and personal development dimensions. He also did not include system change dimensions in his category of control or system maintenance press. On the other hand, two variables which appear to be relationship dimensions were identified by Stern as closeness and group life. Three of Stern's variables appear to reflect personal development. He called these intellectual climate, personal dignity, and achievement standards. Stern's last two system maintenance factors are labeled orderliness and impulse control.

Stern's conceptualization is based on factor solutions and closely coincides with our conceptualization, strongly supporting the notion that there is a limited number of underlying patterns which can characterize a rather large and varied group of social environments.

Two more examples (Table 2) show similar conceptualizations. The Agency Climate Questionnaire (Schneider & Bartlett, 1970) and the Climate Questionnaire (Litwin & Stringer, 1968) were both constructed to assess industrial environments. The ACQ is primarily for insurance agencies and has subscales such as (*a*) managerial support (managers take an active interest in agents as individuals), (*b*) managerial structure (managers require that agents adhere strictly to budgets), and (*c*) agent independence (agents receive an accurate picture of job potential when they are contacted). Some of the Litwin and Stringer dimensions are (*a*) warmth (the feeling of general good fellowship that prevails in a work group atmosphere), (*b*) support (the perceived helpfulness of the managers and other employees in the group), (*c*) identity (the feeling that the employee belongs to the company and is a valuable member of a working team), (*d*) responsibility (the feeling of being one's own boss and not having to double check all of one's decisions), (*e*) risk (the sense of riskiness and challenge in the job and in the organization), and (*f*) structure (the feeling that the employees have about the constraints of the groups in terms of rules and regulations). Both scales are strikingly similar in basic dimensions to those identified by the Social Ecology Laboratory in very different environments.

Finally, an example is provided by an assessment of group processes conducted by Fairweather, Sanders, Cressler, and Maynard (1969) in which they compared a ward-based with a community-based psychiatric treatment program. They found three dimensions which characterized group processes and labeled these dimensions (*a*) group cohesiveness

(cohesiveness, morale, attraction to group, satisfaction with leader), (*b*) group performance (performance, reward, problem input, information input), (*c*) leadership and role delineation (leadership, role clarity).

One may conclude from the work in this area that relationship, personal development, and system maintenance and system change dimensions must all be accounted for in order for an adequate and reasonably complete picture of the environment to emerge.

Environmental Impact on Individual Functioning

The study and assessment of environments are important because of their relevance to individual functioning. The "climate" of environments in which people function relates to their satisfaction, mood, and self-esteem and to their personal growth. Environments shape adaptive potentials as well as facilitate or inhibit initiatives and coping behavior. For example, environments that place an emphasis on relationship dimensions such as involvement and support usually have high morale (Cumming & Cumming, 1962). It has been demonstrated that psychiatric wards and correctional units that emphasize autonomy and personal problem orientation have patients and residents who like the staff and feel they can develop their abilities and increase their self-confidence (Moos, 1974c).

Social environments also have significant impact on more objective criteria of behavioral outcome. For example, Moos, Shelton, and Petty (1973) related the social environment of psychiatric wards to objective indexes of treatment outcome as assessed by dropout rates (how many patients left the ward before treatment was completed), release rates (how rapidly were patients released from the ward), and community tenure (how long patients were able to stay in the community after release from the hospital). Two independent studies found that patients and staff perceived wards that had high dropout rates to be low in involvement, support, order and organization, and program clarity. Wards with high release rates were perceived as strongly emphasizing practical orientation, but inclined toward "unexpressiveness," whereas wards that kept patients out of the hospital longest were perceived as emphasizing autonomy, a practical orientation, order and organization, and the open expression of feelings, particularly angry feelings.

Similar results can be seen in the environment of military basic training companies. Moos (1973d) found that military company "climate" was related to important indexes of outcome, such as total performance on graded tests at the end of basic training, the AWOL (absent without official leave) rate, and the rate of sick call. Companies which did best on total performance criteria were those that emphasized both peer cohesion and officer support. Companies with excessive sick call lists contained men who felt that the work was repetitious and boring, that there was no opportunity for leadership roles and no orientation to the company, and that they were ridiculed in front of others. Company environments had effects on men's moods. For example, companies with high scores on officer control and low scores on personal status had men who felt more anxious. Companies high on officer control and low on peer cohesion had men who felt more depressed. Companies high on officer control and low on both officer support and clarity had men who felt more hostility. Thus, one finds specific relationships between dimensions of basic training company environments and types of negative effects.

Stressful environments have been shown to have cumulative long-term effects on those who function within them. Caffrey (1969) studied the environments of Benedictine and Trappist monks and found a prevalence of coronary heart disease in those environments characterized as competitive with a sense of time urgency. Sales (1969) suggested in his review that environments with work overload are implicated as precursors of cardiovascular disease.

In an extensive review of the literature, Kiritz and Moos (in press) concluded that the evidence supports the hypothesis that "social environmental factors have pronounced effects on human physiological processes." They suggested that

> the social stimuli associated with the relationship dimensions of *support, cohesion* and *affiliation* generally have positive effects—enhancing normal development and reducing recovery time from illness, for example. Goal Orientation and System Change dimensions such as *responsibility, work pressure,* and *change* can increase the likelihood of stress and disease.

Toward an Optimum Environment

What are the criteria by which environment can be judged as favorable? Lewis Mumford (1968) viewed an ideal environment as "seeking continuity, variety, orderly and purposeful growth" as opposed

to an environment that "magnifies authoritarian power and minimizes or destroys human initiative, self-direction, and self-government [p. 221]." Mumford suggested that optimum environments are organic and the qualities that make them desirable have to do with people, not "machines."

There are, of course, no clear, well-defined criteria for an ideal environment that can meet everyone's requirements. Inhabitants of specific environments would undoubtedly have different criteria and different goals. The point is, however, that the likelihood of achieving an optimum environment is greatly facilitated when critical decisions about changing the environment are in the hands of the people who function within the environment. For an outsider to impose the kind of order and structure according to his own unique view of "Utopia" would undoubtedly result in strong resistance which can manifest itself in many subtle and disguised ways.

Moos (1974c) has presented a methodology for facilitating social change which is particularly relevant to small environments that have a moderate to high frequency of interactions among milieu members. The approach has four basic components:

1. Everyone involved in the environment is given the opportunity to report his view of how the current environment is functioning on the relevant dimensions discussed earlier. In addition, all participants are asked to convey information about their conceptualizations of an ideal social system. Thus, the goals and general value orientations of the milieu occupants are systematically assessed.

2. Individualized feedback is then given on the results of these assessments. Particular attention is paid to similarities and differences in the perceptions of various important groups within the environment; for example, in a hospital setting: patients versus doctors and nurses; in an industrial setting: workers versus management; in a classroom: students versus teachers. In addition, emphasis is placed on the similarities and differences between the "real" and the "ideal" social environment and the subsequent implications for change.

3. Practical planning of specific methods by which change might occur along specified dimensions is then instituted. This planning is usually done with the help of a social systems change "facilitator" who is experienced in the ways in which different types of social systems can change.

4. The change process itself is assessed by one or more reassessments of the characteristics of the social environment. These results are continuously fed back to the participants providing an ongoing, systematic approach to achieving the kind of environment participants would like to have.

This methodology is linked with concepts of problem-solving, coping, and adaptive behavior. Many theorists have discussed each individual's active need for involvement and for the prediction and control over his own environment (White, 1959). The active propensities of man as scientist of different aspects of stimulation and variety-seeking motivation and of the importance of cognition and information seeking are central to planning effective social change methods. This approach is consistent with these important needs which include actively helping to mold one's social environment in desired directions. Its use may even help some individuals achieve a new competence, that of being able to change and control their own environment.

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Validity in Personality Trait Attribution

JOHN GORMLY *Rutgers—The State University*
WALTER EDELBERG *Rutgers—The State University*[1]

A fundamental assumption underlying the assessment of personality traits is that people have some generalized ways of behaving that can be observed across a variety of situations. The large number of research articles, graduate courses, and textbooks dealing with the assessment of personality traits illustrates that many individuals are proceeding as if this fundamental assumption were true.

Mischel (1968), in a very considered way, argued that personality traits are constructs of the *observer*, which may have little or nothing to do with generalized behavior patterns of the observed. He reviewed data which showed that the structure of personality traits which come from trait ratings of peers can be duplicated by people rating others after being in the same room for less than 15 minutes (Passini & Norman, 1966). These data, of course, are not definitive in supporting Michel's position with respect to the behavioral validity of traits[2] (Wiggins, 1973); nevertheless, Michel's review of the personality assessment literature clearly announces that data are sorely lacking that would definitively support the notion that personality traits are components of a person's behavior which are consistent across situations.

Mischel summarized data which indicate that stability in behavior is highest for intellectual and cognitive variables, where the situations used in eliciting the behavior have a high degree of similarity. Investigations of the cross-situational stability of social behavior, however, most frequently reveal a low degree of stability from situation to situation; this is in sharp contrast with expectations from the trait viewpoint. The disconfirming data for the fundamental assumption underlying the assessment of personality traits have a long and extensive history in the professional literature. Hartshorne and May (1928) reported a program of research which found a high degree of specificity for honesty in children. Mann (1959) reviewed the literature on the relationships between personality traits and behavior in groups, covering studies reported in the period from 1900 to 1957, and found little or no association between these two classes of variables.

It is certainly striking to find that this area of psychology, which has a long theoretical and empirical history and which has provided the bases for an extensive applied technology, cannot answer a challenge to demonstrate the validity of the trait approach. Highest priority should be given to remedy this deficiency.

Establishing the validity of the trait approach will require an extensive amount of research and development. The assessment of personality traits is not a unitary undertaking; techniques of assessment vary on several dimensions, which include: (*a*) self-description data versus the judgments of others, (*b*) structured or unstructured test stimuli, (*c*) structured or unstructured response options to the test stimuli, (*d*) objective or subjective methods for scoring the responses, and, of course, (*e*) the topic of the trait itself.

Some evidence which gives support to the validity of the trait approach has already been reported. Winder and Wiggins (1964) demonstrated that individual differences in the social reputation which boys had among their peers for aggressiveness and for dependency were related to objectively recorded differences in the overt behavior of these boys in situations where aggressive and dependent behavior had a high likelihood of occurring. Gormly, Gormly, and Johnson (1972), dealing explicitly with the paucity of data supporting the validity of

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Requests for reprints should be sent to John Gormly, Department of Psychology, 73 Easton Avenue, Rutgers University, New Brunswick, New Jersey 08903.

[2] Mischel's position on personality assessment is far more extensive than a criticism of the trait approach; he presents a rationale for an alternative strategy to personality assessment (see Mischel, 1973).

stable individual differences, examined the consistency of four sociobehavioral responses which people use following disagreement with another. The four responses (conformity, underrecall of the extent of disagreement, interpersonal rejection of the disagreeing person, and devaluation of the significance of the disagreement episode) were studied for individuals who each participated in three sessions which differed in the kinds of disagreement topics and the extent of disagreement. The authors found a substantial amount of consistency in the extent to which subjects used these four responses in the three disagreement situations. Ross (1966) reported convergence between teacher ratings of dependency in nursery school children and objective indicators of dependency in the behavior of the children. These three studies indicate that some traits measured in certain ways have demonstrated an acceptable level of validity; additional examples of trait validity from the professional literature, however, are not extensive.

The following study was designed to extend information of the validity of personality traits by using peer ratings of aggressiveness for male college students. Pairs were formed in which one member was high on aggressiveness and the other person was low on aggressiveness according to peer ratings. In each session a pair was observed through a one-way mirror by groups of nonprofessional judges who made ratings at three points during the session regarding which member of the pair was more aggressive. Judgments were made (*a*) when the pair entered the room and seated themselves at a table without speaking, (*b*) when the pair had spent approximately 10 minutes discussing attitude topics on which they had opposite opinions, and (*c*) after the pair had played a game for cash rewards. The purpose of the study was to examine the degree of convergence of aggressiveness scores between ratings by peers who shared a home environment with the subjects and ratings by judges who saw the subjects behave for a brief period of time in the laboratory.

Method

SUBJECTS

The subjects were 16 male undergraduates from an eastern state university. Eight men came from one fraternity, and 8 men came from another fraternity. Both fraternities are participating in a longitudinal study (an investigation of the biological correlates of personality traits). Judges were volunteers from an undergraduate course in the psychology of personality.

PROCEDURE

Members of the fraternities rated themselves and other members of their fraternity for aggressiveness. The format for rating peers involved a binary decision. Raters were given a list of names of the members of their fraternity who were participating in the longitudinal study. They were instructured to put a 1 next to the person's name if they considered the description "aggressive–assertive with other people" to be characteristic of the person and to put a 0 next to the person's name if they did not consider the descriptive statement to be characteristic of the person. A person's score for aggressiveness was the proportion of raters who said that the trait was descriptive of him. There were 26 raters and 30 people being rated from Fraternity A, and there were 26 raters and 31 people being rated from Fraternity B. On the basis of these ratings, the 4 persons who had the highest scores for aggressiveness and the 4 persons who had the lowest scores for aggressiveness from each fraternity were asked to participate as paid research subjects in the present study; thus, we had 8 high-aggressive people and 8 low-aggressive people according to peer ratings.

In addition to peer ratings, each person rated himself on a 5-point scale of aggressiveness in the following format:

Aggressive–assertive with other people
____I am more aggressive than most people.
____I am more aggressive than average.
____I am as aggressive as many other people—average.
____I am less aggressive than average.
____I am less aggressive than most people.

Each person was given a partner for the meeting that was to be observed by the judges. All pairings consisted of one high-aggressive person and one low-aggressive person. In one half of the cases a person was matched with someone from his own fraternity, and in the other half of the cases a person was matched with someone from the other fraternity.

The subjects agreed to participate in the study knowing only that they would be observed through a one-way mirror by undergraduates who would make judgments about their social characteristics. They did not know that they had been selected on the basis of peer ratings of aggressiveness nor that they had been assigned a partner who was in contrast with them on some trait.

CONDITIONS FOR MAKING JUDGMENTS

The judges were seated in two rows in front of a large one-way mirror which gave them good vision into the experimental room. The high- and low-aggressive subjects entered the experimental room accompanied by the experimenter who asked them to sit at a table in chairs which would have them facing each other. At this point the judges could not hear anything from the experimental room. When the subjects were seated, the judges were asked to indicate which person was more aggressive–assertive with other people, the person on their left or the person on their right (Judgment 1). The judges were asked to indicate what it was about the persons which made them decide as they did.

When Judgment 1 was completed, the experimenter initiated an audio system which simultaneously let the judges hear the subjects and the experimenter and tape-recorded the session.

Previous to attending this session the subjects had completed a large survey of social attitudes. The experimenter selected a topic for which the two subjects held opposite

opinions and asked them to try to convince each other of the merits of their own position. The experimenter let them discuss the topic for approximately 10 minutes. If they resolved their differences or exhausted their presentation of the issues before the 10 minutes were over, the experimenter would give them another topic. The experimenter placed no constraints on how the subjects were to behave during the discussion period.

When the experimenter terminated the attitude-discussion period, the judges made their second judgment. Again, they were asked to indicate which person was more aggressive–assertive with other people and to explain why they picked the person they did (Judgment 2).

When Judgment 2 was completed, the experimenter read the following instructions to the subjects:

> I am going to show you words on cards. I would like you to respond to the word by using it in a sentence, but only one of you may respond to each word. It is up to you which of you uses each word, but there is a certain pattern of order in which you are supposed to respond. The only way you can determine the correct responding pattern is by trial and error. When one of you responds, I will tell you whether it was the right person or not. Each time I say "Wrong," that pattern starts over again. If you can make six correct responses in a row between the two of you within 10 words, each of you will receive $2 for that pattern. We are going to do four patterns, so the most you can get for participating in this experiment is $8. However, if you cannot carrectly solve any of the patterns, you will still get the $5 promised you for participation. You may, if you wish, discuss whatever you feel is necessary. The pattern has nothing to do with the actual word I show you. Are there any questions?

After reading the instructions, the experimenter informally explained the instructions. When the fourth game was completed, the judges were asked for their final judgment of which person was more aggressive (Judgment 3) and why they selected the particular person that they did.

The total amount of time that each pair was observed by the judges was less than 25 minutes. Each group of judges observed and rated two sessions; therefore, there were four groups of judges for the eight pairs of subjects.

One week following the initial sessions, the judges returned to listen to tape recordings of two attitude-discussion sessions. The judges had not observed the particular discussions which they heard at this session. After they listened to each pair discuss their differing opinions on attitude topics, the judges rated which person was more aggressive. The subjects were identified by their position on the topics.

The total number of judges at the initial sessions was 50. The total number of judges who listened to the tapes was 36.

Results

Analyses of the data were conducted to examine psychometric properties of the aggression scores. The peer ratings of aggressiveness for each person were divided into two groups to examine the split-half convergence of trait attribution for two groups of raters within the fraternities. Each person had two aggression scores; each score was based on the proportion of 13 raters who said the trait was characteristic of the person. The split-half correlation coefficient was .81 for a sample of 61 men.

The total proportion of peers who attributed aggressiveness to a person was correlated with the person's self-description of his own aggressiveness. There was only a moderated degree of convergence for these two scores of aggressiveness ($r = .40$, $N = 52$).

Chi-square tests for association were conducted on the peer ratings and the data from the judges. The data were represented in 2×2 contingency tables: peer ratings of the subjects (high aggressive and low aggressive) and judges' ratings of the subject (high aggressive and low aggressive). The first judgments, which were based on physical characteristics and nonverbal mannerisms of the subjects, were significantly related to peer ratings ($\chi^2 = 9.2$, $df = 1$, $p < .01$). This represented a 65% agreement between the two groups of judges. The second judgment, which followed the completion of the discussion of attitudes, had a larger chi-square value than Judgment 1 ($\chi^2 = 16.0$, $df = 1$, $p < .001$). The third judgment, which followed the completion of the game, had the highest degree of concordance (72%) between peer ratings and judgments from observers in the laboratory ($\chi^2 = 19.4$, $df = 1$, $p < .001$). The contingency table for Judgment 3 is given in Table 1.

Judgments which were based on tape recordings of the attitude-discussion sessions were not significantly associated with peer ratings ($\chi^2 = 2.0$, $df = 1$, $p > .10$). The final test for association examined the number of correct and incorrect judgments (agreements with peer ratings) as a function of whether the members of the pairs came from the same fraternity or from different fraternities. Whether the pairs were constituted of persons who came from the same fraternity or different fraternities had no significant association with agreements or disagreements between judges and peer ratings ($\chi^2 = 1.8$, $df = 1$, $p > .10$).

TABLE 1

Relationship between Peer Ratings of Aggressiveness and Judges' Ratings of Aggressiveness

Judges' ratings	Peer ratings: High	Peer ratings: Low
High	36	14
Low	14	36

Discussion

The results of this study provide strong evidence for the position that social aggressiveness can accurately be considered as a personality trait; that is, peer ratings of aggressiveness describe a recognizable component of a person's behavior which is consistent across situations. This conclusion is based on the high degree of agreement found in the split-half reliability of peers and the ratings of judges who observed the subjects in a laboratory setting.

The choice of social aggressiveness as the trait to be investigated in this study was influenced by several considerations, notably the wide variance and social importance which aggressiveness (or dominance) has for so many animal species and pilot work on a fraternity, which was not one of the fraternities included in the present study, in which the split-half reliability of peer ratings was .92 for a sample which had 31 cases. Therefore, it may seem that we "stacked the deck" in an attempt to support the trait approach. We are not inclined, however, to represent the findings of this study as a demonstration of the general validity of the trait approach. The validity of each trait measured with any particular method will have to be determined empirically. The findings of this study do present hard data which challenge general assertions claiming that the trait viewpoint has no substance whatsoever in describing some real facet of human behavior.

With the validity of peer ratings of aggressiveness clearly established, we can begin on four related undertakings: finding a more economical way of assigning a person an aggressiveness score which approximates peer ratings; detailing the observable, behavioral cues which influence the attribution of aggressiveness to people; developing the predictive utility of aggressiveness scores; and establishing the validity of other personality traits.

The task of finding an economical substitute for peer ratings is likely to be a difficult one. The data from the present study demonstrate that a person's self-description of his degree of aggressiveness is not a substitute for peer ratings. There are many sources of unwanted variance that enter into trait scores from self-descriptions. For example, the meaning of the words with which you are required to describe yourself probably vary from person to person, and the value (desirability) which people place on traits is probably quite idiosyncratic. Sources of variance which enter into test scores have been well discussed elsewhere in the literature on personality assessment (see Wiggins, 1968).

While sources of unwanted variance which enters into self-descriptions probably have parallel forms in peer ratings, using large samples of peer raters may result in the unwanted variance being randomly distributed in the ratings and, consequently, having a negligible influence on each person's trait score. One immediate task is to find the minimum number of raters that can be used to approximate a larger sample of peer raters.

Although this study has provided evidence that peer ratings of aggressiveness describe a traitlike component of behavior, we do not know what observable cues influence ratings of aggressiveness. Obviously, it is not the content of what a person says when he argues his position on social attitude topics with a disagreeing other. When the judges listened to tape recordings of the high-aggressive and low-aggressive pairs, their judgments of who was more aggressive did not converge with peer ratings. The explanations of the raters on why they selected a person as the more aggressive are only moderately helpful at this point because many people respond with abstractions of behavior rather than with observable events. Frequently, the responders would say that they selected a person because he seemed more sure of himself, more confident, or more assertive. Among the observable cues most frequently given for naming one person more aggressive than the other were direct eye contact, banging on the table to make a point, directing who should respond during the game situation, and overtly showing dissatisfaction when the response during the game was said to be incorrect by the experimenter. When we know more about the behavioral characteristics which constitute a trait, then we will be a long way toward specifying under what conditions we can expect good predictive utility from a trait.

The format of the present study provides a model through which the validity of other traits might be demonstrated. In contrast to the "stacked the deck" evaluation of the method of this study, it is encouraging that the trait of social aggressiveness had such penetrance that the judges were able to be so accurate after less than 25 minutes of static observation. Consideration of the weak format under which the judges were asked to make their decisions strengthens the claim that the findings

of this study demonstrate validity for a personality trait and confronts radical challenges that the trait approach has no validity.

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Contributions to Education in Psychology Awards

The American Psychological Foundation invites nominations for the 1974 Contributions to Education in Psychology award. The annual award is in the amount of $1,000 to be given to the recipient for his achievements. It is hoped that the awardee's institution will contribute a matching sum, thus providing the recipient with a modest "grant" to be used by him as he wishes to "improve the teaching of psychology." Thus, the objective of the award program is to enhance the local instructional program, not just to recognize "master teachers." Because the awards are to be presented at the APA Annual Convention in September 1974, ***nominations must be completed and sent to the Committee by March 31, 1974.*** A nomination form with a statement of the guidelines and suggestions for documentation can be obtained by writing to Wilse B. Webb, Chairman, APF Teaching Awards Committee, Department of Psychology, University of Florida, Gainesville, Florida 32601.

Achievement Motivation in College Women

A Now-You-See-It–Now-You-Don't Phenomenon

THELMA G. ALPER *Wellesley College*[1]

Beginning with the earliest studies by McClelland, Atkinson, Clark, and Lowell (1953), achievement motivation research based on male subjects typically has yielded significant and readily replicable findings. Until recently, however, this has not been the case when female subjects were used. An example of this discrepancy is that for males, achievement-oriented instructions (emphasis on leadership capacity and intelligence) increase the McClelland et al. (1953) need for achievement score, while task-oriented, neutral, or relaxed instructions depress it. Comparable shifts have rarely been reported for females. Instead, both achievement and neutral instructional sets evoke equally high need for achievement scores in women (e.g., see Alper & Greenberger, 1967; Veroff, Wilcox, & Atkinson, 1953). Similar discrepancies arise when other hypotheses are tested: Veroff, Feld, and Crockett (1966), for example, hypothesized that picture cues closely related to the storyteller's own occupation would be less effective evokers of achievement imagery than unrelated cues. This hypothesis held for male subjects, but not for female subjects. To cite one more example, in the French and Lesser (1964) study, despite some positive support for the major hypothesis, the thrust of their main argument concerning achievement motivation in women was weakened in the end by a wholly unanticipated, statistically significant, *contrary* finding.

In addition to the inconsistency of the findings across sexes, what is also striking is the relative paucity of studies based on females. A survey of the specialized texts in this area reveals just how minimal the research on women has been. The first such text, *The Achievement Motive* (McClelland et al., 1953), devoted 8 of its nearly 400 pages to studies of women. By 1958, interest in the achievement and related motives had grown so rapidly that the record book *Motives in Fantasy, Action and Society* (Atkinson, 1958) required 873 pages to cover the literature, but the research on female achievement motivation was relegated to a single footnote (p. 77). Nine years later, Heckhausen (1967) in *The Anatomy of Achievement Motivation*, concentrating on the more recent American and foreign literature in this field, added only a handful of new studies based on female subjects. Of the 215 pages in this book, 9 dealt with sex differences.

Clearly, between 1953 and 1967, researchers seem to have been considerably less interested in female than in male achievement motivation. Why should this have been the case? Do equivocal and/or contradictory findings discourage continued research? Perhaps so. But a more fundamental question is, Why sex differences in the first place? Up to this point, most researchers seem to have assumed that male and female achievement motivation is governed by essentially the *same* laws, despite the theoretical implications of Field's (1951) early findings.

According to Field, achievement motivation in women, but not in men, is linked to the need for social acceptability, that is, the need to be liked. Prior manipulation of this need, it developed, could raise the McClelland et al. (1953) need for achievement score of women but had no effect on the scores of men. McClelland et al. (1953) interpreted these findings to mean that for women the achievement motive is less central than the affiliative motive, while the reverse is true for men. If this is so, it follows that if one wishes to study achievement motivation in its pure form, one *should* use male subjects, and as Sarason and Smith (1971) decried, this is exactly what most researchers have

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[1] This article was presented originally as the presidential address at the meeting of the New England Psychological Association, New Haven, Connecticut, November 1971. It has been updated to include additional studies.

Requests for reprints should be sent to Thelma G. Alper, Psychological Laboratory, Pendleton Hall, Wellesley College, Wellesley, Massachusetts 02181.

continued to do. But why, then, was Field's affiliation lead not more fully pursued? Unfortunately, affiliation arousal, at least within Field's framework, also turned out to be undependable for arousing need for achievement in women (see Atkinson's [1958] report of Vogel's [1954] findings).

Throughout the 1950s and 1960s a few investigators continued to use the original McClelland et al. (1953) approach for arousing achievement motivation, encouraged, perhaps, by Angelini's (1955) findings that an achievement-oriented instructional set could significantly increase the achievement motivation scores of women as well as of men. Though Angelini's results turned out to be no more readily replicable than those of Field, a number of seemingly relevant variables did begin to emerge. Among these were sex of the stimulus figures (Alper, 1957; Lesser, Krawitz, & Packard, 1963; Veroff, Wilcox, & Atkinson, 1953), the effect of the sex and position of the stimulus figures relative to each other (Alper & Greenberger, 1967), differences in value orientation (French & Lesser, 1964; Lesser et al., 1963), and age and family situation of the storyteller (Baruch, 1967).

With the publication of Matina Horner's (1969, 1970) work, a more substantive answer to the question as to why the past research on women had not been more fruitful came into prominence: Women, according to Horner, do not really want to be achievers, they want to be liked. Field's (1951) data had, of course, already pointed in this direction. But now the evidence was more compelling. Given the stimulus "After first term finals, Anne finds herself at the top of her medical school class," over 65% of Horner's University of Michigan coeds told avoidance of success stories, while to the "John" form of the stimulus, over 90% of the male subjects told success stories. Both the intrasex and intersex differences were statistically significant. Because the avoidance themas in the Anne stories focused mainly on Anne's fear (*a*) that no one would like her now that she was so achieving, or (*b*) that because she was so achieving perhaps she was not really very feminine, Horner's findings would seem to indicate that even in these days of women's liberation, on the projective level at least, women still perceive female achievement to be incompatible with the values of our culture. How else can one explain the vehement denial of Anne's achievement? We are told "the computer made a mistake"; or "it was just a fluke"; or "she won't be able to keep it up."

The 65% avoidance reported by Horner in 1968, 1969, and 1970 was based on the sample of 88 females she tested in 1964. Would more recent testing yield as high a percentage of avoidance stories? Using the same stimulus with a sample of "very high ability juniors at an outstanding coed university where the emphasis on achievement is very high," Horner (1972, p. 64) reported that 85% of these later subjects told avoidance stories! Our own data, based on Wellesley College undergraduates tested in the academic year 1970–1971, are of this same order. Given the Anne stimulus, almost 89% told avoidance stories. The sample here, though limited to 26 students, can be regarded as reasonably representative of the college population.

To get a measure of how females would react to the John form of the stimulus, we used a second group of comparable size. Now the percentage of avoidance stories plummeted to 50, a significant shift from the 89% told to the Anne stimulus, though still higher than the less than 10% Horner (1968) originally reported for her male subjects. In other words, not only do these very bright women seemingly not want (expect) to be achievers themselves, but they may also expect (want) men to be less achievement motivated than men really are. Data obtained on a comparable sample of 40 Dartmouth College males in response to the Anne stimulus did not support a similar pattern in men's expectations of women. Quite to the contrary, only 62.5% of the men told avoidance stories,[2] as compared with the 89% noted above in the Wellesley College data. This difference is statistically significant. Can it be that the nontraditional concept of the achieving woman is now more acceptable to men than to women? Two stories taken from the Dartmouth College sample convey the flavor of many of the protocols: The male storyteller recognizes Anne's fear of success and tries to allay her anxiety.

Anne: "Christ! I don't believe it."
John: "Anne, that's great. I mean it's fantastic!"
Anne: "I don't know John, I mean, I never expected anything like this. I mean, God, I'm going to have to think about this."
John: "What's to think about, you made it, you're on top!"

* * * * * *

Anne did all the work assigned to her during the term and

[2] N. Marcuse & E. McMonagle. Achievement Motivation in College Males. Unpublished paper, 1971. In preparation.

occasionally extra but she certainly did not expect to be at the top of her class at the end of finals. When she realized this she was very pleased but subconsciously worried about the reactions her fellow students would have. Would they think her above them and avoid her? This bothered her to such an extent that she tried to keep it to herself but one day soon after a good friend approached her and said, "Congratulations, Anne, I hear you're at the top of your class. I'm really proud of you." And Anne was too.

Lois W. Hoffman's (personal communication, February 1973) recent replication of Horner's (1968) original design suggests that the traditional concept of the achieving man may also be becoming less acceptable to men. Tested in the early 1970s, 79% of Hoffman's University of Michigan male subjects told avoidance of success stories to the John stimulus. This is a striking change from the less than 10% avoidance Horner obtained approximately eight years earlier. Hoffman's female subjects, however, did not show a comparable shift over time. Instead, avoidance of success continued to be the major female response to the Anne stimulus.

But can we really conclude from the studies to date that American women do not want to be achievers? Variations in the percentages of avoidance stories from study to study are fairly large, suggesting that sampling differences, as well as the wide range in years of testing, may be affecting the results. In addition, Anne's achievement is in a traditionally female-inappropriate field, that is, medicine. Is this what makes Anne's success so dangerous? To answer this question, in one set of studies we dropped the medical school reference leaving only "After first term finals, Anne finds herself at the top of her class." As predicted, in repeated samplings at Wellesley College, the reworded stimulus evoked significantly *fewer* avoidance stories than the original wording; the percentage typically approached 50. Similar results have been obtained with female subjects tested in other noncoeducational settings. For example, in a study done in two southern colleges, one for black women, the other for white women, the new wording evoked 36% avoidance of success stories for the black sample and exactly 50% for the white sample.[8]

If the cultural stereotype has been the real villain here, then changing the stimulus to read "After first term finals, Anne [John] finds herself [himself] at the top of her [his] nursing school class" should result in a significant *increase* in the percentage of success stories in female subjects and a significant decrease of such stories in male subjects. To date we have tested only the first half of this proposition. In this study, the subjects were enrolled in two different noncoeducational colleges. One group consisted of 30 junior college women, all of whom were preparing to be nurses; the other of 37 women in a small, four-year liberal arts college. Within each group, half were given the "Anne medical school" stimulus, and half the "Anne nursing school" stimulus. The hypothesis was upheld only for the first group, those who were preparing to be nurses. For these subjects, the medical school form evoked 20% success stories; the nursing school form, 86% success stories. This difference was statistically significant ($\chi^2 = 7.23$, $p < .01$). In the liberal arts sample, however, the two forms evoked success and avoidance stories about equally often. The striking finding with this latter group was the valuing of a liberal arts education and the devaluing of nursing as a career for women. Whether the "nursing school" stimulus would yield similar results in a sample drawn from a more prestigious four-year liberal arts college remains to be seen. Similarly, should we expect professionally oriented but not nursing-oriented females to respond as the nurses do, that is, with success stories?

In short, the now-you-see-it–now-you-don't phenomenon in female achievement motivation research may be attributable to wide differences in how the motive has been measured. Yet, if we are to account for the sizable group of coeds who *do* tell success stories (35% in Horner's original sample; 50% at Wellesley College when the medical school reference was omitted), the search must go beyond sampling and stimulus variables.

As early as the 1950s, evidence was beginning to accumulate which demonstrated that personality factors could affect the achievement motivation scores of women. Field, it will be remembered, had proposed as early as 1951 that being liked was more important to women than to men. Morrison's (1954) finding that the need for achievement scores of females elected by their peers to positions of leadership were significantly higher than those of nonoffice holders is consistent with Field's proposal, if one assumes, as Morrison did, that females who seek office *are* more concerned with peer acceptance (i.e., popularity with peers) than females who do not. Three years later, Alper (1957) reported similar results, but only when the stimulus figure was

[8] B. Grainger, B. Kostick, & Y. Staley. A Study of Achievement Motivation of Females in Two Southern, Noncoeducational Segregated Colleges. Unpublished paper, 1970. In preparation.

male. Yet, the connection between concern for peer acceptability and associating achievement as more appropriate for males than for females had not yet become explicit.

The first step in this direction was taken by Lesser et al. (1963) when an unexpected, statistically significant interaction emerged in their data: Bright, high-achieving high school girls scored higher on need for achievement only when responding to *female* picture stimuli under an achievement-oriented, as compared to a more neutral, instructional set, while equally bright underachievers showed the increase only to the *male* pictures. The authors had anticipated that both groups would show the increase to both sets of pictures. Using Mead's (1949) sex role stereotype argument, Lesser et al. (1963) argued that unlike the underachievers, the high achievers *do not* accept the cultural stereotype that achievement is female inappropriate. A follow-up study by French and Lesser (1964) supported this interpretation only in part. Using a specially designed role-orientation value measure, they found that woman's-role-oriented college women did indeed score higher on need for achievement, as measured by French's Insight Test, when responding to statements about women engaged in domestic and social activities, while intellectually oriented college women scored higher when the activities involved intellectual pursuits. But clearly this was not the whole story because, regardless of individual role orientation, achievement scores were always higher under the intellectual set when the stimulus involved males. Apparently, the values of the culture can transcend personal values. The extent to which the particular format of the value-orientation measure (as used in this study, "answer as you think *most girls* would answer") may favor this type of transcendence is worth considering. This should not, however, negate the seeming importance of the role-orientation variable. And this is where our work at Wellesley College began.

Our basic hypothesis has been that traditionally role oriented women, as in Mead's (1949) framework, are less likely to be achievement oriented (e.g., career oriented) than women whose role orientation is nontraditional. In testing this hypothesis, three considerations have dominated our research: (*a*) the need for, and construction of, a viable role-orientation measure; (*b*) the selection of stimulus cues for evoking achievement motivation; and (*c*) the development of methods for scoring the resulting protocols. Since the rationale of the procedures we used for meeting each of these objectives is described elsewhere (Alper, 1973), only a brief report is included here.

After trying out a number of standardized femininity–masculinity scales, we finally decided to design our own. The end product, known now as the Wellesley Role-Orientation Scale (WROS), is a 24-item pencil-and-paper self-rating scale consisting of three 7-item subscales and three filler items. The three areas tapped by the subscales are (*a*) traits college girls generally regard as "feminine" rather than as "masculine"; (*b*) role activities they find acceptable for themselves as women; and (*c*) career and/or career-oriented activities they consider more appropriate for men than for women.

In our current work, the WROS has imbedded in it the 38-item Levinson and Huffman (1955) Traditional Family Ideology Scale (TFI). Presented as a "questionnaire designed to sample opinions and attitudes toward problems facing college girls," the subject was originally required to indicate merely agreement or disagreement with each item. We are now using a 7-point scale (from strongly disagree to strongly agree). Unlike French and Lesser (1964), we ask the subject to answer in terms of how she herself feels about each statement. In repeated samplings, the correlations between the TFI and the WROS continue to be positive and highly significant (i.e., high-WROS scorers favor a traditionally oriented family pattern; low-WROS scorers do not). On the other hand, the WROS seems to correlate only weakly with the sex-stereotype measure of Rosenkrantz, Vogel, Bee, Broverman, and Broverman (1968) and not at all with the Crowne-Marlowe (1964) Social Desirability Scale.

The first extensive use of the WROS with Wellesley College undergraduates was begun in the academic year 1964–1965. At that time, the typical range of scores for unselected samples, using the agree–disagree format, was 5–19, the total possible range being 0–21. Through 1967, the average score remained at 8.5. By 1969, the typical range was more constricted, 4–14, and the average had dropped to just under 8. In current samplings at Wellesley College and other comparable noncoeducational eastern schools, 14 remains the top score for unselected samples, but at the lower end there is an occasional 2 and an even more occasional 1.[4]

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[4] B. Grainger. The Motive to Avoid Success and Its Relationship to Sex-Role Orientation in College Women. Unpublished honors thesis, Wellesley College, 1971. In preparation.

The average now approaches 7 rather than 8. As expected, moreover, the range for a small "consciousness-raising" group, a highly selected Wellesley College sample, is very constricted, 1–8. Yet even for constricted samples, the scores continue to have validity. Dividing the distributions into thirds, highs and lows within these constricted distributions differ from each other in the same ways as do highs and lows from wider distributions. What are these differences, and how have we tested for them?

Having suspected that the now-you-see-it–now-you-don't aspect of previous findings in this area might in part be attributable to differences in the strength of the achievement cues, we chose for our initial study the two pictures which Veroff, Atkinson, Feld, and Gurin (1960) had found particularly effective for arousing achievement imagery in adult subjects, namely, their "chem lab" and "machine shop" pictures. The chem lab shows two women in a laboratory setting; in the machine shop the two figures are men. In both, one person is watching, while the other is actively working at a task.

Using the McClelland et al. (1953) creative imagination instructions, but omitting all references to leadership capacity and intelligence, we found that both pictures did indeed pull strong achievement imagery as measured by their discrete variable scoring approach. But the need for achievement scores so derived did not differentiate between subjects who scored at different points along the WROS continuum, nor did the total score distinguish between successful and unsuccessful achievement striving. A more clinically oriented scoring scheme was needed. Since even a cursory review of the stories revealed a number of different themas, we decided to try a thema analysis. We subsequently learned that Horner (1968) had also abandoned the McClelland et al. (1953) scoring system in favor of a thema analysis and that only then did the striking sex differences in her data emerge.

For reasons detailed elsewhere (Alper, 1973), blind thema analysis works well for the chem lab stimulus and less well for the machine shop. For the chem lab, four major thema categories emerge: *successful achievement; dangers of achieving; task completion* devoid of achievement striving; and *themas unrelated* either to achievement or task completion. Within Horner's framework, the four would reduce to two categories: *success* and *avoidance of success.*

With rare exceptions, thema category and role orientation, as measured by the WROS, have been found to be clearly related: Success stories are significantly more often told by low-WROS scorers (subjects who score in the lower third of the WROS distribution) than by high-WROS scorers (subjects who score in the upper third). In Wellesley College parlance, the first group is referred to as the "low fems," or "lows," the second group as the "high fems," or "highs." Some highs also tell success stories, but as is shown below, their stories differ markedly from lows' success stories. "Danger" stories appear at both ends of the WROS continuum with about equal frequency, while task completion and unrelated themas are more typical of highs than of lows.

Lows' success stories include four subthemas: hard work pays off; support by an achieving model; achievement through cooperative effort; and achievement facilitated by competition or rivalry. Here is an example of the hard work pays off subthema, as told by a low:

> This lady scientist, like Madame Curie, is about to make an amazing discovery. She has been trying for months to find a cure for cancer and in spite of almost giving up because of repeated failures, she and her assistant know that this is the moment. If she drops the right amount of solution into the tube, it will neutralize and that will be the answer to her years of effort. She will do it.

The next story, also told by a low, illustrates the support subthema:

> The instructor is showing her student how to do a step in a complicated experimental procedure. The student has been working on the experiment for some time, and is nearly at the end of her work. She is observing carefully what the other woman does, and, when she returns to her own work, will repeat the procedure with care and precision and will be able to finish her project and achieve significant results. The instructor would like the student to become an able scientist, and is pleased with her work.

Highs' success stories are characterized by very different subthemas. These include instrumental success (the achievement is instrumental to the gratification of a need other than achievement qua achievement, usually affiliation); woman as man's helper; and to achieve, women have to work harder than men. An example of each, as told by highs, follows:

> [Instrumental success] Two unmarried Wellesley College chemistry professors are titrating a solution. They expect to find a new theory of nuclear activity involved in the solution process that will be critical to the instant coffee concerns. They hope to make a lot of money so they can go on a cruise and meet some eligible bachelors. They both fall in love with MIT professors and live satisfiedly ever after.

[Woman as man's helper] These two ladies are PhD holders in chemistry who are working for the hospital in a surgeon's research laboratory. They are married but wanted to do something for a career with research work filling the bill. They don't want fame but rather to help mankind in their small way to find a cure for hepatitis. They will be of great assistance to the doctor and will perform a valuable function in doing the tasks that the doctor doesn't have time to do.

Note that the first story is present oriented (finding husbands), while the second appears to be future oriented (after marriage). Stories in this latter category often explicitly state that the women no longer have young children to tend to.

The next story illustrates a high's version of the hard work pays off thema, with the special twist that to succeed, a woman would have to work *harder* than a man:

It is a time when women scientists are not common. They are in the minority and are often looked down upon by men. Therefore, they have to work extra hard to prove themselves. These two female technologists are working on a biochemistry experiment. The one standing is trying to train the younger woman whom she feels has much potential. Through their hard work and dedication to their project they will succeed. They will not only make a name for themselves . . . .

Since the *project* is successful, the protocol qualifies as successful achievement. But whether the achievement finally wins them acceptance by men is less certain. The story remains unfinished. Moreover, a downgrading of the status of the women (here to technologists) occurs to this stimulus only in the stories told by highs.

Combining the dangers of success, task completion, and unrelated imagery themas into a single category, avoidance of success, it is clear that it is the highs, not the lows, who tell such stories.[5] The difference between the two groups is statistically significant. As noted above, both highs and lows tell stories involving dangers of success. But there is a striking between-group difference in the nature of the projected danger: For highs, the danger is either to the *person* of the achiever or to her interpersonal relationships; for lows, the danger is that the *project* will fail. Thus, it is the highs' avoidance pattern, rather than the lows', which is the better fit for Horner's (1968, 1969, 1970) category of avoidance of success as a way of coping with the fear of being disliked.

[5] In the McClelland et al. (1953) scoring system, "danger" stories would qualify as achievement imagery (AI), whereas task completion stories and stories unrelated to achievement would be scored as TI and UI, respectively.

Here is an example of a danger of success story, as told by a high:

The lady with the test tube is a movie star who has sunk to doing TV commercials. The other is an admiring nobody [who] pities her . . . . They both fade into oblivion. No one will like them or pay attention to them . . . . The star commits suicide and the other marries a florist and gets fat.

The story, though perhaps tongue in cheek, seems to have a moral: A career, even a glamorous one, can only lead to "oblivion"; better to settle for the more conventional, if somewhat unshapely, role of the married woman.

The downgrading of the status of women in the chem lab situation turns out to be more characteristic of highs than of lows only when *both* stimulus figures are female. It does not hold when the stimulus involves a man and a woman.[6] Using specially posed picture stimuli,[7] we had expected that highs would perceive the male as having the higher status regardless of his role, worker or watcher, while lows would assign the figures equal status roles. But the chi-square analysis of the data failed to support this simplistic prediction. On the other hand, the overall results are consistent with the role theory framework of the research as a whole. Though both pictures now tend to evoke more success stories from highs than from lows ($.10 < p > .05$), unlike lows, highs typically attribute the success to the man: When the man is the worker, he achieves his goal, he solves the problem at hand; when the woman is the worker, the man instructs her, that is, tells her how to perform the task, and if the project is successful, it is the man who is credited with the achievement, the woman serving merely as his assistant. In lows' success stories, the success is the result either of the joint efforts of the two people or of the woman in the role of the worker. In other words, highs continue to perceive achievement as female inappropiate; lows do not.

But life for the achievement motivation researcher is never simple when the subjects are females. The results just reported were based on data obtained at Wellesley College in September

[6] T. G. Alper, H. Leet, E. Bragg, & D. Z. Eister. The Effect of the Sex of the Stimulus-Cue Figures on the Achievement Fantasies of College Women. Unpublished paper, 1972. In preparation.

[7] The seeming age differences between the two figures in the original chem lab picture, their relative positions and postures, as well as the laboratory equipment, were reproduced as closely as possible in the new stimuli.

1971 on the opening day of classes. The subjects were 24 upperclassmen, all members of a limited-enrollment upper level course entitled "The Psychological Implications of Being Female." Because students accepted in the course are required to meet several departmental prerequisites, the sample, though captive, could be regarded as a self-selected, special interest group. Would a less psychologically sophisticated sample of underclassmen respond in the same way? To answer this question, we tested the 113 women in our beginning psychology course, dividing them at random into two groups. The woman-watching–man-working picture was shown to one group; the man-watching–woman-working picture, to the other. Differences in the WROS scores of the two groups were well within the limits of chance.

A chi-square analysis of the data failed to confirm the findings for the older group because now both pictures tended to evoke more avoidance of success than success themas. In the group given the woman-watching stimulus, 80% of the lows and 82% of the highs told avoidance stories ($p < .01$ within each group). To the man-watching stimulus, 69% of the lows and 80% of the highs told avoidance stories. The difference for lows was not significant but that for highs approached significance ($.10 < p > .05$). Incidentally, both pictures evoked significantly more avoidance than success themas in middle-WROS scorers.

On the surface, then, the data for these younger, less psychologically sophisticated college women are seemingly more consistent with Horner's (1968) basic Anne stimulus findings than with either our original chem lab data or the new data for the older group. It is only when the individual WROS scores for all 113 subjects (highs, middles, and lows) are subjected to an analysis of variance for unequal *N*s that role orientation again emerges as an important variable. Picture cue, thema, and perceived status of the man and woman, as separate factors, turn out not to be related to WROS scores. Status × Picture Cue is related, however: Regardless of thema (success or avoidance), the mean WROS score of subjects who tell unequal status stories is lower for the man-watching picture; for the woman-watching picture the mean WROS score is lower for subjects who tell equal status stories ($p = .05$). It is the woman-watching situation, then, which, regardless of the fate of the project, more readily permits the egalitarianism of lows to emerge.

From the beginning of our research with the role-orientation variable, we suspected that neither the highs nor the lows necessarily represent dynamically homogeneous groupings. Returning to our original data using, for example, the Veroff et al. (1960) chem lab stimulus, though most highs told avoidance of success stories, a few told instrumental success stories. Within the lows, most told straightforward success stories; a few expected the project to fail. The differences in these themas call to mind four of the patterns that Douvan and Adelson (1966) described as characteristic of adolescent girls: the unambivalent high feminine, the ambivalent high feminine, the unambivalent low feminine, and the ambivalent low feminine. Douvan and Adelson are of the opinion that both ambivalent groups are in conflict about their femininity even though they admit that they have no direct evidence to support this. Our projective approach would seem to offer a viable vehicle for accumulating relevant evidence. What seems to be needed here, however, are stimuli which, unlike the laboratory setting of the chem lab, readily lend themselves to opting for alternative roles: domestic, nondomestic, or some combination of the two. To this end, we used a new picture stimulus, the "kitchen scene."[8] A youngish woman is seen in the foreground, and a small child, seated in a high chair, is to her right. The woman is painting the child's portrait. In the left background, something (a cake, pie, roast?) can be seen through the glass panel of the oven door.

The subjects for this study were 78 Wellesley College undergraduates, all members of the author's child psychology course. The testing was done during the first meeting of the class before any instructor bias could have influenced either the WROS scores or the thematic content of the stories. Thema differences between high- and low-WROS scorers were in the predicted direction and clearly significant (the chi-square, corrected for continuity, was significant beyond the .001 level).

The themas most frequently found in the highs' data were as follows: (*a*) Motherhood is put ahead of a career; the woman puts the painting aside in order to attend either to the baby's needs or to her own need just to enjoy or play with the child. (*b*) Motherhood and achievement are casually combined; painting is described as a hobby, and the end product is destined to be given either to

[8] The author is indebted to the Boston Gas Company for permission to reproduce this picture from one of their advertisements.

doting grandparents or to an equally appreciative husband. (*c*) Motherhood and a career *cannot* be combined. One may wish to combine them, but in the end, achievement wishes must be put aside for they get one into trouble—one cannot be both a professional and a mother. When one tries to combine roles, the cake burns, the child gets restless, or the husband disapproves—he wants his dinner ready when he gets home. Logically, Themas *a* and *b* would be appropriate to the high who is unambivalent in her acceptance of the role the culture traditionally assigns to women, while Thema *c* suggests some discomfiture with the demands of the traditional role, yet a need somehow to stay with it.

The themas most frequently found in the lows' data were as follows: (*a*) A career is placed ahead of the wife and mother role; the woman is a professional artist who has been called in to do the portrait. (*b*) Career, marriage, and motherhood can be successfully combined; the woman is an accomplished artist and is continuing to use her talents now that she is married. (*c*) Marriage and career cannot be combined; marriage is fraught with disappointments for one has to give up too much; therefore, it is better to walk out on the marriage and to pursue the career. All too frequently the career also fails, not because the woman is inadequate, but because society is. Again, logically, Themas *a* and *b* would be consistent with the value pattern of the unambivalent low, while Thema *c*, with its introduction of the danger theme, would be more characteristic of an ambivalent low.[9]

Having identified ambivalence–unambivalence by a fantasy measure, the next question is, Can these patterns be predicted from the WROS alone? While we do not yet have a definitive answer to this question, the two procedures we have worked out to test it seem promising. One procedure is based on decision-time theory, the expectation being that highs who are role ambivalent, as measured by thema category, will take longer to agree with the WROS items than highs who are role unambivalent, while lows who are role ambivalent will take longer to disagree with the items than lows who are role unambivalent (see Footnote 4). The other procedure uses the standard deviation of the subject's WROS score. The expectation here is that a role-ambivalent subject, again as measured by thema category, will have a larger variance around her own scale mean than a role-unambivalent subject. The first procedure could be used with either of the WROS formats: agree–disagree or strongly disagree–strongly agree. The second procedure would be suitable only for the latter format, strongly agree–strongly disagree.

Our search for the antecedents of differing achievement motivation patterns has just begun and has included so far both projective approaches and life-history data. The life-history materials are still too incomplete to report, but we do have some new projective data derived from a large sample of Wellesley College undergraduates, mainly freshmen and sophomores. The stimuli used here were two parent–child pictures. One, a mother figure and little girl, is an altered form of Murray's (1943) Thematic Apperception Test Card 7GF. The other, a father figure and little girl, is Card 10G from the Michigan Picture Test (Andrew, Hartwell, Hutt, & Walton, 1953). In both pictures, the figures are facing each other.[10] In addition to the modified creative imagination instructions described earlier, half of the subjects in this study were told that the grown-up in the picture was "upset about something," the other half that the "child was upset about something."

Our analysis of the data has centered around two questions: (*a*) Do highs and lows react differently to these two instructions? (*b*) Does sex of parent figure make a difference? We found that the themas for high- and low-WROS scorers *do* differ significantly; sex of parent makes some difference within each group, but the person who is upset, parent or child, does not. Regardless of sex of the parent figure, and regardless of which figure is said to be upset, highs significantly more often than lows attribute the upsettedness to the child's poor academic achievement ($p = .01$); in lows' stories, more often than in highs', the cause of the upsettedness is behavior unacceptable to the adult ($p = .01$). The most common complaints here are defiance of parental authority and clumsiness. Only occasionally do lows mention poor achievement. But when they do, lows see the parent figure under these circumstances as supportive and nurturant, while highs see the parent

[9] The intensity of the anger expressed by many of these girls suggests Shils's (1954) pattern of "far left authoritarianism" rather than genuine egalitarianism. See Bardwick and Douvan (1972) and Rossi (1972) for further discussions of sex role ambivalence.

[10] For a more detailed description of these stimuli, see Alper and Greenberger (1967).

figure as derogating and punitive. There is also a between-group difference in the outcome of such stories. In lows' stories, the child typically ends up as an achiever; in highs' stories, performance is typically not improved. While these differences occur in response to both pictures, they reach significance for the father–daughter picture ($p = .05$) and approach significance for the mother–daughter picture ($.10 < p > .05$).

The crucial question, of course, is, Are lows, in fact, better achievers than highs? There is some evidence that they are. For the subjects who served in the original WROS study (Alper, 1973), both weighted Scholastic Aptitude Test scores and college grades were available. The Scholastic Aptitude Test scores of highs and lows did not differ, but the college grades of the lows tended to be higher ($p = .10$). Lows, then, not only fantasize achievement, they actually do achieve. Judging from these data, it now seems likely that the high-achieving high school girls in the Lesser et al. (1963) study, the highly competitive, high-achieving coeds in Angelini's (1955) Brazilian study, and Horner's (1970) University of Michigan coeds who not only told success stories in response to the Anne stimulus but also performed better in competitive situations would all have at least one factor in common: Namely, they would presumably all score as lows on the WROS. Perhaps these girls also have in common a history of positive parental support and encouragement to be achievers, a pattern some investigators associate with high need for achievement in latency-aged boys (e.g., see Rosen & D'Andrade, 1959; Smith, 1969). As for our high-WROS scorers, if in fact they *have* been exposed to a more authoritarian child-rearing climate than our low scorers, as the high-positive correlation between the WROS and the TFI suggests, then the recurrent thema of the dangers of success which runs through their fantasy material, as well as the perception of parental figures as derogating and punitive, are also consistent. In such a climate, female achievement presumably would be regarded as sex inappropriate and would not be positively reinforced.

In conclusion, the now-you-see-it–now-you-don't phenomenon in the female achievement motivation literature appears to be a function more of wide methodological differences from study to study than of basic instability of the motive. Among the more important variables to be considered here are (*a*) personality factors, specifically differences in the sex role orientation of the subjects; (*b*) sampling differences (e.g., age, psychological sophistication, degree of ego involvement in the task, and perhaps even intrinsic differences in the demand quality of the settings from which samples are drawn—the more social, affiliative, emphasis of coeducational institutions versus the more academically competitive emphasis of the noncoeducational); (*c*) form and contextual differences in stimulus cues ranging from unspecified area and level of achievement to very specifically sex-inappropriate areas; and (*d*) differences in the procedures used in scoring the resulting protocols. The effect of experimentally manipulating such factors has been demonstrated in the new research reported here and in references to studies by other investigators.

One other shortcoming of some of the previous work in this area should be mentioned: a too rigid adherence, especially in the early studies, to the male model of achievement motivation. In our culture, men are not only expected to achieve, they are also expected to want to achieve. Women, on the other hand, have neither been expected to achieve, nor to want to do so. It now appears that both generalizations may be too broad, that indeed a reversal of attitudes may be taking place, with men now less interested in being achievers (Hoffman, personal communication, February 1973), and women more willing to recognize that achievement may be female appropriate as well as male appropriate.

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Learning Theory

Two Trials and Tribulations

P. E. FREEDMAN *University of Illinois, Chicago Circle*
MARK COHEN *University of Illinois, Chicago Circle*
JOHN HENNESSY *University of Illinois, Chicago Circle* [1]

Certain things in life are sacred. Among them are cows, tenure, and some basic laws of learning. Since cows are difficult to bring into the laboratory and tenure keeps us in the laboratory, we are left with basic laws of learning to desecrate. One law which is most basic is the very simple statement of learning theory: When an organism makes a response and that response is followed by reinforcement, there is an increase in the probability that the response will be repeated under the same stimulus conditions. However, Spence (1956) called attention to the need for learning theory to stipulate the initial and boundary conditions under which its hypotheses will hold. In this context, Zimbardo (1969) emphasized that the traditional principles of motivational control must be supplemented with some cognitive postulates. And Orne (1962) stressed how the demand characteristics of the situation may affect response tendencies in experimental settings.

There is no more severe attack on any statement than to propose its opposite. Therefore, we offer, and propose to investigate, the following speculation: Initially, in the learning situation, when an organism makes a response and that response is followed by reinforcement, there is an increase in the probability that the response will *not* be repeated under the same stimulus conditions. This hypothesis is predicated on the rather basic assumption that an organism, prior to exposure to an experimental situation, has had many and varied experiences with reinforcements and has learned through these experiences that once reward has been consumed or removed, it does not magically renew itself.

A fairly well documented example of such behavior comes from the animal spontaneous alternation literature (Freedman, 1965; Glanzer, 1953; Montgomery, 1951; Thompson, 1962). This is the tendency for an organism, when given two alternatives from which to choose, to select on the second opportunity the opposite alternative from that chosen on the first opportunity, even if the first was reinforced. It has been traditional to attribute such behavior to curiosity and/or exploration. An example of such behavior, dubbed but not explained as the *negative recency effect*, has been found in choice behavior in nine-year-olds and adults (Stevenson & Weir, 1961), but is curiosity the most explanatory mechanism?

This suggested one of the simplest mini-experiments that could be designed for human subjects. A box was constructed of wood 46 centimeters × 46 centimeters × 46 centimeters and painted black. (Psychologists have a propensity toward black boxes.) Two round holes with a diameter of 10 centimeters were cut on top and two lids, one with a square and one with a circle, covered the holes. Underneath each hole was a Pyrex cup 9 centimeters in diameter and 5 centimeters deep. A curtain could be drawn around the box to preclude a subject from viewing the top of the box and from observing manipulation with any of the cups.

The first group of subjects to be tested consisted of 32 traditional introductory psychology student compulsory volunteers. The procedures and instructions were as simple as the design. Prior to the subject's entry into the experimental room, each of the two cups was loaded with a quarter. The subject was brought into the room and given the following instructions: "I want you to choose one of these two lids. Lift the lid you choose, and,

[1] The experimenters would like to thank Robert Dulski for his assistance in data collection.

Requests for reprints should be sent to Philip E. Freedman, Department of Psychology, P. O. Box 4348, University of Illinois, Chicago, Illinois 60680.

if anything is in the cup beneath it, reach in and take it to keep. Go ahead." After the subject selected and pocketed the quarter, the lid fell closed and the subject's name, age, sex, and favorite activity were recorded as a time filler. For half of the subjects the curtain was drawn between trials, simulating a guessing game. For the other half no curtain was used. The instructions were then repeated, and the response was made and recorded. A verbal probe requesting why each response was made ended the experiment. If the subject chose the same alternative on the second trial, the response was recorded as a repetition. If the opposite response was made, it was recorded as an alternation.

In this first experiment, when the curtain was closed, the number of alternation responses was 5, and the number of repetition responses was 11. With the curtain open, there were 8 alternations and 8 repetitions. Typical responses to the verbal probe were, "No reason," "I like squares," and "Because." The score was Subjects 1, Experimenters 0. We were left in the position of accepting the fact that some subjects went back to cups they had just emptied and about as many did not, independent of conditions. We were also left with the impression that college sophomores are strange.

Meanwhile, another experimenter had taken the black box out into the real world to test children ages 3–11. Using the same basic instructions and nickels as rewards, the results were 11 out of 12 alternations with the curtain and 12 out of 13 alternations without the curtain. We gave ourselves ½ point for obtaining alternation and gave the subjects ½ point for not showing differential responses to the curtain. The score was Subjects 1.5, Experimenters .5.

Children alternate; adults go back to empty dishes! This is absurd. We reasoned that one basic source of repetition found in the college students could be an anticipation of deception. We therefore sought out what we hoped would be a less suspecting subject pool. We took our black box to the School of Nursing at the University of Illinois Medical School.

Without mentioning the word *psychology*, we solicited 25 volunteers, and got 3 out of 13 alternations with the curtain and 6 out of 12 alternations without the curtain. The score was Subjects 2.5, Experimenters .5. We were clearly losing.

Daunted, but not without the persistence characteristic of scientists who pursue rational hypotheses in spite of data, we decided on one last and desperate venture. Having had more success in the real world (with children) than in the laboratory, we set out once again. (Our major thrust was to reduce any hint of deception to a minimum.) As experimenters, we used 14 undergraduate honor students from the introductory psychology course. They were naive with respect to our hypothesis (but they might have guessed). The black box with its attendant curtain and Pyrex cups was locked away in the storage closet, and each experimenter was equipped with two identical three-ounce paper cups (Dixie Bathroom Refill System, American Can Company, Greenwich, Connecticut) with a yellow heart design. The experimenters were instructed to go forth and find six real people without regard for the sex, age, color, or relationship of the real people to them. They were to set up the experiment in advance by placing the two cups upside down about one foot apart on a table. A nickel was placed under each cup. The experimenter then brought the real person into the room and gave him the following instructions: "What we're going to do is very simple. I want you to choose one of these two cups. Lift the cup you choose and, if anything is beneath the cup, you may take it and keep it. Go ahead."

After the first response was recorded, half of the real people were told to turn around and face away from the table (back condition). The experimenter simply replaced the cup upside down in its original position. For the other half of the real people, the cup was replaced upside down while the real person was watching (front condition). The real person's age was requested and recorded. The following instructions were then read: "Now I am going to let you choose again. I want you to choose one of these two cups. Lift the cup you choose and if anything is beneath the cup, you may take it and keep it. Go ahead." The response was recorded, and the real person was probed as to the reason for his second response.

The results were collected and tabulated. Under the back condition, of 40 real people, 16 alternated and 24 repeated. The response to the probe indicated that almost all real people in the back condition treated the experiment as a guessing game. In contrast, under the front condition, of 42 real people, 36 alternated and only 6 repeated. The chi-square comparing all four conditions (front versus back and alternators versus repeaters) was 23.03 ($df = 1, p < .001$). Of the alternators in this condition, most responded that they selected the alternate cup because they knew the other cup had

nothing beneath it. Of the repeaters, the most characteristic response was that some magic trick was being performed. The range of ages was 4–50 with very few below 12 and most above 17.

After having lost twice to subjects under laboratory and quasi-laboratory conditions with our elaborate black box, and having scored a partial success with children, we had finally achieved victory with "real people." Under the rule of selective selection of data, we concluded that our hypothesis was supported.

Our current research again calls attention to initial and boundary conditions that should, if not must, be included in any traditional learning theory if the predictive power of that theory is to be improved. The data (that part of which we choose to emphasize) do not challenge the bedrock of traditional learning theory. But they do loosen a few pebbles that stand at its base. One of perhaps many of the initial conditions that determine the predictive power of learning theory is the expectancy that the subject brings to the learning situation. If the suspicion of deception is minimized in the learning experiment, the probability will be increased that the experimental subject will generalize from his/her prior experience and consequently avoid an alternative where reinforcement has already been consumed. Why not?

As a combination of our experiences and experiments, we have varying degrees of confidence in the following hypotheses:

1. Learning theory does not account for the experience that a real person brings into a learning situation. We believe that most organisms bring into a situation the hypothesis, based on prior experience, that once a reward is consumed or removed, given no other information, it will no longer be present on the next exposure. Furthermore, we believe one of the fundamental principles that an organism *learns* in any new situation is that reward *does* replace itself.

2. Learning theory, as presently stated, may only be applicable to utterly naive subjects.

3. There is no such thing as utterly naive subjects; there are only naive experimenters.

4. The college subject population is contaminated by expectancies of deception.

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Distinguished Contribution for Applications in Psychology Award for 1973

Conrad Kraft

CITATION

For diligence, skill, and effectiveness in applying psychological principles and knowledge to improve the efficiency and safety of man's interaction with his environment and in complex man–machine systems. His experiments reflect both a sensitivity to problems stemming from the technological nature of our society and an appreciation for the potential value of the data and methods of experimental psychology. His ability simultaneously to encompass both points of view has produced a series of unique and important contributions. In particular, his development of radar room lighting systems and his identification of a visual illusion during night visual landing approaches as a major contributing factor to a series of fatal jet aircraft accidents represent outstanding examples of his and psychology's contribution to our society.

At the 1974 APA Annual Convention in Montreal, Conrad Kraft was honored as the first recipient of the Distinguished Contribution for Applications in Psychology Award. He received this award for his program of research, in particular his work in aircraft night visual landing approaches. He was presented with a citation of his formal contribution to the application of psychology and also with a check for $1,000. APA established this award in order to honor individuals who have pursued a systematic program of research that has had an applied impact.

The January 1974 *American Psychologist* incorrectly reported that Conrad Kraft was a recipient of a Distinguished Scientific Contribution Award. That issue carried his biography and list of scientific publications (pp. 32–36).

Proposed Bylaws Amendments

Nominations and Elections Committee and Election of the Board of Directors by and from the Previous Council

Nominations and Elections Committee

At the meeting of August 27 and 30, 1973, the Council of Representatives voted to establish a Nominations and Elections Committee to make nominations for vacancies on standing boards and committees of the Association, rather than the Board of Directors which makes such nominations under the current Bylaws. The following proposed amendments will transfer the responsibility for making such nominations from the Board of Directors to the newly established Nominations and Elections Committee.[1]

ARTICLE VII

Section 8. The Recording Secretary shall be a Member of the Association, elected by the Council of Representatives, following nomination by the [Board of Directors] *Nominations and Elections Committee of Council* . . . .

Section 9. The Treasurer of the Association shall be a Member of the Association, elected by the Council of Representatives, following nomination by the [Board of Directors] *Nominations and Elections Committee of Council* . . . .

ARTICLE X

Section 1. The boards and committees of the Association shall consist of the standing boards and committees provided by these Bylaws and such other boards and committees as may be established in accordance with the rules of the Council of Representatives. Members of standing boards and committees, except those serving ex officio or as otherwise stated in these Bylaws, shall be elected for staggered terms by the Council of Representatives. The [Board of Directors] *Nominations and Elections Committee of Council* [may nominate a single person for not more than one-third of the positions on any standing board or committee and the Board] shall nominate at least two persons for [at least two-thirds of the positions] *each position to be filled.* Additional nominations may be made by members of the Council of Representatives. Selections of members to committees other than standing committees and those reporting directly to the Board of Directors and Council of Representatives shall be the responsibility of the board through which the committee reports, with the approval of the Board of Directors. Except as otherwise provided in these Bylaws, each standing board and committee shall annually elect its own chairman. Standing boards and committees shall meet not less often than annually at the call of their chairmen. They shall report annually in writing to the Board of Directors and the Council of Representatives.

[1] Bracketed material is deleted; italicized material is added.

Election of the Board of Directors by and from the Previous Council

At the meeting of January 18–20, 1974, the Council of Representatives approved the following recommendation from the Board of Directors:

Amendment to Article VI, Section 1, of the APA Bylaws to provide that the election of the Board of Directors from Council is by and from the previous Council. This would not apply to the present Council. The rationale is that members of Council will have had at least one year to observe their peers in action prior to voting for the Board of Directors.[1]

ARTICLE VI, SECTION 1

The Board of Directors shall consist of the President, the President-elect, the Past President, the Recording Secretary, the Treasurer, the Executive Officer (without vote), and six others elected by a preferential ballot by [the Council of Representatives from among its own members] *those members and from those members holding seats on the Council of Representatives during the year immediately preceding the election.* Directors not serving ex officio shall serve for staggered terms of three years. All members of the Board of Directors shall serve until their successors are elected and qualify.

Psychology in Action

COMMUNICATION WITH COLLEGE STUDENTS

HAROLD WEBSTER *Brooklyn College*[1]

Longitudinal studies of college students undertaken in the 1950s and 1960s revealed systematic changes in attitudes, interests, and values. Intensive interviews and lengthy test batteries were employed. Changes observed during college may be summarized briefly as (*a*) increases in autonomy with respect to familial, educational, religious, and political institutions; (*b*) increases in impulse expression; (*c*) decreases in authoritarianism; and (*d*) increases in both anxiety and realism, particularly concerning future possible roles (Webster, Freedman, & Heist, 1962).

Theorists such as Sanford (1962) were quick to point out some implications for personality theory, two important ones being, first, that the ego growth reflected by such changes is sufficient to permit greater expression of impulses during college, and, second, that this ego growth may in time counterbalance infantile or neurotic personality trends. As important as these findings may be, I think that we have overlooked something of even more importance to be discussed subsequently.

Although we did not predict the specific nature of the student upheavals of the late 1960s, we were hardly surprised by them. We had already found serious disenchantment with cultural institutions. For example, in one study we had labeled the principal factor in an attitude change scale *rebellious independence* (Webster, 1958). Later it was found that the more militant students scored higher than others on such empirical measures of development. A covert, persistent trend had become overt in those students who became militant and in those who simply withdrew from the more general culture (e.g., drug users).

The alienation has been more severe than many psychologists would like to believe, for they themselves are often implicated now. Even a decade ago we had little trouble in collecting data compared to today. Now the psychologist is often regarded as evasive or deceitful in communications with subjects. We often hear, "He pretends to be interested in one thing, but actually he is interested in something else." It is inferred that this "something else" may possibly be used to the subject's disadvantage.

We now resort to college committees that are set up to protect human rights. Malpractice insurance premiums are known to be increasing. Moreover, communication requires a *context*, and if the researcher seems to be identified with the current political establishment, then this increases the alienation. How far have we come in supporting a thoroughly paranoid society?

I see little need to document at length the fact that large numbers of subjects now distrust us. Attention should really be focused on what to do next. But in actual experience, during the past year I have seen graduate students lose their subjects in the following ways: Subjects (*a*) refused to be identified by name; (*b*) refused to return for additional testing; (*c*) asked for certain test material back and tore it up; (*d*) expressed fears of later blackmail; (*e*) said they thought that the researcher was testing only their consistency and truthfulness in answering questions; (*f*) compared answers even though politely requested not to; (*g*) complained that various tests, including ability measures, were "too personal"; (*h*) were unable or unwilling to read the items; (*i*) refused to answer items that they could not prejudge for meaning; (*j*) refused to give any data about family or friends.

Researchers themselves are coming to believe that such attitudes about giving data are of *primary* interest. Among those subjects who provided data without complaint, the majority were so passively dutiful that a colleague commented that they would "probably do absolutely anything if asked." This attitude is as problematical as the attitudes of the resisters. Cooperative subjects may want to provide the investigator with whatever they think it is that he is looking for. This is surely a far cry from the objectivity that once prevailed, or that we believed once prevailed, in data collection.

What can we do about this alienation, either from a service-oriented or from a research-oriented view-

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[1] This article was originally presented in the symposium "The Revolt against Assessment—Where Do We Go from Here?" at the meeting of the Eastern Psychological Association, Washington, D.C., May 4, 1973.

Requests for reprints should be sent to Harold Webster, Department of Psychology, Brooklyn College, Brooklyn, New York 11210.

point? It is *communication* for which better methods and theory are needed. Obviously, data collection is not mere information exchange, but communication at the most general level, the level that communication theorists refer to as *pragmatics*. Pragmatics includes both syntactics and semantics, but it goes beyond these to include the characteristics of the persons who communicate, especially as they are affected by communication.

Now a fundamental rule of communication is that it is supposed to be *true*. To assert by one means or another that one communicates falsely is a metacommunicative act that vitiates communication, such that the receivers become anxious, hostile, cynical, etc.

In human affairs deception never works, or never works for long. To prove a truth is admirable, but to prove that you can deceive is no achievement at all. Deception has been the easiest business of history. In order to study the conditions requiring deception, psychologists are confronted with a dialectic, which might be paraphrased, "How can you study the effects of deceit without being a deceiver?" The answer in my opinion is to study the truth about persons without deceiving them and without self-deception on the part of the researcher, to the extent that this is possible.

Efforts to induce subjects to compromise their principles, or efforts to deceive colleagues by sending them fake patients, can only have a double-edged-sword effect: You'll get it back, once the deception is revealed. Or your whole profession will get it back, as general mistrust and passive resistance. (How many practicing internists would send a person to a hospital with specific instructions to fake the symptoms of appendicitis, and then ridicule the colleague who unsuspectingly removed a healthy appendix? This kind of so-called research deserves to be censured by the profession, for it is clearly unethical.)

There is plenty of deviousness around to study without inventing more of it. A graduate student recently thought it necessary to harass subjects as one of the conditions in a study on the effects of frustration. Some of us went along with this scheme because of its apparent scientific merit. But I suspected (and said so) that most artificially controlled experimentation on human affective states (such as frustration) is a waste of effort and is possibly also an infringement on human dignity and freedom. Experimentally induced frustrations are not at all comparable to the natural and often severe kinds of frustration that affect most of us in our daily lives. We should ask ourselves whether studies of naturally occurring states have exhausted the possibilities of furthering our understanding of them before we embark on these artificial designs.

But lying is only one metacommunicative aspect of the extensive scientific problems of psychological communication. A great many more specific examples can be recognized in diverse research situations.

Metacommunicative phenomena (other than lying) include *Einstellungen* (from the psychophysics laboratories of the last century); rapport; many participation effects (e.g., volunteer, placebo, expectancy); the diverse mental test response biases; and coding–decoding values and conventions, including what to do with, that is, *how* to communicate with researchers, teachers, or psychotherapists. Even the "basic rule" of psychoanalytic practice ("Say everything that comes to mind, without exception") is metacommunicative, and so are the typical instructions to respond rapidly or uncritically to a personality inventory. Almost any situation in which information is collected or transmitted, verbally or nonverbally, is metacommunicative, which is actually the case for any social interaction. Lawyers know that there are rules of discourse, but for some reason psychologists are often unaware of these. It is this fact, the presence of significant metacommunicative effects, that we failed to take into account in the earlier studies of changes in student values, attitudes, and interests.

Twenty years ago a student might not respond to a test battery in the interests of "science," a motive that the staff thought was eminently compelling. But he usually did respond anyway because that was what everyone else was doing. Trust prevailed, and trust is a prerequisite for communication. Participation was voluntarism; that is, it was an act of will. I recall no poetic or Blakean types who said, "It doesn't feel right." But now the students *are* saying this, and I think that we had better listen.

But how shall we listen, or how *can* we listen, to students today? First, I think that *interviewing* has to be reinstituted into most research designs. In the past we never had much confidence in our tests anyway until we saw the substantial correlations with interview ratings. Of course, it is undevious interviewing that I am advocating, that is, a semistructured kind of interview in which there is plenty of opportunity for the subjects to bring up their own personal orientations or problems. These problems easily come to subjects' minds, for example, in discussing particular test items. These kinds of interviews are also essential for self-assessment.

Second, inasmuch as communication is strongly influenced by context, we have to be aware of the changing values of those who provide our data. Rowe (1973) attributed the radical change over a decade in Peace Corps assessment procedures to the changing values of the subjects. He discussed a marked trend away from the use of traditional selection and assessment methods, a trend that brings the psychologist more into a counseling role and that permits more self-assessment by Peace Corps volunteers. This trend was recognized too late to avoid "turning off" thousands of potential Peace Corps applicants, who resented the traditional assessment procedures.

Third, we will have to develop better *theories* of

communication. For example, rapport used to be considered some kind of constant across subjects. We still find in individual testing reports the statement "Rapport was established." Now just what does this mean? "Rapport" in order to take a test and be admitted to or rejected from some treatment program? "Rapport" such that one gets a job or fails to get one with General Motors? "Rapport" to be accepted or rejected for hazardous duty or demanding service somewhere? If we research these metacommunicative effects, we may improve our understanding of our contemporary communication problems.

The theory of communication that is general enough to embrace intrapersonal as well as interpersonal processes has not yet been written down, but it surely will be. Most communication models of the past have erred in being excessively objective; that is, they have ignored intrapersonal processes. One very well known model, the psychoanalytic one, has, on the contrary, had great difficulty in moving from intrapersonal processes into a wider domain, in spite of the commendable efforts to explain phenomena such as transference.

Reusch and Bateson (1951) were among the first to move toward a communication theory of sufficient generality to be of service in theoretical psychology, and Bateson (1972) continued in this direction. Good theory is necessary for good practice, and vice versa. We may hope for more of both soon.

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PSYCHOLOGICAL THRILLERS REVISITED

A TENTATIVE LIST OF "MASTER THRILLERS"

ARNOLD LeUNES *Texas A&M University* [1]

The topic of supplemental reading assignments to accompany the more scientific materials presented in the undergraduate psychology course is not a new one. Of particular concern is the introductory course, a purported vehicle for "turning people on" to psychology. In this connection, McCollom (1971) presented a most intriguing list of what he called "psychological thrillers" to accompany the introductory course. This listing of 15 books was created essentially to illustrate the author's contention that hard science can be substantially supplemented, as opposed to detracted from, by suggested readings of a "softer" and lower key variety. Taking McCollom's premise a step further, it would stand to reason that such a list, however tentative, could be drawn up to capitalize on the added knowledge, experience, and maturity of the beginning graduate student. Accordingly, the purpose of this article is to suggest a list of "master thrillers" for the more sophisticated reading audience that the psychology graduate student obviously represents.

Development of the Reading List

With regard to supplemental readings, personal experience has indicated that several points are noteworthy.

[1] Requests for reprints should be sent to Arnold LeUnes, Department of Psychology, Texas A&M University, College Station, Texas 77843.

One is that certain books seem to get significantly better reception than do others. Among them are those in which human behavior is presented by concerned parents, involved professionals, or sensitive writers with a flair for psychologically based character development based on their experiences with deviant behavior of various types. The graduate student particularly profits from this in that he has the freedom to integrate previous learnings and personal experiences into what is read. This, in effect, allows a brief book of 200 pages to become a psychological experience of considerable additional length. Perhaps this point alone is justification for a master thriller list, that is, the greatly increased opportunity to combine past and current scientific material with the reports of actual experience in the field. The crucial consideration here is a matter of appreciation somewhat analogous to reading *Huckleberry Finn* at age 15 and age 30.

A second point of worth bears on McCollom's contention, documented with carefully selected references, that you can lead a horse to water but you can't make him drink. In dealing with an often large and heterogeneous group of undergraduates, there is some tendency to require readings. With the graduate student, the inclination is to suggest readings based on the twin assumptions of higher motivation and increased homogeneity of interests.

Another emerging conclusion is the appreciation afforded brevity of expression. Many students, be they beginners or old pros, report that they are under heavy required reading pressures in most courses but would like to read outside materials of interest if they could be fitted into an already crowded schedule. Brevity should therefore be a virtue.

A fourth conclusion reached was that carefully selected readings can serve as potent reinforcers of current learning experiences, particularly if a variety of choices are made available and the element of punitive grading is held to a reasonable minimum. Few students would argue with this point.

Finally, student requests for stimulating supplemental readings run at a reasonably high level if the notion that such an option exists is put forward. Graduates and undergraduates alike vary little on this point, but the level of interpretative sophistication does. This, again, is the major justification for the master thriller listing.

Keeping in mind the aforementioned points of reader acceptance, simplicity of expression, brevity, reinforcement, and demand, a list of books was created for the use of beginning graduate students.

The Setting

The students in question were enrolled in the second half of a year-long proseminar series taught by six faculty members, involving, in separate five-week sections, sensation and perception, learning, physiological psychology, social psychology, personality, and abnormal behavior. The present readings represented 10% of the requirements of the final section on abnormal behavior, taught by the present author. The format for the book reactions was that they be couched in terms of the way the students would explain their feelings about a particular book if a friend were to walk up to them at a party and say, "Have you read———? What did you think of it?" One reading per week was required for a total of 5 from the 31 titles presented at the beginning of the section in the form of an annotated bibliography. An effort was made to soften the required nature of the assignment by placing it on an all-or-none basis; that is, read the book and write the brief informal report for weekly credit of 20 points or simply do not do it at all. There were no abstentions on the part of the class. The expectancy was that the readings would be done and the reactions would be of sufficient quality so as to preclude grading by degrees, and this assumption was borne out in performance.

The initial list of 31 titles has been abbreviated to include only the final 9 selections, and they are as follows:

MASTER THRILLERS

AXLINE, V. *Dibs.* Boston: Houghton Mifflin, 1964.
FRANK, G. *The Boston strangler.* New York: New American Library, 1966.
GREEN, H. *I never promised you a rose garden.* New York: Holt, Rinehart & Winston, 1964.
KESEY, K. *One flew over the cuckoo's nest.* New York: Viking Press, 1962.
KEYES, D. *Flowers for Algernon.* New York: Harcourt, Brace, 1959.
LINDNER, R. *The fifty-minute hour.* New York: Holt, Rinehart & Winston, 1955.
PETER, L., & HULL, R. *The Peter principle.* New York: Morrow, 1969.
SKINNER, B. *Walden two.* New York: Macmillan, 1948.
VISCOTT, D. *The making of a psychiatrist.* New York: Arbor House, 1972.

In order to qualify for the preceding listing, each book was read by at least one person in six during the course and achieved a rating of better than 2.00 on the following rating system: 1, excellent; 2, good; 3, fair; 4, mediocre; and 5, poor.

It would seem appropriate at this time to discuss briefly the nine books. *The Making of a Psychiatrist* was a top selection despite a very late start and owed much of its acceptance to the analogous situation between what the students are currently undergoing and the travails mentioned by Dr. Viscott, the author, in his struggle to become a competent psychiatrist. His reflections on psychiatry and psychology were of particular interest to the group because most aspire to become clinicians themselves, and many have worked as undergraduates and as graduate students in mental health facilities of various types. The experience level of the group was sufficiently high to allow them to

incorporate their observations into the many provocative points presented by Viscott.

One Flew Over the Cuckoo's Nest was a top-drawer reading selection because of the character of McMurphy, the continual machinations of Big Nurse, the potentials for misuse of psychosurgery, electroshock therapy, and power itself, and the general tragicomedy tone of the book.

Dibs, the No. 1 thriller on McCollom's list, was also big among this readership. The characterization of Dibs and the gentle expertness of Dr. Axline made for a most satisfying reading experience.

The Boston Strangler was popular, in part, due to the question of how such a thing could have happened in the first place. The psychological phenomenon of gullibility and the other side of the coin, the con man, created quite a puzzle for these readers. They just could not readily believe that some of the approaches used by DiSalvo to enter the apartments of prospective victims would "sell." Quite obviously and unfortunately, they did.

I Never Promised You a Rose Garden was a reasonably popular choice, probably due to the vivid portrayal of the lead character by the author Hannah Green. The sensitivity with which the book was written was commented on several times. Few, however, were aware of the autobiographical nature of the story.

The possibility of biochemically induced higher intelligence and the innumerable problems encountered in moving progressively from mild retardation to superior intelligence made *Flowers for Algernon* a selection of fair frequency. Certainly, the earlier movie version of this book had been widely viewed and had gathered some appeal as a function of this medium and the interest generated thereby.

Those who read *The Fifty-Minute Hour* did so purportedly to acquire a greater familiarity with psychoanalysis. The case histories presented by the late Dr. Lindner were thought provoking and received good acceptance, but the analytic interpretation of them was questioned. Perhaps this is reflective of the prevailing mood with regard to psychoanalysis today.

Walden Two was a doubtful addition to the original list and was included almost as an afterthought. Certainly its relationship to abnormal behavior is tangential at best. However, laboring under the highly unsubstantiated assumption that no reading list is complete without *Walden Two*, it was deemed worthwhile to include it. Accordingly, it was read by a third of the class, received rather good ratings, and met with general acceptance. It was a bit surprising that only another third had read the Skinner book prior to taking the course.

Another book with only peripheral concern to abnormal behavior was *The Peter Principle*. The decision to suggest this book arose out of a personal conviction, perhaps shared by others, that the mental health field is not without readily recognizable examples of the sort of inefficiency to which Peter and Hull address themselves. It was read by half of the class, and several students drew parallels between the examples of inefficiency mentioned therein and those they had personally observed in their experiences in mental hospitals, schools for the mentally retarded, or community mental health programs. Most, however, were more readily able to relate them to their immediate surroundings, namely, the university (using the university in a very broad sense because approximately 15 different universities are represented in the undergraduate experiences of the 18 participants). Several commented that the book was "cute," but not particularly illuminating for the purpose of scientific advancement.

Conclusions

Any conclusions drawn at this point must be highly tentative due to the embryonic state of the proposed idea. However, several points of worth seem clear at this time with regard to suggested readings. One is that beginning graduate students do welcome the opportunity to involve themselves in low-pressure readings of a type that allows them to integrate previous and current learnings. Second, there is a discernible thread of consistency that marks their interests in abnormal behavior, and these readings can constitute the beginnings of a master thriller list for students at that level. And the final conclusion to be drawn is that "soft" readings can complement the hard core of material in a scientific discipline such as psychology.

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Comment

The Manpower Issue Revisited

Reactions to the special manpower issue of the *American Psychologist* (May 1972), for which I served as guest editor, fall in four areas:

1. Constructive suggestions for achieving a better fit between psychologists and jobs for psychologists, for improving psychology's information base, and for uncorking new employment opportunities for psychologists;

2. Criticisms of the special manpower issue—disappointments over the omission of certain groups of psychologists, problems not covered explicitly among the articles published, or demurrals with individual contributions to the issue;

3. Specific psychologists seeking help in entering the job market or in changing jobs; and

4. Praise to APA for producing, or to individuals contributing to, the special issue.

Among the promising proposals received is one suggesting that psychologists avail themselves of employment challenges abroad. Another adjures psychologists to create opportunities for enhancing the human condition through improving systematically—omitting, alas, criteria for determining what constitutes improvement—the way people behave in schools, on the job, and in society at large, the reason being that the development of programs for producing these improvements would spur jobs for psychologists at all levels. But unless proposals like these, and others, are nurtured beyond their present embryonic brainchild stage of abstraction they will surely die aborning, and this would be quite possibly a loss to psychology and to society.

Nonacademic Psychologists

Lament has been expressed over the failure of the special issue to analyze more explicitly the utilization of nonacademic psychologists. Needless to say, the time and resources for achieving this highly desirable coverage were simply unavailable. The decision was made, therefore, to get on with the special issue and to produce it earlier so that it might be of some value to the profession and to the public, rather than to delay its publication in order to enjoy a more perfect and more comprehensive—though a later and less immediately useful—set of articles. Retrospectively, it is my judgment that this decision will have proven to be in the better interest of the APA membership and of the publics we serve. Nevertheless, it should come as good news to these critics, as well as to psychologists in general, that a proposal for examining the feasibility of methods for studying the nonacademic utilization of behavioral scientists has been developed and is under consideration for support by a government agency.

While nonacademic psychology was not examined as exhaustively and directly as would in fact have been attempted had the resources been available, still the special issue cannot be faulted for *totally* overlooking this domain. Even though it was ineluctably gross, Cates and Cummings (1972) did provide a ratio of applicants to positions according to area for academic, clinical-counseling, and industrial research classifications. Freeman (1972) surveyed a variety of nonacademic rubrics—government, business, other, nonprofit, and self, and not academic employment exclusively. Sorenson's (1972) study of the income of industrial and organizational psychologists naturally included and was virtually dominated by nonacademic psychologists. The review (Wildman et al., 1972) of salary schedules of agency clinical psychology programs certainly involved nonacademic psychologists.

While it is regrettable (though for all practical purposes unavoidable) that stress was given to the academic scene, it must be remembered that considerable attention was devoted (on the supply side) to psychology's students, a population including individuals many of whom eventually ply their wares beyond the groves of academe. Moreover, Speisman's (1972) sample study of the APA *Directory* revealed that 56.0% of America's psychologists are academically employed, a figure which is remarkably close to that of 56.6% which Freeman cites from the 1970 National Scientific Register of Scientific and Technical Personnel. The point is that, notwithstanding the fact that a hefty 44% of the nation's psychologists are employed in nonacademic settings—governments, schools (infra higher education), hospitals, clinics, community centers, business and industry, private practice, research centers, and elsewhere—data on the relatively more homogeneous 56% in academia were either more accessible or easier to generate than data on the more heterogeneous 44% in the foregoing nonacademic strata.

Independent, Private, or Group Practice

One reader grieved copiously because the special issue failed to embrace psychological manpower in independent, private, or group practice. Speisman's study revealed that 5.2% of us are in private practice. Employment data typically denote too that those who are self-employed command the highest salaries of all psychologists. On these two counts alone, then, while one may rue our explicit though not deliberate omission of psychologists engaged in independent practice, still I, as guest editor, cannot feel *that* remorseful by our failure to formally consider this 5% of the population of psychologists who, it must be added, don't do *that* badly financially.

Continuing, while we did not explicitly address ourselves to private

practice, we certainly did not exclude a variety of aspects of clinical psychology covered either wholly or in part in a number of articles. Since clinical psychology is the chief area of concern, generally speaking, to psychologists in private practice, this is still another reason why our exclusion of those in private practice does not strike me as a particularly unforgivable oversight.

Nevertheless, this omission will be remedied. APA currently has an HEW-funded project entitled Standards for Service Facilities. The project will examine the utilization of psychologists in agencies, institutions, *and private practice*. You private practitioners out there, are we forgiven?

The Vail Conference

Since publication of the May 1972 special manpower issue of the *American Psychologist*, a number of groups have addressed themselves wholly or in part to the manpower problem. Among these is the Vail (Colorado) National Conference on Levels and Patterns of Training in Professional Psychology.

The Vail Conference, sponsored by APA through an NIMH grant, brought together for a week in July 1973, over 130 psychologists and others concerned with training patterns, settings, and levels. The conferees were solicitous of the utilization of psychologists in a variety of environments. Perhaps foremost among these environments are those where the delivery of professional services is being supported by funds and employment opportunities no longer in great abundance so far as the more traditional fields of psychology—experimental and physiological, for example—are concerned.

At Vail, a number of resolutions were hammered out in plenary sessions following the development of recommendations by a number of task and interest groups. Of interest here are the resolutions bearing on manpower where, in particular, recommendations were made for modifying graduate (and undergraduate) training curricula so as to better prepare students to apply for and function in those jobs which are available and which are reasonably expected to continue to be available as greater emphases in national priorities are invested in applied and socially relevant programs. It should be noted that professional psychology was defined at Vail as including all those subdisciplines within psychology which interface with the providing of services for individuals, organizations, or institutions—not only clinical and counseling psychology, but also personnel, prison, consumer, school, community, and other components of psychological science.

True, the Vail Conference, like others preceding it during the past quarter century, has no official status and hence no real clout with university training programs. Still, it is not unreasonable to assume that through the publications, symposia, workshops, and controversies it will have spawned, the Vail Conference will have at least a modicum of impact in making explicit for university and professional training programs opportunities and ways for better preparing students to consider and become marketable for positions which the society believes psychologically trained people can assume.

A Final Word

The question is frequently asked as to how a young person contemplating graduate work in psychology should be counseled. In view of job shortages, particularly in academic milieus, should the aspiring psychologist be dissuaded from majoring in a field where openings are reportedly scarce? Should the student opt, rather, for specialties where the job opportunities are greater? Perhaps he should not even go into psychology? Should graduate departments curtail those programs where the data suggest graduates will find the fewest job openings?

Naturally, the answer to these questions must be reached by the individuals or the departments themselves. But a couple of thoughts might be in order here. First, what do you say to the person at the baccalaureate level who cannot get into graduate school? With selection ratios of, say, 30 applicants for one opening in a specified graduate program, many otherwise capable and promising students are unable to matriculate in a graduate program. A not unreasonable option for such a person is to look for a job in psychology, or for a job stressing the use of psychological skills or the application of psychological principles. Such jobs are available, though not necessarily plentiful, in community agencies, local and state government offices, and private industry. Is it not preferable for the baccalaureate psychologist to seek to satisfy his thirst for continuing in psychology by getting a job than to abandon altogether his interest in psychology when he is turned down by a graduate school? Given a year or two on this psychologically oriented job he might then wish to apply again to a graduate program, at which time he would probably have a sharper notion of the specialty within psychology in which he would like to concentrate. Or perhaps, be he a vigorous believer in and a promoter of psychology in his particular agency, he might have then developed a sufficient awareness of psychology's capabilities for helping to solve important problems so that the budget would have been acquired enabling him to hire a bunch of PhDs to work at his agency!

But considering those persons admitted to graduate school, it would be folly and perhaps cruel to inhibit a fledgling psychologist from specializing in whatever area of study he chooses, from tracking whatever direction his interests and motivations dictate, solely on the basis of a supply-and-demand ethic or manpower figures whose validity might be nil a few years hence. For example, it was not too long ago—maybe only four or five years back—when we were advised of the grave job shortages in engineering. Yet, today, according to a recent report (Hieronymus, 1972), this situation is changing dramatically; the demand for engineers is rising swiftly.

Even if the various predictions of employment forecasters turn out to be true, it would seem that the most psychologically defensible course to follow would be that of advising the student of the facts as these are known by us today, but then after he is furnished the facts he should not be discouraged from majoring in any area that is congruent with his interests and talents, nor should he be admitted grudgingly into a program with the deflating caveat "Don't say I didn't warn you." The choice is that of the individual.

Finally, the experiences and hardships so many of our colleagues have endured in the last few years underscore, as nothing else can, the importance of having a broadly based graduate education capable of enabling the psychologist to shift gears more or less in tune with the vicissitudes of the economy and the ebbs and flows of national priorities.

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Roger Bassett
University of Pittsburgh

On O'Leary's "Our Employment . . ."

Sound author and editorial judgment seem to have given way to an eager ness to publish a timely article in the case of O'Leary (February 1975). Not that I disagree with the proposed view favoring work sample tests as predictors, but the presentation is superficial and the supporting evidence is meager. In fact, O'Leary combined anecdotes and speculations with an apparent lack of employment and selection process savvy. Is there an increasing tendency toward laxity (or arrogance) among us that by virtue of being "experts" unwarranted and sometimes unsupported expertise develops? Editors are responsible for weeding out speculations that masquerade as credible scientific pronouncements; though speculations are necessary research antecedents, they are not the stuff on which conclusions should be based.

To be specific, there is the anecdote and absurd associated argument against using a valid selection instrument simply because the research showed that younger rather than older applicants are preferred. Yes, a consequence of utilizing a valid selection instrument is that "a particular 60-year-old might do a much better job than a particular 15-year-old (or many 15-year-olds) but will not be given the opportunity (p. 147)." What is the point? Forget the study results. Use the valid selection instrument sparingly. Combine the information obtained with the valid selection instrument with information from other sources. Research-based selection instruments, surveys are not perfect. After an other anecdote, O'Leary stated that an application blank validated against tenure would not be useful for predicting job proficiency. Should it? Hopefully, very few researchers are surprised. If proficiency-level predictors are desired, the research should be designed so that appropriate criterions are measured (for example, [illegible] forklift operation [illegible]), where is the evidence for the really appropriate of [illegible] effective predictors of job performance (that is, quantity and quality of work)?

A brief comment seems in order concerning the point that "The more nearly the test duplicates the specific tasks to be performed on the job, the greater the chances of developing selection devices that are fair (p. 148)." It seems that *valid* would have been a better word than *fair*, and though the logic is persuasive, without supporting evidence the contention is pure speculation; logic simply is not sought nowadays as a means of establishing fairness. The little evidence available on the topic (including some of my test validation research) indicates that some valid predictors approximating work sample tests are not necessarily fair for all job applicants. Work sample tests are, in fact, subject to the same fairness problems as are other types of predictors, and predictors need not necessarily be fair simply because they are valid.

There are sufficient exceptions to the conclusion that work sample tests are costly and difficult to apply to warrant a brief comment. Work sample tests for many jobs, especially clerical jobs for which filing, table reading, etc., are work samples, are no more costly to develop and administer than are other tests. When a company is willing to hire and train inexperienced job applicants—a practice certainly favorable to minority groups—costly high fidelity job simulations, especially where technical craft jobs are concerned, may be unwarranted as well as limited in applicability and utility. O'Leary flippantly dismissed a serious flaw with one sentence. The basic model penalizes those who are inexperienced applicants for a skilled position, for example (p. 149). Who generally are the inexperienced?

Last, and probably least, is the perplexing discussion of Type I and Type II error reduction through the application of work sample tests. Perplexing because the jargon seems out of place when α, β, N, and hypothesis testing are missing. In an employment context more familiar and meaningful terms are false negatives and false positives or hits and misses. Nevertheless, unless I misconstrued O'Leary's point, given the same conditions, the relative relationship

ship between respective error types should maintain when cutoffs are raised or lowered irrespective of the type of predictor. Certainly fewer selection errors are expected for predictors with higher validity but when the conditions are equal, manipulating cutoffs will reduce one kind of selection error and increase another.

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SIDNEY GAEL
American Telephone and Telegraph Company
New York, New York

O'Leary Replies:

I will not waste the reader's time nor my own in responding to Gael's *ad hominem* remarks. I would like, however, to examine some of the issues that he finally raises.

Predictive instruments are classifiable into three broad categories. The first can roughly be labeled *job simulation instruments*, which include assessment center techniques such as in baskets, management games as well as other content-valid approaches like "skills" tests in typing and shorthand. These instruments are frequently referred to as "piece-of-the-job tests."

A second broad category of predictive tests could be labeled *intermediary tests*. Here the emphasis is on measuring some construct such as "intelligence," mechanical ability, or personal qualities like abasement or interpersonal assertiveness. This category involves measuring some quality judged necessary for the job and correlating these scores with some performance measure. This is where much of the psychologist's and personnel person's efforts have been placed when it comes to validation work.

Category 3 can be generally labeled *biodata instruments*. It not only includes actually quantified biodata blanks but also scorable interview forms and self-ratings. These are tied to some job criterion (e.g., turnover, performance) on a purely actuarial basis, frequently with little attempt to explain logically the relationship between predictor and criterion. However, there is the compelling logic involved in the fact that past behavior is one of the most effective predictors of future behavior.

Each of the three instrument categories has advantages and disadvantages. The thrust of my original article was that there was some merit in psychologists reexamining the advantages of job simulation techniques. A fundamental and practical issue here is utilization of professional time in validating selection devices.

The Civil Rights Acts of 1964 and 1972 as well as the *Griggs* v. *Duke Power* case (in which the U.S. Supreme Court unanimously ruled against the employer's use of a number of preemployment pencil and paper tests) have amplified the need to develop "job-related" selection devices. All three of the above test categories can demonstrate job relatedness through concurrent or predictive validities.

The question is one of long-range efficiency in professional time utilization. As the demand for more valid selection systems justifiably grows and more people with the bare decision get rid of selection devices (O'Leary, 1973) we face the possibility that all selection instruments will be recklessly and foolishly discarded. This subsequently enlarges and dramatizes the need for valid selection procedures. The need is widespread and calls for a substantial effort by selection specialists. This in turn calls for the most efficient use of validation time by psychologists working in the field. A predictive instrument that more closely approximates the job itself will usually have a higher probability of being a more valid predictor of job performance than an intermediary type test simply because there are fewer non-job-related sources of variance. To repeat, this is not a call for eliminating or downplaying the other selection device categories but a recommendation for a closer examination and awareness of the timely advantages of the job simulation approach.

In numerous situations the job simulation approach may be a more efficient solution. In addition, the job replica approach may substantially reduce the amount of professional time wasted in looking for a frequently nonexistent performance criterion that *accurately* reflects job success. In these cases a systematic content validity approach using job simulation or a job replica may be most effective and more amenable to content validation.

Gael's first issue-related comment is to point out that selection instruments simply are not perfect, and we should use valid biodata results sparingly. This can be said about any validated selection device and indicates that Gael missed the point, namely that the article compared the advantages and disadvantages inherent in different selection approaches and was not a discussion of how one compensates for these disadvantages.

This is also true of his next comment regarding the prediction of turnover versus performance criteria using biodata. Biodata approaches have been more frequently used and requested in predicting turnover as a criterion than in predicting performance. Again, my comment was made to demonstrate the especially advantages and disadvantages of *past* uses of biodata.

In response to Gael's question, "Where is the evidence for the ready availability of biodata as effective predictors of job performance, that is, quantity and quality of work?" I would encourage him to begin with the references in the original article. There he will find one of the most notable studies in this area. It was conducted at Standard Oil of Indiana by Smith, Albright, Glennon, and Owens (1961). They predicted and

cross-validated success predictions on three measures of performance. Other successful and published studies include Lockwood and Parsons (1960), who used biodata items to predict performance of production supervisors, and Scollay (1957), who successfully used biodata to predict performance. These are just a few of the existing studies.

My statement regarding the greater probability of developing fair selection devices (devised having both content- and criterion-related validity) if we direct more effort in developing piece-of-the-job tests when appropriate, lead Gael to another comment: "and though the logic is persuasive, without supporting evidence the contention is pure speculation." Again, there is substantial evidence of the validity of job simulation techniques in specific hire and promotion situations. Again, I direct Gael's attention to my original reference list as a start. Bray and Grant's (1966) monograph is listed there, and there are many more reports of validated uses of the assessment center as one type of job simulation, including Bentz (1971) and Campbell and Bray (1967).

The disadvantage of the simulation approach to the inexperienced job applicant was acknowledged in the original article, and I reiterate my concern about it in as concise (rather than "flippant") a manner as possible. However, that concern should not immobilize one from constructive action. In many situations, the minority group members and the young who constitute a substantial proportion of the "inexperienced" group will have a better chance with a fairly run, job simulation method than an "off-the-shelf," verbally oriented pencil-and-paper test.

Finally, the commentator could more easily understand my discussion of Type I and Type II selection errors were he to look at the development of selection devices as a process. His comment suggested that he is, at the end of the whole process, comparing two validity coefficients—one from a job simulation predictor and the other from the "intermediary tests" category. Under such conditions, and if both instruments have the same validity coefficients, cutoff movement will lead to similar increases and decreases in both types of error. I recommend that Gael back up in the process of instrument development to the point at which he is deciding what selection tools to use. A specific selection problem may be approached more effectively when one uses a job simulation method because the job simulation device will quite possibly have fewer non-job-related sources of variance (error) and, consequently, generate higher validities than the other approach.

The problem of validating selection tools has developed substantial proportions in today's society. The systems approach appears to be our best solution for attacking the selection validation issue at this time. This means using all *three categories* of predictive data previously described. My article (O'Leary, 1973) was an attempt to add some perspective to the solution by underscoring the content-validation advantage and the reduced need for inference that are frequently found in the job sample approach.

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Lawrence O'Leary
Psychological Associates
St. Louis, Missouri

Job Samples:

A Better Approach to Selection Testing?

An important approach to selection testing has been given deserved emphasis by O'Leary (February 1973) in that he draws attention to the usefulness of content-validity considerations in the development of selection procedures. As the EEOC and OFCC guidelines force more and more employers to look carefully at their procedures, content validity will undoubtedly become a more prevalent and important technique. However, before accepting too quickly the notion that job samples are satisfactory content-valid predictors, and before claiming that "it is time for the profession of psychology . . . to communicate job simulation's significant advantages over traditional tests to test users [p. 149]," there are important considerations to make with regard to job sample tests. I would like to point out, first, some problems with O'Leary's presentation and, second, some important considerations that must be made about job samples.

The first problem with his presentation is that he stated the problem of individual exceptions to probabilistic predictions as if this is a problem peculiar to predictive and concurrent validity approaches. In fact, the problem of making nomothetical selection decisions which might be in error in individual cases is a problem of *any* imperfect decision scheme. Unless job sample tests are perfect predictors of job performance, they will also be subject to this problem.

The second problem with his presentation is that he switched criteria in midexample in order to show a "shortcoming" of the use of biographical data in selection. His example (p. 148) pointed out that biographical data validated against a criterion of turnover potential is not likely to be related to job performance on the task. Of course, that is true. However, if a task performance predictor was desired, that should have been used as the criterion in the first place. The failing in the example is not with the validity method, but with using data to predict something for which they had not been validated.

The final problem in his presentation was his statement that the use of job simulation tests as selectors would simultaneously reduce both Type I and Type II errors. That can only happen if the strength of the relationship between the job simulation and the criterion performance is stronger than the relationship between other predictors and the criterion. The relative strengths of those relationships is an empirical question, not one to be decided by a statement of faith that job simulations will reduce the non-job-related variance.

Psychologists who are faced with making decisions about the use of job samples in selection procedures should consider some additional points before accepting O'Leary's arguments (or the similar arguments of Wernimont & Campbell, 1968). First, there is the problem of bias in job sample tests. O'Leary mentioned this problem, but it did not dissuade him from exhorting psychologists to "speak out regarding the advantages of job simulation approaches [p. 149]." Though there is reason to believe that protected groups may be at a disadvantage on the performances required by many predictors, they are probably even less prepared to compete on job sample tests. For instance, if performance on a scholastic achievement test is used as a predictor, minority groups may be at a disadvantage because they have had (as a group) less educational opportunity. But their opportunities to gain experiences appropriate to performance on job sample tests are likely to be even more restricted than their opportunities to get educational experience! It is quite possible that the increased use of job sample tests will increase the bias of selection procedures.

The second consideration of job sample testing which psychologists should make is that a job sample is an ability test. It tells us what persons can do, not what they will do. Job performance is more than the possession of the necessary skills. It is the possession of the appropriate behavior patterns to utilize those skills. The best typist is not necessarily the one who typed with the greatest speed and the fewest errors during the employment test situation. It is the one who types with the greatest speed and fewest errors day in and day out while holding the job. The prediction of consistently good typing performance may be aided by predictors which do not look at all like a sample of the typing task.

Finally, the question of whether job samples have "significant advantages over traditional tests" is an empirical question. It will only be answered by determining our testing goals (lack of bias against protected groups, accuracy of prediction, etc.) and by empirically establishing the procedures which most nearly accomplish them. If job samples are best, then we should use them. If other predictors are superior in some situations, we should not discard them simply because they do not look like the job task.

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Milton R. Blood
University of California, Berkeley

The Right Question, Wrong Answers

O'Leary (February 1973) raised an important question but provided inadequate answers. The issue concerns the fairness and appropriateness of using certain biographical data (age, in the example on page 147; and suburbanite versus city dweller, on page 148) for empirical prediction even when such data are empirically valid. O'Leary then confused the argument by noting the idiographic or individual errors in such a situation. Of course, all decisions regardless of the variables on which they are based are uncertain predictions in which individual errors are made. The problem with using age, sex, race, place of residence, or other such variables as a legitimate basis for employment selection is not a question of psychometrics but of social values and public policy.

Suppose the rate of success among employees from minority racial-ethnic backgrounds is lower than the success rate among majority employees. Then racial-ethnic background is an empirically valid predictor of job performance under which majority applicants are to be preferred. The question remains whether employment selection on this basis is socially proper or even legal. Although great stress is put on empirical validity to justify selection procedures by both the EEOC (1970) and the old and new APA Standards (1966, 1973), empirical validity provides a necessary but not sufficient condition for an appropriate selection procedure. There should always be a preference for a more socially appropriate, empirically valid procedure over the procedure described above.

This leads us, as it did O'Leary, to concern with the content of the predictors used. Suppose Company W used racial-ethnic identity as a valid predictor of job performance in employment selection for forklift operators; Company X used a vocabulary test which was also empirically valid to select forklift operators; Company Y used tests of manual

dexterity and space relations which were also empirically valid; and Company Z used an empirically valid job sample test. Although each procedure will help in the selection of successful employees, there is considerable variability in the side effects of the procedures. Which of these procedures are legal and which are most socially desirable and in the public interest? This is not a psychometric question but one of social judgments.

We cannot escape the fact that we are forced to make value judgments about the appropriateness of predictor content and face validity. In our historical concern with objective, empirical evidence we have often tended to place greatest emphasis on the empirical, criterion-related validity of a selection process. And such validity is crucial, but it does not alleviate judgments about the quality and appropriateness of the content. Content validity is also crucial. In the present legal and social climate, content validity is also a necessary but not sufficient quality of an appropriate selection process and is no less deserving of attention and concern than empirical validity. Perhaps to stress this importance, much more work is needed in the preparation of guidelines and procedures for assessing content validity.

O'Leary's concern with job samples lies in his judgment that such content is more socially defensible, and, within the bounds of practicality, I agree with him. However, job samples do not necessarily have anything to do with reduction of selection error as stated on page 148 (that is an unanswered empirical question), and job samples create their own set of special problems, some of which O'Leary noted in passing. In addition, there are good and bad job sample tasks just as there are good and bad assessments of other types.

In summary, O'Leary raised an important issue and then buried it in inappropriate psychometric arguments and overly optimistic and simplistic judgments about one type of content-valid approach. These failings should not detract, however, from the issue of the importance of content-valid selection variables as we attempt to apply procedures in employment selection which are legally fair and socially proper.

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Nancy S. Cole
American College Testing Program
Iowa City, Iowa

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
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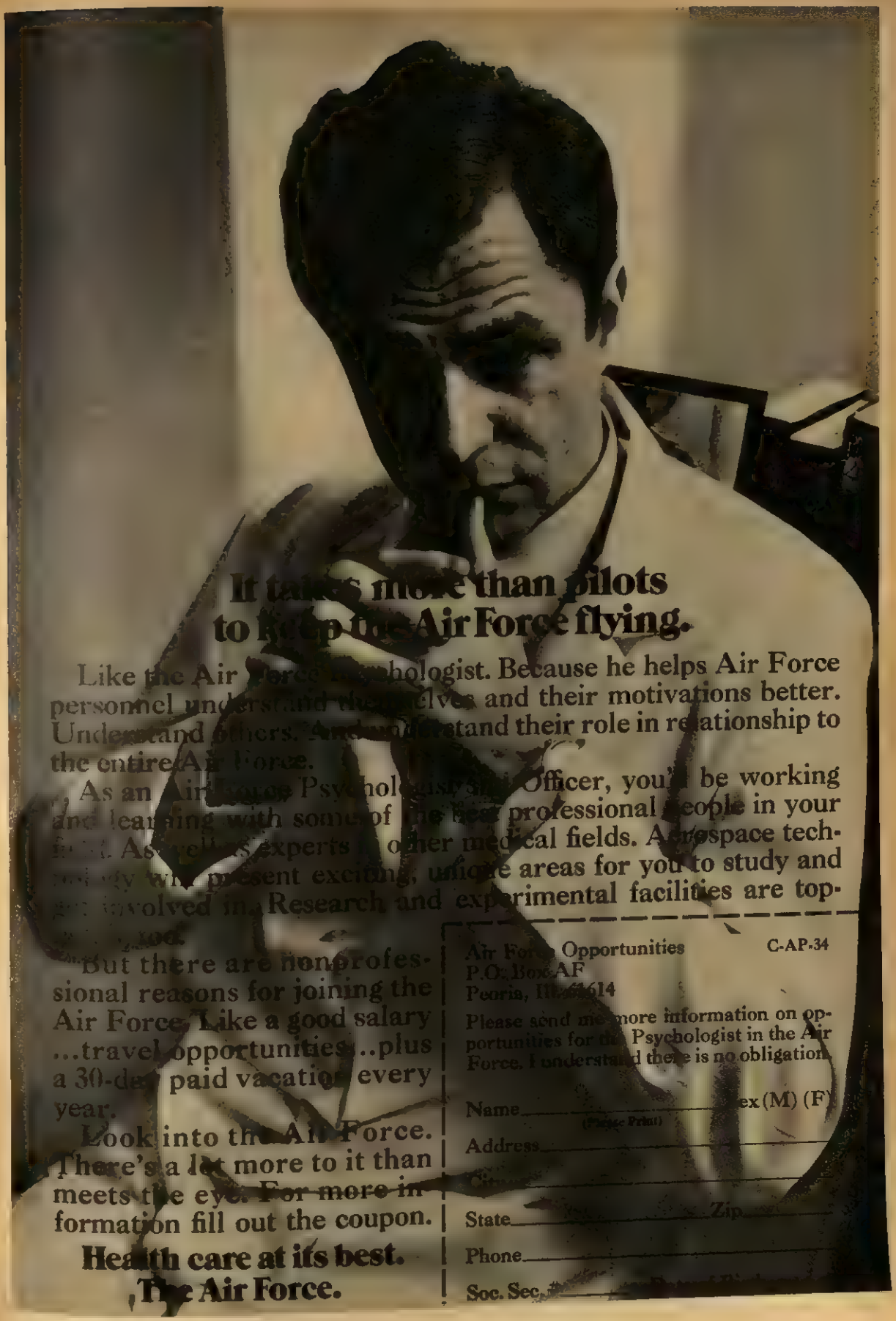
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Law Enforcement in an Urban Society

J. WOODFORD HOWARD, JR. *Johns Hopkins University*[1]

Crime, George Gallup informs us, is the "number one concern" of residents in American cities. Cutting deeply into the freedom and quality of urban life, not to mention private and public purses, crime and fear of crime have come to symbolize a deepening crisis of confidence in American justice and law enforcement. Social scientists, in their capacities as citizens and professionals, have a special responsibility to comprehend this pressing national problem and to decide what can be done about it. At the risk of belaboring the obvious, this article examines four basic conditions affecting the capacity of our legal system, especially of the police and the courts, to cope with urban ills, after which several remedies that are of particular concern to psychologists are suggested.

The Law Explosion

Most social scientists are generally familiar with the range of social problems that we seek to control through law: crime, drugs, welfare, zoning and land use, pollution, family squabbles, economic relations, and so on. Less obvious perhaps is a consequent condition of systemic demand often called the *law explosion* (Jones, 1965). Lawyers invoke this term to convey the enormous growth in both the quantity and qualitative difficulty of contemporary social conflicts to be solved by legal techniques.

The simplest way to describe the explosion is quantitatively. The workload of federal district courts, for example, doubled between 1961 and 1971. Filings in U.S. Courts of Appeals increased over 250% in the last decade. Spiraling caseloads have strained judicial organizations to the point that lawyers speak of crisis, legislatures create new judgeships, and scholars propose new tiers of courts. But the law explosion involves more than volume. The scope and complexity of legal conflicts have increased no less dramatically than caseloads.

Examine any recent volume of state or federal reports. The cases calling for informed decision range from the legality of the Vietnam war or the Alaska pipeline to the right of rehabilitation in prison or the wisdom of busing schoolchildren. The jobs of "judges and company" are simply harder. Americans increasingly turn to courts to settle social and personal disputes formerly handled by family, church, school, and other branches of government (Gilmore, 1961; Rosenberg, 1971). Behavioral scientists and some judges rightly perceive the need for aid from the social sciences in meeting these demands. Yet professionals on all sides of the bench are at sea about how best to mobilize scientific resources within the framework of the adversary process (Miller, 1968).

The causes of the law explosion are subject to much speculation. Some apparent reasons, for example, population growth, migration, secularization, and urbanization, are worldwide. Some are homegrown. In the United States, as deTocqueville noted years ago, great issues of public policy sooner or later will wind their way into the courts. Americans are also a moralistic people. Our criminal law is a "moral busybody" (Morris, 1973). We have a mental habit that the way to combat a public problem is to throw some federal money and a criminal statute at it. The result is serious overuse of criminal sanctions (Packer, 1968). Even though crime in the United States is primarily a state rather than a national responsibility, the President's Commission on Law Enforcement and the Administration of Justice found in 1967 that federal statutes define over 2,800 crimes. A parallel proliferation of crimes also prevails in most American states (President's Commission, 1967).

Malaise in Criminal Law

Criminal justice and urban problems, to be sure, are not coterminous. Urban law enforcers are taxed by much more than crime. Obviously there

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Requests for reprints should be sent to J. Woodford Howard, Jr., Department of Political Science, Johns Hopkins University, Baltimore, Maryland 21218.

is a growing role for behavioral scientists in such fields as torts, family law, environmental law, hospitalization, and incompetency, to name only a few. Crime is uppermost in the public mind—and for good reason. A recent survey showed that 52% of American urban dwellers feared to walk in their neighborhoods after dark. Last year, one in five Americans and one in three Americans living in central cities considered themselves the victims of crime or attempted crime against their persons or property. No one really knows how much crime there is, nor what causes it. Every discipline offers pet theories; most plausible, none complete. All we can say is that crime exacts a heavy toll in suffering, restricted freedom, and economic loss—and that crime is normal. Every reader at one time or another has been a criminal—or you haven't lived!

Crime *is* flourishing, far too much. Statistically, as Morris and Hawkins (1970) observed, "the United States may or may not be the land of the free, but it is certainly the home of the brave [p. 57]." Our homicide and assault rates are the highest in the civilized world. It is important, nonetheless, to keep the problem of crime in perspective. Perspective raises the bog of crime statistics, an area of perennial frustration because the best data, the Federal Bureau of Investigation's Uniform Crime Reports, are incomplete, dependent on local reporting and fudging, and seldom adjusted for population growth or inflation. Still, if used cautiously, some lessons come into focus.

1. Americans fear most the crimes that are likely to happen least, that is, violent crimes committed by strangers. The President's Commission reported in 1967 that the risk of serious attack by strangers is two times less than the risks from friends and relatives. Suicide rates are higher than murder rates. So are accidental deaths in homes. People are not necessarily safer on the streets than at home or alone. But the closer the relationship, the greater the hazard.

2. The risk of death and serious bodily harm from the automobile is far higher than from murder and assault. Over 56,000 lives were lost in automobile accidents in 1972, three times the homicide rate. According to the National Safety Council, more than half of all deaths from automobile accidents involved drinking. Yet the slaughter engenders little public alarm and few convictions.

3. It is less well known that, while homicide rates are still rising, murder and robbery rates were substantially lower (30%) in 1967 than in 1933. Crime rates go up and down with the times.

4. Homicide apart, general crime rates have leveled off and have actually fallen in several large cities such as Washington, D.C., and Baltimore, Maryland. The greatest increases now occur in the suburbs and among women.

The ambivalence of these trends is not to deny the existence of a crisis in criminal justice, but to delimit it. The malaise rises partly from ignorance and fear of the unknown. Too often, misleading crime statistics and press reports alarm the public into hysteria, symptomized by periodic "wars on crime," stampedes on civil liberties and police statistics, political manipulation at budget and election time, and pressures for unsophisticated responses, for example, more police and fancy equipment rather than better training of police and, above all, improved corrections, which everybody generally concedes is the most defective part of the entire criminal process. Even professional police chiefs, such as Patrick V. Murphy in New York and Donald D. Pomerleau in Baltimore, have lamented the uneven distribution of resources throughout the system. What good does it do for police to make more arrests unless the courts and correctional facilities are equipped to handle the increase?

Organizational Fragmentation

American law enforcement, especially in criminal justice, should be thought of as a system of interdependent, though loosely related, parts. Although some people regard the idea of system as more aspiration than reality, a major contribution of the President's Commission on Law Enforcement in 1967 was that, conceptually, it got across the idea that the various agencies of law enforcement are interdependent, rather than separate processes. Like a spider web, change in one element perforce alters the rest. Recognition of interdependence is especially useful in assessing how the strengths and weaknesses of each component affect the others. If we think of the law enforcement mechanism as a system, it becomes clear how fragmented things really are. The central characteristic of American government generally—diffusion and decentralization of institutional authority—is also the central characteristic of the legal structure.

The United States, as Walt Whitman remarked, is "not a nation but a Nation of teeming nations."

Formally, we have 51 legal systems; informally, thousands more. The 1967 census of governments counted 3,049 county governments, 18,051 municipal governments, and 17,107 township governments, most having authority to define and enforce crimes. There are over 25,000 separate police forces in the United States with great variety in organization and practice. The most individualistic decision makers in the country, moreover, are policemen and judges—and I have not forgotten the FBI or the Army Corps of Engineers.

Organizationally, ours is an interdependent system. But the system is one of loosely articulated parts or subsystems: police, detectives, prosecutors, defense counsel, judges, correctional and parole authorities, etc., in thousands of units. This fragmentation, albeit obvious, has several important implications. For one thing, it leads to a "bottom-up" view of the legal process (Dolbeare, 1967). To find the battlefield of urban justice, look down, not up; to state courts, not national; to trial courts, not appellate; to the police above all.

The men in blue talk as if they have the least discretion in the system. In fact they have the most, far more than the prosecutors and judges about whom lawyers fret. *The decision to arrest is the critical nexus of discretion in American law enforcement.* Several studies show that police themselves screen out far more legal infractions from the criminal process than do prosecutors or judges, whom police often condemn for permissiveness (Goldstein, 1960; LaFave, 1965; Morris & Hawkins, 1970).

Whatever relevance these findings have for the presumption of innocence, they speak volumes about the decentralization of authority in our legal order. The overwhelming number of social conflicts never become law suits, the overwhelming number of cases are never litigated in courts, the overwhelming number of litigated cases are never appealed, the overwhelming number of appeals are affirmed at state supreme courts or federal appellate courts, and this holds more for criminal cases than for civil cases. Only for the few cases in which the Supreme Court grants review do exceptions appear to these patterns (Howard, 1974; Jones, 1965). Hence, the overwhelming amount of justice in our system is dispensed in the community not by judges and juries but by the police. On the civil side the same is true of social workers. The police are the key to the quality of urban criminal justice. They have more direct power over the lives of our people than any other officials. They are clearly the cities' most important social workers, the major representatives of law to the community and, too often, mediators *between* law and the community (Morris & Hawkins, 1970; Reiss, 1971).

One obvious consequence of such decentralized authority is the limited intersection of law and psychiatry. With all respect, the grand debates over criminal responsibility and forensic psychiatry at trial are outside the main current of criminal justice today. Decentralization is also important for understanding police and the problems of their control. Members of each component of the criminal justice system are likely to have different perspectives about their jobs than do those in other parts. Role conflicts abound throughout the system. Such conflicts give rise to a larger theme: the tension between crime control and administrative values, on the one hand, and the values of due process and justice, on the other, flourishing throughout the whole (Packer, 1968).

To illustrate, take the police. The notion that the police automatically arrest everyone who breaks the law, and then turn the offender over to other professionals, is a myth. Police officers perform several jobs, ranging from crime prevention to prosecution, traffic control to social service. They face daily dangers and hostilities in a society deeply ambivalent about authority. All of these functions create an impossible set of expectations. Adding to the burden is extreme personal discretion. For unlike other professionals, police usually exercise their discretion alone, often without direct supervision in hostile environments, and with only post hoc standards of performance (Wilson, 1968).

What guides police discretion? Patrolmen tend to have a different view of their role than do other actors in the system. Their model of the job is not so much law enforcement as maintaining order, a task in which enforcing law is merely one of several means that may have to be ignored. By contrast, other actors in the system espouse a law enforcement view of the policeman's job. That is true of lawyers and judges, even detectives and professional police managers, and certainly it is true of their educated, middle-class constituencies. Thus, the police tend to have a peacekeeping view of their job implying wide discretion, while their constituencies tend toward a law enforcement view implying limited discretion. As James Q. Wilson (1968) showed, police chiefs are often the key

figures because they set the style or model to be followed in each community.

This divergence in role conceptions underlies much of the conflict between levels of a diffused legal system, especially over standards of evaluating performance. It also conditions police responses to external controls. The fact is that we have different standards of evaluating police behavior. To patrolmen, the norms of law enforcement, which may lead a professional police chief to go "by the book" or a judge to probate, are *conflict creating*. And so to a ghetto dweller, the middle-class professional norm of law enforcement comes over as harassment, because the law is unevenly enforced and everybody knows it. Wilson's (1968) description of changing police styles in Oakland provides an instructive example. After a new chief shifted the emphasis from peacekeeping to law enforcement, "all hell broke loose."

Of course, similar conflicts occur among officials in courts and corrections. Tension between the two models—crime control versus law enforcement and due process—exists throughout the legal system. We hear much talk today about law and order, as if the two always go together. In fact they do not. It is an ancient insight that law and order, not to speak of justice, may collide. Today the collision locates most directly in the daily work of the police.

Any seasoned bureaucrat understands that role conflicts of this sort are endemic in government. Administrative decentralization, however, makes the task of applying intelligent controls much more difficult. Fragmented organization means that few formal mechanisms exist to coordinate or resolve conflicts among the system's component parts. The major controls are *internal* within each subsystem. As Reiss (1971) pointed out, that is important not merely because it converts the courts into supervisors of police and prison officials, but because procedural rather than substantive issues tend to dominate our methods of control. Each subsystem also tends to create informal bureaucratic protections and shortcuts, much of it "justice without trial." Bargaining among prosecutors and defense counsel over guilty pleas is one prominent example. Passing the buck of determining criminal responsibility from jurors to psychiatrists as expert witnesses is another.

The most vexing products of diffused organization are our difficulties in controlling police misconduct. Basically, there are three principal control mechanisms over the police: (*a*) political–administrative, (*b*) judicial, and (*c*) professional. Presumably, it is the job of political authority to put Humpty-Dumpty together again, as in Britain or France, where ministries of justice coordinate and supervise the activity of the police, the courts, corrections, etc. However, the evidence strongly suggests that supervision by political authority in this country is fragmented, spasmodic, and particularistic. Mayors, governors, or attorney generals may occasionally issue orders to get tough on speeders, go easy on blacks, or clear the streets of addicts and prostitutes, but there is no common policy-making authority among the parts. Nor do the parts devise or monitor policy cooperatively. For all the efforts of the Law Enforcement Assistance Administration to encourage states and municipalities to create planning councils for criminal justice, these organs are usually little more than advisory groups or interdepartmental coordinating committees.

Consequently, the police and the courts are often in conflict, each attempting to control the other laterally, the police through the decision to arrest, the prosecutor through the decision to indict, and the courts through the control of evidence. Though police officials do not operate in a total policy vacuum, they clearly operate more independently in fact than in theory. How otherwise can we explain the diversity of what is called an arrest in this country or the divergent policies governing police interrogation prior to *Miranda v. Arizona* (1966:384 U.S. 436)? In most instances, our constitutions, statutes, and responsible political authorities gave no clear standards to guide interrogation of suspects. Police departments tended to make those policies themselves. The outcomes ranged as widely as the systematic exploitation of the psychology of confessions, condemned in *Miranda*, to the FBI's anticipation of *Miranda* itself.

Inadequate administrative supervision of police led to the second form of control, judicial review. Yet judicial controls are inherently limited. Lacking direct organizational command, judges wield mainly the sanctions of excluding evidence from trials and reversing convictions. At best these are indirect, lateral controls which put a premium on procedure and encourage a vicious cycle of continuing appeals. The irony is that, whatever one thinks of decisions like *Miranda*, the effects on police are probably exaggerated (Becker, 1969).

Judicial attempts to supervise police also produce a curious interplay with the third or internal control, professionalism. The Supreme Court resorted to the exclusionary rule and legislated the rules as to warnings during interrogation because the justices lost faith in police discipline. These rules contrast markedly with those in Britain and other English-speaking countries where illegal evidence is admissible in court if considered trustworthy, while police are disciplined administratively for obtaining it illegally. As Benjamin Cardozo asked years ago, Why should the criminal go free because the constable blundered? At the same time, however, the Supreme Court has relied on the professionalism of patrolmen on the beat in approving authority to "stop and frisk" (*Terry v. Ohio*, 1968:392 U.S. 1). And the Court's remedy for in-custody questioning is the countervailing professionalism of its own kind—lawyers—with differing professional values and orientations. How effective is professionalization—internalized standards as distinct from external political or judicial controls—over police conduct? To my mind this is one of the vital issues underlying the whole controversy between the courts and the police. The New Left and the Old Right appear to unite in the belief that government institutions, including law enforcement agencies, are overloaded in resolving social tensions. Doctors, lawyers, and professors all claim the right as professionals to supervise themselves. Can we safely entrust the same to the police?

Overload of the Courts

A symptom of the law explosion amid organizational diffusion is a quantitative overload of judicial machinery in many cities which seriously strains the quality of urban justice. First is interminable delay in civil cases, especially in state courts (Jones, 1965; Zeisel, Kalven, & Buchholz, 1959). Though some trial courts have cleared their dockets by crash programs, an average delay of two years in civil cases is not uncommon. The automobile is the chief culprit. So-called accidents inflict not only their statistically predictable quotient of death and injury, but also the injustices that inevitably arise between weak and strong litigants under the law's delays (Ross, 1970).

A second result of overload is assembly-line processing in urban courts, especially criminal courts. Even excluding traffic cases, one study in Los Angeles a decade ago estimated that the average workload per judge was 2,500–4,500 cases per month. The average trial time was 1½ minutes. It was estimated then that a 5% increase in not-guilty pleas would paralyze the courts—a prediction which the Watts riot confirmed. Los Angeles is not atypical. It is shocking but true that "the great mass of criminal cases, divorce proceedings, and adjudications into mental illness, alcoholism, narcotics addiction, and juvenile delinquency" are disposed of on "an assembly-line basis" all over the United States (Jones, 1965). This is volume processing by mass production techniques, not adjudication as studied in school. Furthermore, these are the very problems with which most citizens confront the courts, the most intractable of human problems we attempt to solve through law, and, all too frequently, the locus of discrimination among "haves" and "have nots" (Jacob, 1969, 1973).

A third manifestation of overload is "justice without trial." Together, delay and workload have spawned an informal system of pretrial bargaining and settlement so pervasive that our system may well have changed under our noses from a system of adversary adjudication to a process of decision predominantly pretrial in character (Skolnick, 1966). Adjudication in court no longer defines our system. Adjudication is largely a consequence of failure to settle by negotiation or by threat. That is true of both civil and criminal cases. In the great cities, trial has become "a painful last alternative" in private actions (Jones, 1965). In criminal cases, too, a wide range of practices from prosecutor screening and plea bargaining to assembly-line trials reduce criminal trials to a fraction. Though cities differ widely in their methods of weeding out cases, most resort to one form or another of mass processing to avert adjudication (Jacob, 1973; McIntyre & Lippman, 1970).

The most worrisome of these techniques is plea bargaining. Guilty pleas have become an administrative imperative in keeping the American trial courts open. In 1969, over 95% of the felony cases in New York City and 80% of those in Philadelphia were disposed of by guilty pleas. In U.S. district courts, too, guilty pleas account for roughly 90% of all convictions. Now, much is to be said for the practical advantages in pretrial bargaining. Defendants usually receive lesser penalties than they would after trial in a system in which sentences are, generally speaking, excessive. Justice is swifter and cheaper than by trial and can be flexibly tailored to individuals (Jones, 1965).

Adjudication in court has always rested on preliminary screening and argument to narrow the scope of conflict (Fuller, 1964). Plea bargaining fits within a broad trend toward pretrial settlement among fully informed parties in accordance with the values of voluntarism and privacy.

Let no one mistake the risks, however. Foremost is regression to a presumption of guilt. A negotiated plea presupposes guilt. How is that guilt determined? The answer is administratively without the procedural safeguards that the adversary system, for all its faults, promotes. The law explosion aggravates the danger because prosecutors, defense counsel, and judges share a common interest in expediting the volume. Cooptation of erstwhile adversaries at the expense of individual defendants is a constant temptation (Skolnick, 1966).

Thus, plea bargaining intensifies the tension between administrative and due-process values in the criminal process. Most defendants will and *must* plead guilty or the judicial process in many American cities will collapse. This is what underlies police resentment of the Warren Court's extraordinary moves enlarging the right to counsel and governing police interrogation—not the loss of a few individual cases, but the impact on scarce time and resources in contexts of massive volume pressures, threats to public order, and declining public confidence in the thin blue line. And it helps to explain why compliance with these decisions has been equivocal (Becker, 1969; Oaks, 1970).

A book could be written about the ramifications of court overload, but two conclusions bear emphasis. First, the problem of striking a balance between individual rights and social needs in this field will not yield to ideological flack, Left or Right, but only to hardheaded decisions rooted in the realization of what our system has become. Second, overload, itself a symptom of an urban society, may not have reached the point of crisis; but mushrooming intakes, along with the other conditions described, impose serious constraints on the capacity of judicial institutions to control urban conflicts, let alone to direct social change in lieu of stalemated political agencies. I stress these limits not to replay Cassandra's theme, nor to underrate the contributions of courts to social reform, but to warn against the easy assumption that the judiciary offers a substitute around a blocked or overloaded political process. Personally, I think that courts have responded to urban problems as well if not better than the rest of the government. But they are too limited structurally, functionally, and physically to provide a safe haven from the old cliché that our governmental system, like a piston engine, works best when all the parts work in concert. That is another way of saying that the judiciary offers no escape from the tough, collective political tasks lying before us in legislatures and executive agencies. In short, the law enforcement apparatus alone cannot solve the problems of crime in America.

Remedies

What then can be done? A responsible critic should stress that there are realistic reforms and proximate solutions that would substantially improve the capacity of law enforcers to deal with urban problems. It is symptomatic of the malaise that most of these proposals have flourished among experts for at least two generations. While no single line of attack will accomplish much by itself, in concert the following general approaches would help considerably in making law enforcement more effective.

MORE RESOURCES

Though increased spending is the bureaucrat's eternal remedy, the case is strong that until quite recently law enforcement has been underfinanced. As late as 1969, the total expenditure for law enforcement at all levels of government was $6.7 billion, roughly equivalent to the space program at its peak (*Baltimore Sun*, May 4, 1972; Committee for Economic Development, 1972). Former Chief Justice Warren has put the point graphically: The annual budget of the federal courts equals the cost of one cruiser. Despite the recent spurt in federal spending for law enforcement, the police, the courts, and especially corrections remain badly overtaxed.

REORGANIZATION

It follows from the fragmentation of authority that substantial reorganization is in order to improve controls and coordination among the interdependent parts of the legal system. The classic argument of Benjamin N. Cardozo in favor of ministries of justice at local levels is still to be refuted—or acted upon. Surely it is possible to devise ways in which political authorities and judges could sit

down with police chiefs, prosecutors, defense counsel, public defenders, and correctional officials to improve the administration of justice without undermining the adversary process in individual cases.

Similarly, heavy reliance on pretrial stages of litigation creates an urgent need to stiffen procedural safeguards during plea bargaining. Many dangers in this practice would be reduced if controlled by the elementary standards of due process (e.g., notice, the right to counsel, a public hearing) which courts and legislatures require for other forms of administrative adjudication and which police demand for their own disciplinary proceedings. Another promising approach is to relieve the courts from settling disputes that other agencies are better equipped to handle. A prime example is automobile accidents. No-fault insurance would remove the single greatest cause of delay in court and provide superior compensation to accident victims at lower cost (Bombaugh, 1971). Mediation and arbitration offer still further alternatives to adjudication in court (Rosenberg, 1971).

REORIENTATION AND REDISTRIBUTION OF RESOURCES

For once, however, the usual bureaucratic remedies of more personnel, more money, and reorganization are not enough. A consensus is emerging that Americans need to reorient their priorities and to redistribute their resources in order to combat crime more effectively (National Advisory Commission on Criminal Justice Standards and Goals, 1973). At the top of the agenda is a more realistic, less moralistic, and less ambitious conception of the criminal law. John Stuart Mill urged over a century ago in *On Liberty* that society is justified in depriving an individual of life or liberty only for its own self-protection, not for the purposes of reforming the immoral. While the aims of the criminal law are multiple, the central object is protection of persons and property (cf. Hart, 1958; Morris & Hawkins, 1970). Government should get out of the business of enforcing private morals by changing the substantive rules that make criminal law a "moral busybody." Legislatures in Britain, Illinois, and New York have successfully completed this task and, in response to the recommendations of the Brown Commission, recodification now proceeds in Congress.

A few examples will illustrate why the case for *decriminalization* is so compelling. Over half of American arrests concern crimes without victims. The crime of being drunk in public accounts for almost one third of all arrests (President's Commission, 1967). Granted, rounding up drunks and bums consumes a smaller portion of police time and represents informal relief to alcoholics and derelicts. But the "revolving door" is obviously defective as a social welfare policy and represents a serious diversion of police resources from the prime task of public safety. For example, only six drunkards in the District of Columbia were responsible for 1,400 arrests and combined sentences of 125 years at a public cost of $600,000 (Morris, 1973).

Other so-called "victimless" crimes, such as vice, gambling, pornography, and sexual deviation among consenting adults, generate similar exercises in futility. The criminal process is an ineffective and inappropriate way to control these eternal, yet intensely personal, transgressions. For lack of complaints, police discretion is greatest in these fields. Organized crime, temptation to corruption, and entrapment of informers abound. Police cannot possibly enforce all of these laws evenhandedly. Prosecution is a lottery (LaFave, 1965; Newman, 1966). Over two thirds of those arrested for gambling are blacks. Such conditions put law enforcers in an untenable position, erosive of public confidence. For as Reiss (1971) observed: "Selective enforcement of unpopular laws invariably is seen as the arbitrary exercise of authority [p. 176]."

Alcoholism and drug abuse are among modern society's most unyielding scourges. The ineffectiveness of criminal sanctions to control them suggests the superiority of the British approach of treating both conditions as essentially medical problems. Though hardly a panacea, this approach at least reduces the incentives to criminality and the threats to public safety now involved in supporting these habits as well as the breeding grounds of organized crime and corruption. Accordingly, many authorities support measures to decriminalize personal possession and use of drugs in reasonable quantities and to make access relatively easy under medical supervision, while imposing strict controls over supply (Morris & Hawkins, 1970). By like token, we should get tough on dangerous *behavior* such as drunken driving, as in Sweden; on possession and use of "Saturday Night Specials," which are far more dangerous to friend and family than to strangers; and on tempters, such as drug pushers.

Thus, decriminalization by no means embodies permissiveness or removes these social problems from legal control (Morris, 1973). Still less does decriminalization imply that criminal sanctions are inherently futile. On the contrary, while the deterring effects of criminal penalties are poorly understood and debatable, the Internal Revenue Service has demonstrated the potency of selective prosecution as a deterrent in taxation; and recent research supports the old notion that certain punishment is a stronger deterrent to crime than severe penalties (Wilson, 1973). Rather, the priority of public safety simply means that we need to be more discriminating in the use of criminal sanctions. Americans have long cheapened the concept of crime by overuse and underenforcement (Hart, 1958; Packer, 1968).

In tandem with decriminalization should go redistribution of resources currently being expended for law enforcement. The prime targets should be the police and corrections. We ask of the police decisional competence and responsibility comparing to the greatest of our professions, yet pay them a pittance after giving them inferior training. One of the saddest statistics in the President's Commission report of 1967 was that the average training time of police in cities with a population under 250,000 was less than three weeks. Hence, we should support efforts to professionalize the police, knowing full well that there will be unanticipated consequences, such as increased unionism and involvement in municipal politics to define the rules affecting police. At the same time, the police should be removed from nonessential functions like directing traffic, which the Danes found women perform better than men and with greater civility.

Corrections are commonly viewed as the chief failure of American criminal justice. High recidivism rates and prison uprisings have eroded public faith in the rehabilitative ideal. While reform of the human animal has never been an object to inspire much optimism, there is no reason to retreat from the goal of correction as long as we recognize its limitations. Rehabilitation is not the only aim of the criminal law, nor has it always failed. What fails are assumptions that the only fit punishment for crime is jail on a massive scale. Over 1.3 million convicts are now in the correctional process at an annual cost of over $1.5 billion dollars. One can support a student in an Ivy League college annually for the cost of maintaining an inmate in prison. Bail reform alone promises to save many millions of dollars now spent on feeding and housing prisoners in overcrowded jails. Over half of the local jail population merely await trial. Many will not be convicted. No attempt is made at corrections. If anything, incarceration amounts to little more than cram courses in crime.

It is small wonder that rehabilitation appears to be such a failure. Seldom has it been seriously tried. Despite some suggestions in the literature that the end result is the same no matter the type of institution, controlled experiments here and abroad, especially by the California Youth Authority, suggest a governing principle that the more we rely on family, group, and community ties the greater the chances of success (Morris & Hawkins, 1970). This principle, embodied in the American Law Institute's proposed Model Penal Code, means that social isolation should be the last resort rather than the first. Imprisonment should be limited to what is necessary for public protection and corrections. For those who do not constitute a threat to others or themselves, we should aim at swifter, more certain punishments, such as restitution, that avoid social ostracism. For those who must be detained, the rhetoric of rehabilitation should be taken seriously by programs, above all in smaller institutions, that stress education, job training, and counseling along with gradual reintroduction into community life via work release programs, furloughs, and halfway houses, practices similar to those now fashionable in the field of mental health. No illusions should be harbored about the outcome or about public support of community-based reforms. Still, more humane ways hold promise as cheaper and more effective methods of meeting what Chief Justice Burger called our "collective moral responsibility" to help the nation's half-million prisoners become more productive members of American society (*Baltimore Sun*, December 8, 1971).

Finally, redirection of priorities and resources also should be on the agenda of psychologists and psychiatrists. As an outsider whose knowledge is at best secondhand, I state this conclusion humbly. But the impression is hard to escape that scientists of the mind do not play major roles in the processes of criminal justice. Strained relations among lawyers and behavioral scientists, of course, are neither new nor surprising when we consider that their concepts and methods "share about the same degree of ideological kinship as does the Mafia and the

Women's Christian Temperance Union . . . [Allen, Ferster, & Rubin, 1968, p. ix]."

However, part of the difficulty lies in the direction of psychiatric energies. Forensic psychiatry has focused primarily on problems of trial, for example, perception and evidence or the insanity defense, highlighting the irrationality of the legal model of rational man. These are important, fascinating issues in which morals, science, and law connect. Yet, from a systemic standpoint, psychiatric efforts seem misdirected and marginal. For trials, as we have seen, are but the tip of the iceberg. And trials, as Karl Menninger and others have long argued, do not center on the critical, unsolved problems of how to identify potentially dangerous offenders and how to treat those who are psychologically disturbed (Menninger, 1968; Morris & Hawkins, 1970). These are the jugular questions on which progressive programs of pretrial and postconviction release depend.

A strong case exists for shifting the emphasis of behavioral scientists away from trial and toward pretrial and posttrial stages of the criminal process, where the action is. For psychiatrists, this means less emphasis on diagnosis and more emphasis on treatment. One need not banish psychiatrists from the courtroom to see merit in proposals to abolish the insanity defense as a distraction that breeds discrimination among defendants and only marginally defines the pathological basis of crime (Halleck, 1966; Morris & Hawkins, 1970). Resources thus saved could be far better spent during sentencing and in postconviction treatment of mentally disabled criminals (Lindman & McIntyre, 1961).

Morris and Hawkins (1970) also made an appealing recommendation that, given the frequency of guilty pleas and the failures of detention, psychologists should place top priority on research to identify socially dangerous offenders. Every official from patrolman to parole officer must make such predictions now. Cooptation of psychiatrists and psychologists as adversaries at trial has shaken confidence in the relevance of individual case histories and clinical insights to those judgments. What we need, in conjunction with clinical analysis, are statistical profiles and probability tables, based on experience in various types of crime, of potentially dangerous defendants so that responsible authorities can weigh the risks of release before and after sentencing. However these proposals for aggregate data may jar clinical traditions, neither judges nor the public are likely to accept community-based release programs without harder evidence than they now receive of the risks to public safety. Peeping Toms, for instance, seldom have courage to commit serious sex offenses. How much effort has been expanded in predicting more dangerous forms of criminal conduct than the relatively insignificant threat of potential skyjackers?

For law-related disciplines, as for courts and police, the conclusion is thus the same. We need a more discriminating allocation of tasks and resources among the various elements of the legal process, pending fundamental attacks on what we presume, without really knowing, are the two chief causes of urban crime: the lack of social justice in cities and conditions in American prisons. Reasonable men will differ about these suggested remedies, most of which have been around for years, but *something can be done*. The nub of the problem is less imagination than will.

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Harm, Offense, and Nuisance

Some First Steps in the Establishment of an Ethics of Treatment

DANIEL N. ROBINSON *Georgetown University* [1]

In a recent article (Robinson, 1973) I attempted to outline certain tensions and contradictions between current (and imminent) psychological therapies and principles of constitutional law. The purpose of the earlier article was to encourage analysis of the problem, and, out of a desire not to foreclose the necessary debate, I avoided presenting any specific recommendations. This gave the article a certain hit-and-run flavor which I hope to repair in the present article.

Because the domain of *ethical practices* is a vast one, it is important at the outset to establish the limits of the present discourse: I am concerned here only with the principles that guide therapeutic efforts directed at individuals who either have not consented to be treated or who, by virtue of their state or condition, are not in a position to engage in those rational operations required for a meaningful and valid consent. This is not to say that what follows does not apply to voluntary therapeutic associations, only that it does not *necessarily* apply. In further delimiting the sphere of concern, the present article takes as *given* that some degree of coercion is inescapable in a free society, that no right is absolute, that the State has legitimate interests which it is duty bound to hold in trust for the people, and that these interests can and do conflict with the will of individuals. A brief but remarkably complete evaluation of these as "givens" is offered by Feinberg (1973). I will depend heavily on his development of the grounds for coercion in the following section.

[1] I wish to acknowledge the critical reading of this manuscript by H. Arkes, M. Beauchamp, T. Beauchamp, J. Cortes, N. Finkel, J. Notterman, and R. Ruskin.

Requests for reprints should be sent to Daniel N. Robinson, Department of Psychology, Georgetown University, Washington, D.C. 20007.

The Ethics of Coercion

Feinberg (1973, p. 33) summarized traditional justifications for coercion under seven principles:

1. *Private harm.* In this instance, the State (or, really, any agent) may justifiably limit the freedom of an individual when the failure to do so will cause real harm to that individual, for example, the forcible prevention of suicide.

2. *Public harm.* Here, the individual is justifiably restrained from actions that will harm those institutions of governance in which all the people have a just interest. Forcible prevention of the disclosure of State secrets in time of war would come under this rubric.

3. *Legal paternalism.* This principle is rooted in the conviction that the State has a just interest in each of its citizens and may (must) therefore prevent them from doing injury to themselves. This is logically tied to Principles 1 and 2.

4. *Offense.* Unlike John Stuart Mill, who insisted that society need only remain "aloof" from those who offend it, those who support the offense principle insist that society has the right to legislate against actions that offend the sensibilities of the larger community, for example, laws against public lewdness.

5. *Legal moralism.* There are cogent arguments which insist that the notion of civilized society is an empty one unless it is granted that certain moral precepts are common to its members. To this extent, the State may justly prevent actions that ignore or defy moral convention. To remove this principle from that puritanical context for which modern society has such little patience, I offer the torture of animals as an example of the application of legal moralism. The torture of animals involves no harm to human beings; it offends no one if practiced privately. Yet, it is judged to be un-

conscionable by the public and it is outlawed by the State.

6. *Extreme paternalism.* This principle was most fully articulated by Plato in *Laws* and *Republic*. According to it, the State must guide the moral development of its citizens, must save them from the corruptions that are intrinsic to the masses. It is based on the belief that the public at large lacks the discipline, motivation, and talent necessary to protect its own interests, even to *learn* its own interests. Arguments for a philosopher-king, for a benign dictator, for the State as supra-public all rest on this principle.

7. *Welfare.* This is the other side of Principle 6. According to it, the public is to reveal its wants and the State is to operate in such a way as to "optimize" this public good. This principle is the cornerstone of utilitarian ethics.

None of these justifications for coercion is devoid of merit nor is it necessary that any one of them exclude the others in attempts to justify actions against the freedoms of an individual. A policeman may prevent a man from jumping from a window because such an action will harm the individual (Principle 1), because it is in violation of the laws of the State and therefore threatens the authority of its legal institutions (Principle 2), because he is sworn to protect life (Principle 3), because he—the policeman—finds the act personally offensive (Principle 4), because the law judges suicide to be immoral (Principle 5), because the man at the window does not know what is good for him (Principle 6), and/or because the State is charged with the task of securing a happy and prosperous life for each of its citizens (Principle 7).

It is one thing to assert that each of these justifications enjoys some merit but quite another to suggest that they are all equally valid. And it is manifestly the case that they do not share equally in the force of law. Yet, while not sharing *equally*, they have all, on one occasion or another, been relied on to validate a legal judgment. Important decisions in free-speech cases point this out convincingly. In the landmark case of *Schenck v. United States*,[2] Justice Holmes, speaking for a unanimous Court, delivered the now-famous "clear and present danger" criterion. While it reversed a lower court's conviction, the *Schenck* decision clearly removed free speech from the category of absolute right. Indeed, in *Whitney v. California*,[3] Justice Sanford, in delivering the opinion of the Court, removed all doubt: "The freedom of speech which is secured by the Constitution does not confer an absolute right to speak, without responsibility, whatever one may choose."

In *Schenck* and in *Whitney* (the Court affirming the convictions in both of these cases), the relevant issue was that of clear and present danger, whether the spoken or written material constituted a threat to the public order, to the safety of citizens, or to the security of the United States. But over the years, the Court has broadened the contexts in which free speech might be legitimately suppressed and has even included "symbolic" speech, as in the case of *United States v. O'Brien*[4] where the burning of draft cards was judged to be unprotected by the First Amendment. Presenting the decision of the Court, Chief Justice Warren offered these portentous words:

> A government regulation is sufficiently justified if it is within the constitutional power of the government; if it furthers an important or substantial governmental interest; if the governmental interest is unrelated to the suppression of free expression, and if the incidental restriction on alleged First Amendment freedom is no greater than is essential to the furtherance of that interest.

From *Schenck* and *Whitney* to *O'Brien*, there has been a subtle but continuous liberalization of the restraints on the individual states in their regulation of speech. In some respects, the midpoint of this development is found in *Beauharnais v. Illinois*,[5] in which the Court upheld the right of Illinois to prevent the distribution of racist pamphlets and to prosecute those responsible for such distribution. While avoiding the question of clear and present danger and relying instead on the fact that the pamphlets were libelous, the Court acknowledged the connection between the *offense* and *public harm* principles; that is, certain expressions of free speech were so *offensive* as to be conducive to civil strife. Indeed, while settling the matter on the basis of libel rather than clear and present danger, the Court, through Justice Frankfurter, insisted that "no one would contend that obscene speech, for example, may be punished only upon a showing of such circumstances." (The "such circumstances" being clear and present danger.)

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[2] Schenck v. United States, 249 U.S., 47 S.Ct. 247, 63 (1919).

[3] Whitney v. California, 247 U.S. 357, 47 S.Ct. 641, 7 L.Ed. 1095 (1927).

[4] United States v. O'Brien, 391 U.S. 367, 88 S.Ct. 1673, 20 L.Ed. 2nd 672 (1968).

[5] Beauharnais v. Illinois, 343 U.S. 250, 72 S.Ct. 725, 96 L.Ed. 919 (1952).

Implications

I cite several of these First Amendment cases not only because they involve what are generally recognized as our least violable rights but because they describe the gradual transfer of legitimacy from *harm principle* justifications to *offense principle* justifications, and this transfer must have implications for those engaged in rendering therapy to involuntary patients. The clearest implication is this: If the State is now less restrained in abridging certain forms of expression, it is, perhaps, commensurately freer in assigning those found guilty of illegal forms of expression to therapeutic contexts. I insert the "perhaps" because the Court has yet to establish the boundaries of therapeutic intervention. There are, however, three decisions that would appear to bear heavily on any future judgments the Court may make in this area and it is profitable to review them. Each centers either on the Fifth Amendment's protection against self-incrimination or the Fourteenth Amendment's guarantee of due process, or both. (Over the years some Justices have interpreted the Fourteenth Amendment as but a shorthand form of the first eight amendments; others have proffered upon it a unique set of protections.)

The Case of Gerald Gault

Gerald Gault [6] was a 15-year-old boy who had been placed in a detention home pursuant to a neighbor's complaint that he had been making obscene telephone calls to her. Committed as a juvenile delinquent (i.e., on noncriminal charges), Gerald was ordered to spend six years in the home; the time required for him to become legally responsible. Under criminal statute, had Gerald been 18 the penalty would have been no more than two months in jail and a fine not exceeding $50.

I need not go into the myriad bases on which the Supreme Court overturned the ruling of the juvenile court. Gerald and his parents had been denied nearly every element of legal consideration entailed in the concept of due process. It is sufficient for the present purpose to examine part of the decision as read by Justice Fortas:

> A boy is charged with misconduct. The boy is committed to an institution where he may be restrained of liberty for years. It is of no constitutional consequence—and of limited practical meaning—that the institution to which he is committed is called an Industrial School. The fact of the matter is that, however euphemistic the title, a "receiving home" or an "industrial school" for juveniles is an institution of confinement. . . . Under our Constitution, the condition of being a boy does not justify a kangaroo court. . . . Against the application to juveniles of the right to silence, it is argued that juvenile proceedings are "civil" and not "criminal" . . . [but] . . . the availability of the privilege does not turn on the type of proceeding . . . but upon the nature of the statement or admission and the exposure it invites.

Justice Fortas then addressed himself to the assertion that Gerald Gault had been consigned to a home and not a prison:

> Proceedings to determine "delinquency," which may lead to commitment to a state institution, must be regarded as "criminal" for purposes of the privilege against self-incrimination.

Concurring with the majority, Justice Black added this:

> Where a person, infant *or adult*, can be seized by the state to be confined for six years, I think the Constitution requires that he be tried in accordance with all the guarantees of all the Bill of Rights . . . [italics added].

If we substitute "mentally ill" or "psychologically disturbed" or "antisocial" or "infirm" for "boy" and "juvenile," we gain some sense of where the Court stands on several aspects of psychological therapies. It would seem, for example, to follow necessarily from the decision in *In re Gault* that the fact that someone is not being "tried" on "criminal" charges for purposes of "imprisonment" has nothing to do with his rights against self-incrimination and of due process. If the possible outcome of the enterprise in which he is involved is involuntary incarceration in any place "*however euphemistic the title,*" he cannot be compelled or tricked or confused into testifying against himself. Whether psychological tests ordered by a court fall into this domain remains to be decided, as does the status of expert testimony when based on information volunteered by an individual during psychological or psychiatric interviews. In this matter of self-incrimination, the two following cases are of moment.

Rochin v. California

Mr. Rochin [7] was suspected by the police of selling drugs. One morning, the police entered his quarters without warrant or apprisal and found him sitting with his wife. On a table next to the bed were two

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[6] In re Gault, 387 U.S. 1, 87 S.Ct .1428, 18 L.Ed. 2nd 527 (1967).

[7] Rochin v. California, 342 U.S. 165, 72 S.Ct. 205, 96 L.Ed. 183 (1952).

pills which Rochin grabbed and swallowed before the police could intervene. Unable to force the pills out of Rochin, the police rushed him to the hospital and ordered an attending physician to administer a gastric lavage. One product of this medical procedure was the two pills which, on subsequent analysis, were found to be morphine.

The Court reversed the previous conviction of Rochin and in concurring with the opinion as put forth by Justice Frankfurter, Justice Black added these words:

> [A] person is compelled to be a witness against himself not only when he is compelled to testify, but also when as here, incriminating evidence is forcibly taken from him by a contrivance of modern science.

However, that the matter is far from settled is evidenced in the more recent (1966) decision in *Schmerber v. California*.[8] In this case, a conviction was gained against Schmerber as a result of an analysis of his blood following an automobile accident. Speaking for the majority, Justice Brennan refused to overturn this conviction, in part, because "the test chosen . . . was a reasonable one . . . and performed in a reasonable manner." In this case, where the brutality of the *Rochin* case is not present, the State's rights in criminal matters are held higher than the individual's. In regard to *Schmerber*, the test followed the ascertainment of "probable cause" (i.e., there was an accident involving reckless and, therefore, perhaps, *drunken* driving), and this was therefore not in violation of protections against unlawful searches and seizures. Perplexingly, the majority did not find that the extraction of blood constituted the involuntary according of self-incriminating testimony.

From the foregoing, it would seem that (*a*) rather broad discretionary rights are available to the State in obtaining the kind of evidence necessary for convictions when this evidence is of a nonverbal variety—this may well include the evidence implicit in psychological tests and (even) interviews where the latter lead to interpretations of "illness" rather than proof of criminal intent or action—but that (*b*) the overall process involved in obtaining this evidence is protected constitutionally even though criminal prosecution is not involved, that is, even if the gravest consequence is (involuntary) hospitalization.

Because it is clear from the foregoing that the rights of the patient may not be as faithfully or fully protected as the given therapist would wish, it is necessary to explore what is *right* (ethically) and not just *legal* (adjudicatively). This exploration brings us back to the principles of coercion borrowed from Feinberg (1973).

Therapeutic Intervention and the Principle of Private Harm

Society does not derive its right to prevent harm to itself or its members from the necessities of logic but from the necessities of civilization. One need not subscribe to the rigorous *legal* and *moral paternalism* of Plato's Spartan model of the State to accept the right of a majority to live under law, unmolested by their neighbors, and protected against the villainies perpetrated by the ignorant and the diseased. Even Rousseau (1960), who was as far from the idea of Spartan order as Western philosophers have been wont to move, treated some actions as those of "a rebel and a traitor to his country" who, when imprisoned or put to death, suffers "as an enemy rather than a citizen . . . and, in such a case, the right of war involves the killing of the vanquished [p. 200]." Quite simply, the *sine qua non* of that condition of life we call "civilized" is the rule of law over the rule of passion. It would seem, therefore, that individuals presented for treatment—for therapy, behavior modification, etc.—for reasons of having physically harmed others can lay no moral claim on the right not to be changed. Whatever evidence is marshaled legally by the state preliminary to their "sentencing" is evidence, *prima facie*, that just cause for therapy exists. It is my view (and here I answer to the criticism that my earlier article was insufficiently proscriptive) that proof of physical harm, intentionally perpetrated on one's self or on another, constitutes a forfeiture of what, earlier, I called "the right to be different" (Robinson, 1973). This, however, is limited to private harm.[9]

Therapeutic Intervention and the Principle of Public Harm

Public harm, while potentially more injurious to more people, presents a different case for the therapist. We are no longer dealing with a person who

[8] Schmerber v. California, 384 U.S. 757, 86 S.Ct. 1826, 16 L.Ed., 2nd 908 (1966).

[9] I employ *private harm* in the rigorous sense of acts now proscribed by laws against homicide and suicide. Thus, the alcoholic or the smoker, who might do himself personal harm, is readily excluded.

physically jeopardizes his own life or that of another. Rather, he conspires, through speech, gesture, and seditious conduct, to undermine the institutions of governance. His actions are against the State, are politically motivated, and, while perhaps "deluded," are not necessarily "crimes of passion." The duly constituted authorities may have every good and legal right to arrest this person and to imprison him. Indeed, those who, during World Wars I and II, were in sympathy with Germany, who conspired to steal secrets of state, and who attempted to dissuade young men from enlisting or serving in the armed forces were arrested and were imprisoned under laws deriving their force from the principle of public harm. But suppose the psychological disciplines had effective therapeutic methods by which the political convictions of these individuals could be transformed. Suppose it had been possible, through drugs, surgery, or conditioning, to cause them to desist from seditious and treasonous actions. Would the application of these methods, even if *legal*, be *right?* I submit that such applications would not be right and that the domain of treatment and healing is in no way connected by allegiance to any particular State in which it may be temporarily located or exercised. The domain of treatment and healing exists for the benefit of those who voluntarily solicit its remedies and not for the benefit of any particular ideology, no matter how congenial that ideology might be to the goals of the therapist. Indeed, even in the instance of the private harm principle, compulsory therapy is offered as an "evil" of lesser gravity than the ultimate evil of death or life in prison. It is in this sense that the therapist strives to save the individual from the worst act the state may execute against him, and it balances a reluctance to intervene against the harm the individual might *personally* do to others.

Therapeutic Intervention and the Remaining Principles of Coercion

Since I have drawn the line separating warranted and unwarranted therapeutic intervention just under the principle of private harm, it necessarily follows that intervention for "offense" and for "nuisance" is proscribed. Society, through its laws and its immense power of persuasion, would seem to have all the influence it needs to spare itself the embarrassments of "offensive" actions and the inconveniences promoted by public "nuisances." The individual who would rather be treated for his homosexuality, alcoholism, exhibitionism, etc., than imprisoned for these "compulsions," should be given the option. But in instances of victimless crime, there would seem to be no ethical or principled basis on which the therapist must foist his treatments on an unwilling patient. That examples can be culled from the vast region of possibility which show that involuntary therapy would be better than a therapeutic laissez-faire is merely to prove that life in a free society is seldom ideal. In instances of victimless crime or of crimes that involve merely an offense to sensibility, the Court has held that the State may arrest and punish the perpetrators. But, to state it still another way, every punishable act does not constitute a justification for involuntary therapy. Transferring moral paternalism from the legislature to the counseling room (or surgical theater) can have only monstrous consequences. It is in this connection that we must distinguish between the paternalism exercised in the behalf of children and that which treats adults as if they were children.

The State requires that children receive some fixed number of years of formal education to prepare them for the responsibilities of citizenship. Children are required to be educated not because they are "sick" and because knowledge is "therapeutic" but because they are uninformed. Indeed, physicians are also required to receive education for licensure and this is not because they are "sick." Therapy, no matter how we debate it on theoretical grounds, is legally distinguishable from education, and adults are similarly distinguishable from children. The illiterate child cannot participate in a meaningful way in representative government; he cannot comprehend those details of political action that bear upon his rights and opportunities. The inveterate homosexual, however, may be a model citizen; he may be intelligent, hard working, politically conscientious. That his conduct is "maladaptive," that society may chastise him, or that the police might arrest him are possibilities he must weigh. If, according to the view of some, he is "sick," they have every right to offer him their evidence, as does the Surgeon General in the matter of tobacco. Whether and how he chooses to act upon this advice is for him to decide, even if his decision leads to imprisonment. Laws and conventions are more easily changed than is one's personality once effectively treated by the arsenal of therapeutic modalities. For the therapist who is unwilling to draw the line at the private harm principle, for the therapist who is willing to

treat those involuntarily assigned to him on the basis of offensiveness or because they are "nuisances," for this therapist the pool of potential patients is large and varied.

If Illinois can prosecute Beauharnais for the lamentable villifications contained in his pamphlet, an Illinois judge can well decide that Beauharnais needs "help" and many would concur. But aspersions just as libelous and tawdry were shouted and written by extreme student radicals throughout the 1960s and were directed at *specific* groups also. In the last analysis, is the therapist's unwillingness to reform the behavior of the involuntary patient to be based on the sympathy that *particular* therapist has for the *particular* libels, slanders, and degradations heaped on some isolated group by that *particular* "patient"? *Mein Kampf* was authored in a prison cell and there are few who do not rue the day it found its way to the printer's press. Many would now submit that *any* therapeutic procedure would be justified in keeping such ideas from the public domain. But if this is the popular will, it can be expressed in *law*. Behavioral and medical scientists have no role in arbitrating the competing claims between individuals and society in general. Our charge is to inform, not to transform, our fellow citizens unless they seek such help with the full knowledge of the consequences attending their receiving it.[10] Since the popular will can change the laws but not the therapies, protections against the latter must be drawn more sharply. Until the courts get around to doing it, practitioners must exercise informed restraint. At the present time, this restraint should begin with the refusal to administer compulsory therapy in all cases not embraced by the principle of *private harm*. Even in those instances in which private harm is involved, there are limits to the nature and extent of involuntary therapeutic intervention. These, at least provisionally, are best established by balancing the potential harms of treatment against the real damages contained in the Court's sentence.

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[10] I recognize the enormity of the issue of consent. In this article I have evaded it by stipulation, that is, by discussing only those instances in which the patient validly withholds consent or in which his consent is suspect.

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Controversy Concerning the Criminal Justice System and Its Implications for the Role of Mental Health Workers

DAVID E. SILBER *Institute of Criminology, Hebrew University of Jerusalem* [1]

The old debate over how to view criminal offenders, always lively and occasionally acrimonious, has taken a new turn in the past decade or so. The argument used to be over whether criminals were sick (and thus ought to be treated instead of punished) or bad. The new dimension is concerned with the consequences of a psychiatric conception: (*a*) Is it an encroachment of civil liberties? and (*b*) Is the treatment conception psychologically sound? The issues are complicated and interlocked, emotionally charged, and important to society. Before going to the question of treatment and roles for treatment workers, it would be worthwhile to review the arguments; what follows is an attempt to separate the main hypotheses of both camps.

Criminal Behavior Is Symptomatic of Psychological Disturbance

This *was* the enlightened liberal position. Many psychiatric writers (Abrahamsen, 1967; Alexander & Healy, 1935; Alexander & Staub, 1956; Halleck, 1967; and Lindner, 1946; among others) have characterized criminals as having the same traumatic developmental difficulties, leading to similar deficient personalities, as neurotic individuals. Often the explanations hypothesize the same psychosexual connections between adult criminal behavior and infantile fixations as are assumed to operate in the formation of neurotic behaviors: "We direct our regard back to the fog-enshrouded days of infancy and early childhood, when were laid down the irrevocable patterns of our lives [Lindner, 1946, p. 58]." The most passionate advocate of the psychoanalytic–psychodynamic interpretation of criminals as "mentally ill" is probably Menninger (1968). Menninger argued that society's response to criminals (as evil and immoral persons) is as misguided as earlier notions that mental illness is a sign of satanic possession.

Most interpretations of criminal behavior as disturbed are made in a reformist spirit, usually coupled with appeals to radically change the criminal justice system. Proposals usually include changing the court system, changing the role of mental health worker, and transforming the prison system: "We [the public] commit the crime of damning some of our fellow citizens with the label 'criminal' and having done this, we force them through an experience that is soul searing and dehumanizing [Menninger, 1968, p. 9]." Their alternative proposals may be summarized as follows:

1. *Precriminal detection.* Delinquency-prone youths should be detected prior to contact with the police, to allow preventative measures (unspecified) to be instituted. Presumably this might entail enforced treatment for uncooperative juveniles and/or their families.

2. *The substitution, in most cases, of informal nonadversary proceedings (either civil or criminal) for the formal adversary proceedings now characteristic of criminal trials.* Others (e.g., Glueck, 1962) have argued for some informal method of determining guilt or innocence that would not take *mens rea* ("evil intent") into account, in much the same way that juvenile court is supposed to work.

3. *The substitution of indefinite sentences* [2] *for fixed or indeterminate sentences.* Along with an

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[1] Requests for reprints should be sent to David E. Silber, Department of Psychology, George Washington University, Washington, D.C. 20006.

[2] The indefinite sentence lasts until the person is pronounced "cured," as in the case of civil commitments to a mental hospital. The indeterminate sentence is a penalty sentence, usually with a maximum and minimum time specified.

indefinite sentence, the person would be remanded to a treatment center rather than to prison for incarceration. Institutionalization would be mandatory, but the aim would be correction of psychological disturbance and social rehabilitation.

4. *Treatment.* Psychotherapy in one form or another would be widely used, along with environmental manipulation, guidance, and education. It is recognized that some persons might have to remain in such confinement programs on a more or less permanent basis. Interestingly, most commentators do not mention using the newer behavior modification techniques in their proposals for such treatment centers, although psychologists in prison settings often do (Brodsky, 1972).

In short, the general purpose of these proposals would be to provide settings where the man, rather than the deed, is adjudicated and treated.

The New Opposition

To a growing body of civil libertarians, these proposals are appalling and pose a challenge to personal freedom occasioned by the growth of what they call the "Therapeutic State." Kittrie (1971) has admirably clarified the threats to traditional legal safeguards posed by nonadversary civil commitment proceedings. Szasz (1963) has attacked the intrusion of psychiatric thought into the legal arena as a curtailment of liberty. Treatment in specialized corrective facilities and mental hospitals has been attacked by Spece (1972) on the same grounds. In the process, the concept of the mental health worker as a helper of persons takes a dreadful beating. We come off, instead, as wanting a piece of the legal action (Christie, 1971), ill-informed and judgmental (Szasz, 1963), sloppy in thought and hasty in action (Kittrie, 1971), or unreflective and mildly sadistic (Spece, 1972). It would be wise to take close heed of the main points raised by these writers.

1. Informal, civil proceedings jeopardize a person's basic rights ordinarily guaranteed under criminal law. Such rights include the right to counsel, specified indictment, time to prepare a defense, trial by jury, be present during the proceedings, confront adversary witnesses directly, and be judged solely on the basis of innocence or guilt.

2. Civil commitment for indefinite periods is a judgment against the person, not the deed, and as such represents a step toward totalitarian social control. Indefinite sentences also unfairly penalize the person accused of crimes which normally carry light sentences.

3. Once committed, a person loses all civil liberties and is even more vulnerable to enforced treatment and mistreatment than the incarcerated prisoners.

4. Psychiatry and clinical psychology contain inconsistent, contradictory views of human functioning, so that any psychiatric stance in a legal proceeding is capable (in good faith) of being opposed by another psychiatric stance.

5. Treatment programs are widely carried out without theoretical rationale, or solid empirical support. Little is known concerning the relative efficacy of various treatment programs, if, indeed, they even work. An especially problematic area is physical intrusion into the body as a way of changing behavior, such as occurs with electroconvulsive shock treatment, electrical stimulation of the brain, injections of noxious substances during aversive conditioning, and psychosurgery of one kind or another.

6. "Mental health" concepts, as uncritically applied to convicted persons, are not based on medical considerations, but rather on social and ethical value systems.

7. Coercive treatment programs stand little chance of success precisely because they are coercive.

8. Psychiatrically oriented institutionalized treatment programs extant have failed dismally, yet are "total institutions," as are prisons, and are often even more degrading and dehumanizing than prisons. The social stigma of the label *mentally ill* is at least as bad and enduring as being labeled *criminal.*

These objections leave unsolved the problem of what to do with and for the convicted offender so as to provide correctional rather than retributive experiences during imprisonment. And they completely ignore the question of whether the individual criminal is in a position to perceive what is best for himself.

An indirect result of these arguments, however, is that they make it appear that treatment and therapy are the order of the day in correctional facilities. Professionals and the general public alike tend to believe, as does Antilla (1972), that "treatment ideology has achieved a kind of breakthrough within the last few decades [p. 287]." Bazelon (1972) has criticized psychologists for failing to change the majority of offenders, after making the explicit assumption that "the experts—

you—are minding the store [p. 151]." This is not a fair attack because it is not true. Mental health workers in the adult correctional system are few and far between, and treatment is the exception, rather than the rule. The rest of this article will examine what ought to be the role of the mental health worker in the criminal justice system, what activities might be expanded, and what avenues for staffing might prove fruitful.

Roles for Mental Health Workers in the Criminal Justice System

In view of the foregoing, it is quite clear that mental health workers in the criminal justice system must examine and question what their role ought to be. This section proposes limits and reiterates some areas where increased efforts and programs can be developed.

First, mental health workers should become very leery about testifying in court. Almost all writers, including Menninger, have complained about psychiatric testimony, yet psychologists and psychiatrists continue to march to the witness box with increasing frequency. Interestingly, the pressure may come more from the prosecution than the defense. Kittrie (1971) quoted a study done in the criminal courts of the District of Columbia which found this to be the case. In such instances, where psychiatric workers are directed to testify, reports ought to be limited to the current mental status of the defendant. If the defendant is suspected of being acutely psychotic (and thus unable to participate in his own defense), the determination ought to follow from a formal civil commitment procedure. In other situations, the mental health worker ought to avoid doing what he patently cannot: that is, render a post hoc judgment of psychological status. Since the major grounds of exculpation are now via the M'Naughton rule, evidence in support of it should come from people who observed the defendant at the time of the offense. To qualify as a defense under M'Naughton, a person would have to be so overtly psychotic as to appear so even to untrained observers. Another reason for allowing adjudication to follow from legal considerations has been stressed by Rubin (1965): Treatment can be as meaningful and successful in a prison facility as a mental hospital or special treatment program. Mental hospitals have the wrong "set" for treating felons; they perceive the patient as helpless and disturbed. Most criminals are not out of contact with reality, but rather are deviant in social values and behaviors.

Similarly, mental health workers ought to invoke extreme caution before suggesting mandatory treatment programs as substitutes for imprisonment. Removal from society is punishment, and mandatory treatment programs, with indefinite commitments, constitute potentially greater punishment to the individual than determinate imprisonment. Even the fact that such treatment programs would be controlled by the state health or social welfare agency is a questionable advantage, because abuses to patients could more easily masquerade as "treatment" and elude legal detection. An example of the dangers to civil liberty is the project outlined in the American Psychological Association *Monitor* of August 1972 ("Treatment Camps for Addicts"). The plan would call for mandatory treatment of addicts in special camps, in order to protect society from drug-occasioned crime and speed the addict's "recovery." There was no mention of how the incorrigible addict would be identified or adjudicated, what legal safeguards would be present during or after commitment proceedings, or what treatment modalities would be used. It is clear from the article that the recalcitrant or negative inmate would face long-term confinement indistinguishable from a prison setting except for the lack of an expiration date.

Research into effective methods of treatment ought to be encouraged, but the research personnel should be drawn from staff outside of the correctional setting. The mental health worker in the correctional institution ought to function as an ethical watchdog to discourage dangerous programs. There are enough innocuous techniques available which have not received adequate scrutiny so that such methods as, for example, aversive conditioning using anectine (Spece, 1972), can be eschewed for a very long time.

Finally, in deciding where the role of the mental health worker ought to be, it can be argued that prisons are not necessarily any more static than other institutions of society. In addition, correctional programs have the practical advantages of being already in existence, having avowedly rehabilitative goals, and having (in most states) a central coordinating office. In general, the mental health worker ought to stay out of the adjudication process and in the correctional system.[8]

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[8] There is a valuable service that mental health workers can render without infringing civil liberties: to protest against laws which define psychologically abnormal or unusual behavior as criminal, for example, laws against homosexual behavior between consenting adults.

Treatment in Correctional Settings

Most prisoners receive little or no systematic treatment of any kind. Even in the Lewisburg Federal Penitentiary, this has been found to be so (Rother & Ervin, 1971). There seems to be three major reasons for this: First, the staffs of most correctional facilities are woefully inadequate. In 1967, it was estimated that the ratio of professional staff to prisoners was 1:179 nationally (Task Force on Corrections, 1967). Smith (1971) quoted a survey that listed only 60 full-time psychiatrists for 230 adult correctional facilities in the United States. Clearly, the experts are not "minding the store."

Second, most prisoners do not manifest "traditional" forms of psychopathology. Using data quoted by Brodsky and Vandiver (Brodsky, 1972), Smith (1971), plus Brodsky's survey (1970), it is clear that the percentage of psychotic and neurotic individuals is quite low (ranges of 1%–3% and 4%–7%, respectively). The major diagnostic category was some variation of the character disorder/psychopathic personality label. Such prisoners are hostile, suspicious, and immature and tend to identify with subcultural values that are legally proscribed. They usually perceive their personality functioning to be acceptable. They are not, in general, likely to clamor for individual psychotherapy and usually are seen on referral. Thus, there is no strong sustained interest on the part of prisoners themselves for treatment.[4]

Finally, the custodial staffs of prisons usually view the professional staff with ambivalence or outright hostility. They worry about the possibility of riots, believe that prisoners are generally incorrigible, and suspect that therapy is a form of mollycoddling. Guards usually have little or no formal training in psychology or human relations, often fear and dislike the prisoners they are charged with guarding, and may resent the professional because of his higher status. They therefore are often reluctant to encourage the expansion or initiation of treatment programs within the correctional setting and may passively oppose ones already present.

Thus, it is one of life's little ironies that treatment is attacked because it does not help prisoners (Antilla, 1972; Bazelon, 1972), when in fact the prisoner's chance of receiving treatment is almost zero. Therapy has not been given the chance to work, and it ought to have that chance before being written off as worthless. Treatment has been identified with psychoanalytically oriented individual psychotherapy, an error that has gone largely unchallenged; obviously, there are more and newer techniques in use. Let me now suggest some specifics concerning treatment in corrections.

THE DIAGNOSTIC AND RECEPTION CENTER

Increasingly, prisoners are being sent first to these centers. To have an impact on the prison career of the inmate, diagnosis and classification should occur as soon as the offender is incarcerated. The same panoply of professionals as in a well-run mental hospital should have contact with the prisoner, and the results should be discussed at a case conference. Major decisions concerning diagnosis, therapy, and placement with the system would be made there, and a particular "package" tailored to the individual would be arrived at. The inmate should be given feedback; diagnosis is too often a one-way street. This is a particular danger in corrections, where hostility and suspicion are excessively present anyhow. The center staff ought to use the feedback technique to motivate the prisoner to get involved in his own rehabilitation. Enough general evidence exists to conclude that if a prisoner is treated as a dignified and reasonable human being, he will more often than not respond in a like manner. A similar procedure has been in operation in North Carolina, which seems to offer some sort of crisis intervention aid, such as found in a neighborhood community mental health center, for prisoners with acute disturbances.

THE TRADITIONAL PRISON SETTING

Within the penitentiary and reformatory, the mental health worker can be a treatment source and "system challenger" (Brodsky, 1972). As a system challenger, he ought to goad the administration to eliminate retrogressive and abusive regulations; as a positive contribution, he should intervene to work on the attitudes and behaviors of the custodial staff toward the offender. Such activities would include in-service training for current staff, human relations instructions to new guards, role-playing sessions, and psychological instruction. To explicate the viewpoint implied above: The mental health worker stands outside the institutional hierarchy. I would argue that the first concern of the therapist is the client, and by staying on the periphery the mental health worker can make his

[4] The opposite view is that prisoners recognize the low quality of treatment services and stay away for that reason (American Friends Service Committee, 1971).

contribution most effectively. This is neither "radical" nor antiestablishment, but simply the recognition that treatment works best when the therapist is not identified as a cog of the system by the inmate. Thus, activities such as sitting on disciplinary boards, writing parole recommendations, or acting as an information conduit to the administration are not properly within the treatment worker's purview.

The backbone of the mental health worker's activities ought to be as a purveyor of therapeutic services. The immediate questions that arise are, What sort of therapy and with whom? Usual guidelines applied to nonincarcerated patients are meaningless, given the behaviors and personality configurations of prisoners. Practical considerations dictate that the cheapest and most intensive methods be used most often, such as group therapy, behavioral techniques, and marathon therapy groups. The few published reports available suggest that intensive therapy can be effective (Sindhu, 1970), as can therapeutic community organization (Miles, 1969), marathon-type retreats (Carrol & McCormick, 1970), and behavior techniques and token economies (DeRisi, 1971; Wright, 1968). Experience suggests that intensive, Synanon-type marathon groups are particularly effective when embedded in the context of regular group sessions.

Simply reducing defenses or eliminating undesirable attitudes and symptoms is not enough, considering the environment to which the prisoner usually returns. Treatment legitimately includes teaching the prisoner ways of putting the institutions of society to work for him (a point suggested by Warden R. Williams, 1970, of the Maryland House of Corrections) and ways to either change or avoid negative environmental influences.

Ideally, the report of the diagnostic center will accompany the prisoner, so treatment—if desirable—can start immediately. If, however, we really take seriously the issue of individual civil rights, prisoners should not be coerced directly or indirectly into participating. Thoughtful explanations of method and desirability are indicated, especially in instances where noxious stimuli or stress methods are used (e.g., in Synanon-type encounters).

Mental health workers can serve as informal counselors to self-help groups such as Alcoholics Anonymous or the Black Muslims. Ethnic-pride groups such as the Black Muslims can have a profound effect on prisoners, and mental health workers should feel free to offer aid. There are two other activities that the treatment worker should do, but probably will not be allowed to do: (*a*) arrange human relations seminars between inmates and guards, and (*b*) arrange couples groups in the prison which include both inmates and their spouses (or fiancées).

PARTIAL-RELEASE AND POSTRELEASE SETTINGS

The need to confront the social realities and pressures on ex-offenders in their readjustment to street life marks the partial-release and postrelease efforts as perhaps the most important correctional and treatment settings. Two important innovations in the recent past have been the halfway house (usually in an urban setting) for nondangerous offenders and the work-release program (usually operated out of an honor camp). Counseling, and especially experientially oriented group therapy, can be crucial here in alleviating the great strain placed on someone who is *in* a free environment, but not *of* it. The therapist will have to handle complaints and frustrations, help control impulsiveness, defuse defenses, and serve as a stable model—all without appearing smug or saintly. Immediate family members can be brought into the treatment at this point, in combined family groups.

Currently there are almost no programs of postrelease treatment. This is extraordinarily unfortunate, as the first postrelease year is apparently the hardest; something on the order of 40% are rearrested within the first year (Federal Bureau of Investigation, 1970). Aftercare programs run by the correctional system should be quickly established, so that treatment can either be continued or be available if needed. I stress the need for these to be under the aegis of the correctional system because its professionals are probably most qualified to perceive the special problems of ex-offenders in making a satisfactory postrelease adjustment.

All of the foregoing makes no sense at all, given the current staff situation in the correctional system. In addition, it is no secret that many are incompletely trained or marking time until something better comes along. Recruitment is hampered because salaries are relatively low (Task Force on Corrections, 1967), prestige is low, and prisons are frequently away from the urban scene, where professionals like to locate. There are, however, a number of pragmatic, relatively inexpensive remedies available. Perhaps the single most neglected resource is the university-based professional training program. All programs in clinical psychology, social work, and psychiatry require significant

amounts of field experience. There is no reason why correctional facilities cannot function as training centers (as, for example, does Jackson State Prison in Michigan). State universities in particular ought to be approached, given their common funding source. To give the correctional-system professional more status as a supervisor, those who meet licensing requirements could be appointed as adjunct clinical staff (a current practice for hospital staff supervisors). An even more innovative plan is in practice in North Carolina, where mental hospital psychologists are hired by the University of North Carolina itself, then assigned to field placements in hospitals and clinics. Something like this could be done with correctional staff as well, thus providing them with perceived prestige, as well as resolving some staffing problems.

A second possibility is for departments of corrections to underwrite the cost of professional education in return for a commitment to work for a number of years following graduation. This system seems to work for the armed forces and the Veteran's Administration, many of whose career professionals were recruited that way. Within the system itself, employment would be more attractive if the duties of the mental health worker were split between various institutions. Or, if not practical, some sort of biyearly rotation between institutions could be offered. Finally, professionals could be enticed by the offer of five-fourths pay; that is, they would be allowed to take one full day per week for outside consulting while receiving full pay and benefits.

Interrupting the cycle of arrest, conviction, and rearrest is crucial; as New York City Police Commissioner Murphy (1972) said, the correctional systems must be changed and changed rapidly. Offering full mental health services at every stage of the prison experience—from sentencing to and after release—can help achieve this goal while offering satisfying roles for mental health professionals within the criminal justice system.

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Reflections on Introspection

JOHN RADFORD *North East London Polytechnic, England* [1]

Joynson (1970, 1972) has argued for the use of introspection in opposition to what he regards as the "generally accepted doctrine" (Broadbent, 1961) of objective experiment. This view "defines the subject matter of psychology as behaviour rather than mental life; it rejects the view that psychology investigates conscious experience through introspection [Joynson, 1972, p. 1]." Now this view, as Joynson allowed, has perhaps never been "generally" accepted, though it has been extremely influential. As an example of a phenomenon that ought to interest psychologists, but that might have no behavioral counterpart, C. A. Mace used to give an *unfulfilled intention:* "He thought of going out by one door, but actually went out of another." Mace (1965) observed that "psychology is regaining consciousness [p. 1]." Burt (1962, 1968) put a forceful case for the study of consciousness.

On the other hand, some writers, imbued with the "accepted doctrine," have gone to considerable lengths to incorporate some of the phenomena of consciousness within a respectable behaviorist framework. Hebb (1968) presented a somewhat tortuous argument with respect to mental imagery. He wished to "dispose of what seems to be a misconception, that reporting imagery, or describing it, is necessarily introspective [p. 466]." His argument is, first of all, that experiences may give rise to reactions: experiences, that is, in the sense of stimulus conditions that can be specified by an observer. Among the reactions may be vocal sounds or indeed verbal reports. But these are simply objectively recordable instances of behavior: "It is obvious that in such reactivity—when I burn my fingers and say 'Ouch'—no question of looking inward arises. My verbal response is no more dependent on introspection than a dog's yelp when his tail is trod on [p. 467]." The processes involved can, in principle, be reduced to a physiological chain of cause and effect. In the case of imagery, much the same chain is called into play, except that the initial stimulus is absent. To make his point, Hebb (1968) quoted the case of a *phantom limb:* Above the level of the amputation the same nervous mechanisms may operate as before the limb was lost. The same argument applies to *memory imagery:*

> Though there is now no sensory input, the same central process, more or less, is exciting the same motor response—more or less. . . . It is the same outward-looking mechanism that is operative, not introspection. . . . At least, it is not introspection in the sense of a special inward-looking mechanism of self-knowledge [p. 467].

This led Hebb to suggest that "the mechanism of imagery is an aberrant mechanism of exteroception." This is an odd view, for why should imagery be in any sense "aberrant"? Imagery is presumably a form of internal representation of the environment, and there is no reason to think such mechanisms anything other than natural. The view arises, however, from Hebb's determination to abide by what he regards as "the rules of objective psychology, in which mental processes are examined by inference, and not by direct observation [p. 468]." This is interesting, for Hebb seems here to adopt the exact opposite of the view of the classical introspectionists. "One does not perceive one's perceptions," said Hebb, "nor describe them; one describes the object that is perceived, from which one may draw inferences about the nature of the perceptual process [p. 467]." Now this view of perception itself can certainly be questioned. Piaget and Inhelder (1966), for example, held that even when one makes a copy of a drawing, it is not the drawing that is copied directly, but a mediating image of the drawing. However, even if the mechanism of perception were of a stimulus–response kind and even if the mechanism of imagery were similar, it would not follow that the reporting of the image was not introspective. It

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[1] This article is based on a section in *Thinking: Its Nature and Development,* to be published by John Wiley in 1974.

Requests for reprints should be sent to John Radford, Department of Psychology, North East London Polytechnic, Three Mills, Abbey Lane, London, E15, England.

might not be systematic, as in some of the classical studies (I suspect this is part of what Hebb meant by a "special" mechanism). And, of course, the *statements* about the image can be treated as behavior. But that is not the same thing as saying that no introspection is involved, as I hope to show.

Let me quote another attempt to bring some of these awkward phenomena within the behaviorist fold. Bourne, Ekstrand, and Dominowski (1971) presented a textbook, *The Psychology of Thinking*. But thinking must be treated in a certain way: "Thinking is purely and simply a behaviour concept . . . thinking is a simple noncommittal way to describe a behaviour which, like all behaviour, is completely observable [p. 7]." With this view, it is not surprising that introspection finds no place in the book, nor indeed that "thinking" is restricted to concepts, problems, and language. This view is interesting, because it is almost identical, in essence, with that of J. B. Watson. The latter, as is well known, wished to reduce thinking to subvocal language, that is, a particular aspect of observable behavior. It might be felt that in 50 years little has changed. All the behaviorists we have quoted have been happy to include speech in their objective studies, and Boring (1953) suggested that this meant that introspection was being allowed to carry on its business under a new name, that of *verbal reports*. But this, it seems to me, is unfair to the behaviorists.

It seems to me that we can distinguish between introspections as data and introspection as a method. By "data" I mean here the statements of introspections, not the objects of introspection. Introspections as data in this sense can, I think, always be treated as verbal reports, if that is what we wish to do. They can be recorded in the same way that we record "the subject pressed the lever," or "the subject scored x on the test." What is recorded is "the subject says that . . ." he has an image, a feeling, etc. Such records must, of course, be subject to the usual checks used for all experimental data and in particular must yield satisfactory levels of significance. It may also be argued that records of this type need a special sort of check, analogous to that used for personality questionnaire responses. Essentially, this is not to take the response at its face value, but to establish empirically its relationship to some other variable. This might be an aspect of the stimulus conditions, and/or other behavior.[2] Some examples are provided by Woodworth (1938). If this can be done then, it may be argued, statements of introspections can be considered acceptable in the same sense as other widely accepted sorts of psychological data.

But is there, in fact, no difference between Hebb saying "Ouch" and a dog yelping? The real issue must lie in whether there is another class of data, distinguished by being accessible only by a particular method, that of introspection. This question has a number of aspects. Philosophically, it is a version of the body–mind problem. Historically in the behavioral sciences, there has been the change from regarding consciousness as a reservoir of experience to be investigated directly, to regarding it as a construct, inferred from other observations (Boring, 1953). As we noted, that is Hebb's view. But there are also more strictly psychological aspects.

The first of these is that "the method of introspection" covers several different, though related, methods. McKellar (1962) offered a classification of these. He made six sorts of distinctions:

> We may first contrast the systematic methods of "classical introspection" with those of a relatively unsystematic kind. Secondly, the use of trained introspectionists differs from the work of the Gestalt psychologists and others who sought, by introspection, to study the phenomena of naive human experience. Thirdly, introspections may be classified by the circumstances in which they are obtained: the laboratory, the clinic, the analytic situation, and daily life. Fourthly, some introspection is carried out mainly for communication to the experimenter, while in other cases it is carried out partly or largely to extend the investigator's own empathic understanding. Fifthly, introspection under normal circumstances may be contrasted with the use of the method in special, experimentally produced circumstances: those produced by drugs, sensory deprivation, hypnosis, etc. Sixthly, we may contrast introspections made or elicited by trained psychologists from those made or elicited by others [p. 628].

This is the only classification of introspective methods I have come across in the recent literature. Clearly, McKellar's distinctions do exist, though perhaps they are not all of equal theoretical importance. For example, introspections when the subject is or is not under the influence of a drug would not appear to be different *methods*, though the *results* will very likely be different in the two cases—as would the results of any observation. It seems to me, considering the range of activities that have been referred to as "introspection," that three groups can be distinguished, in terms of what it is

[2] I owe this point to E. Valentine.

that the subject is to do. There are, first, cases of self-observation. The individual reports on his own experience: He aims to observe his own mental events, in the same way that an astronomer observes stars or a behaviorist observes responses. Second, there are self-reports: By this I mean cases in which the person tells of his experiences, but without trying to be objective—for example, experiences under unusual conditions. Third, there are cases of thinking aloud: the technique used, for example, in Duncker's (1945) well-known experiments on problem solving. These distinctions are not absolutely clear-cut, but they do, I think, refer to three different activities. Wundt distinguished between introspection and self-perception (Boring, 1953). By the latter I take it he meant what I have called self-reports. For introspection proper, trained observers were required. The distinctions are confused by several factors. First, all the activities can, in principle, be more or less systematic. That is to say, the experimenter (who may also be the observer) can exercise a greater or less degree of selection. This feature, however, has in practice been an issue mainly in the first case. Watt (1904; quoted by Humphrey, 1963) developed the method of fractionation in an attempt to get at thought processes by systematic self-observation. Second, the activities have in use sometimes been confounded. A good example here is psychoanalysis. The only point about the analytic use of introspection that is clear is that it comes at the extreme end of the nonselective range, because the prime rule for the patient is to hold nothing back. Now there is a sense in which psychoanalysis could be said to use what I have called thinking aloud: The patient simply says out loud what is passing through his head. He does not have to observe himself; that is the analyst's job. In this respect psychoanalysis takes on a curiously behavioristic air, for the analyst deals with what is objectively observable: the patient's behavior and speech. (A patient might make a remark and add: "I meant . . ."; and the analyst might reply: "Let us look, not at what you meant, but at what you said.") Since the rule is to hold nothing back, however, the patient will inevitably also engage in self-observation. And since the aim of analysis is to give the patient more control over his own behavior, he will, if the analysis is successful, become more objective about what he says (and does). He moves, in a sense, from being subject to being experimenter. The analyst *also* makes inferences about the patient's internal experiences, and the patient learns to do this too. A third confusing factor, though of less importance here, is that it is difficult to be sure that introspection is not always in fact retrospection (Bakan, 1954).

It will be seen that the data resulting from all three methods can, if desired, be treated as verbal behavior, in the way we have already discussed. A further step can be taken, and mental events can be inferred from this behavior. The first method, however, that of introspection proper, is distinguished in that its essential aim is to make direct observations of a particular class—observations of events not otherwise accessible. The distinction between these and other events is expressed by Bakan (1954) as that between experience and that of which the experience is. It is brought out by the example of the *stimulus error*. The phrase was coined by Titchener (Boring, 1957) to refer to the "error" of an observer in lapsing from the psychological viewpoint, in reporting on phenomena, to some other viewpoint. The psychologist should report on what he experiences. It is for the physicist, or some other scientist, to report on the external stimuli that bring about the experience. Boring instances a two-point threshold: The Titchenerian psychologist's task was to report when he experienced the two points as one, regardless of the fact that he knew there were two.

This brings out another aspect: the question of trustworthiness. As Boring (1953) pointed out, in classical psychophysics controls are not used; the control lies in the training of the subject. Psychophysical experiments can, of course, be done with animals using methods of discrimination training. Titchener ruled out of psychology animals (and children and clinical patients) because they could not be trained to observe their mental events and to distinguish between these and physical events.

Thus, it seems important to know whether there is a unique method; and if so, whether this should be used by psychologists, either among other methods or, as Titchener demanded, exclusively. Classical German psychology, as established by Wundt, defined psychology as the science of experience: immediate experience, not inner experience, because there is no valid distinction between inner and outer experience. As Boring (1957) put it: "The data of experience are merely themselves; a perception does not have to be perceived in order to be a perception; it has only to occur [p. 332]." This is interesting in view of Hebb's objection to introspection, quoted earlier. However, it follows from this definition that the method of psychology is

that of self-observation. For Wundt, "having an experience is the same as observing it [Boring, 1957, p. 332]." Boring spoke with great authority, yet a glance at the simplest form of Wundtian experiment casts doubt on this. For example, Wundt (1924) himself gave as an introductory and basic experiment listening to a metronome and observing the way in which we perceive the beats falling into a rhythmic pattern. The attempt to be more systematic led the followers of Wundt, notably Külpe and Titchener, to emphasize the training necessary for such self-observation.

A number of departures from, and reactions against, these views took place in psychology. They include Gestalt psychology and psychoanalysis. The most extreme, as far as method is concerned, was behaviorism. It was on this point that Watson took his stand. He wrote in 1930:

> Behaviourism, as I tried to develop it in my lectures in Columbia in 1912 and in my earliest writings, was an attempt to do one thing—to apply to the experimental study of man the same kind of procedure and the same language of description that many research men had found useful for so many years in the study of animals lower than man [p. IX].

Watson seems to have had several reasons for this view. One was his interest in the study of animal behavior itself. Another appears to have been an intense dislike of any suggestion of mysticism, such as he felt was involved in the study of consciousness. The only point of theoretical interest, however, is that of objectivity. Watson (1930) equated behaviorism with objective psychology, and introspective psychology, with subjectivity. He does not, however, present any arguments in support of this.

Thus, Watson agreed with the classical psychologists in insisting on a particular method, necessitated by a particular subject matter. Some attempts have been made, on the other hand, to resolve the opposition. Thus, Boring (1953) argued:

> Operational logic, in my opinion, now fuses this single dichotomy because it shows that human consciousness is an inferred construct, a concept as inferential as any of the other psychologists realities, and that literally immediate observation, the introspection that cannot lie, does not exist. All observation is a process that takes some time and is subject to error in the course of its occurrence [p. 187].

But the dichotomy is not fused here, for if consciousness is inferred, it presumably cannot be observed directly. Thus, Boring comes down on the side of the physicist and not the (Titchenerian) psychologist. It is, besides, not satisfactory to reject a method merely because it can lie. Infallibility of method does not exist in science.

A more sophisticated attempt is due to Burt (1962). He argued that the early objection to introspection, that two observers would disagree, was merely the result of failing to realize the importance of individual differences. The behaviorist insistence that data be public is unhelpful, because certainty in science is unobtainable. This objection, argued Burt, rests on the distinction between what is "public" and what is "private." Thus, it is said that language is inadequate to convey the experience of one person to another. But, in general, we do understand what is meant. Then it is said that only one person can observe the events of consciousness. But strictly speaking, this applies to every firsthand observation. The behaviorist attempt to make introspections respectable by treating them as verbal reports merely misdirects our attention, for it is precisely the subject's experience, not his verbal behavior, in which we are interested. Burt next argued that the model of the physical sciences adopted by Watson was a crude and oversimplified one, which would not be accepted now. It is not clear, however, what in Burt's view would be accepted; nor whether he thinks this presumably more sophisticated model appropriate for psychology. Finally, Burt attacked the newer behaviorists' argument from the principle of parsimony. A system such as that of Hull, Burt claimed, is far from parsimonious. And parsimony can only be achieved, in any case, when a science has reached a highly developed stage. Here again Burt's arguments are open to objection. Given that behaviorism is not parsimonious, it could still be more so than introspection. And by what criterion are we to tell if a science is highly developed, other than its degree of parsimony? If the principle of parsimony is accepted, then it is for each side to show that it is more parsimonious than the other. Burt concluded that introspection should be among the methods of psychology, though it should certainly not be the only one.

> Introspection is necessary, not only because it brings new questions to the fore, but also because it alone can supply much of the observational data needed to answer them . . . . We need to know what intervenes between the stimulus and the subsequent response; and here, so I have argued, introspection yields the most important clues [p. 238].

Burt's (1962) argument, then, seems to be that introspection as a method is not unique, but that

the data it yields cannot be obtained in any other way. This implies a contradiction. Burt rests his case, first of all, on the notion of the probabilistic nature of science, arguing that no scientist in any field would insist on absolute certainty before accepting a datum. But, as he goes on to say, the crux lies in the distinction between what is "public" and what is "private." Burt (1962) seems here to wish to deny any such distinction: "Strictly speaking, *every* first-hand observation is necessarily 'private'. Whether certain observations are treated as 'public' turns not on their specific or intrinsic nature, but merely on the context [p. 231]."

I think Burt's argument is open to objection, but in practice, the objections may not matter much. First of all, it may well be that whether observations are *treated* as public depends on the context, but that does not answer the question whether they *are* public. Now, in what sense is every firsthand observation "private"? This seems true in the sense that each individual experience is unique. First, each individual is unique, by virtue of different makeup, different history, etc. But even if an exact facsimile of an individual were produced, there must logically be two experiences, since to qualify as individuals the beings must at least be distinct in time and place. The experiences might be identical in content, but there would be no way of proving this with absolute certainty. But it does not follow that because there are always two experiences, there are always two things of which the experience is. The question as to whether there is a reality independent of observers has of course a long philosophical history. It seems to me that science, if it is to operate at all, must assume that this is so. But we could, in fact, rest the argument at the point of saying that there is a distinction between events that we may suppose to exist independently and those of which we cannot suppose this. The first are "public"; the second "private." In the second class we put experiences; in the first, stimuli and responses. In practice, the distinction is often not very important. On the "private" side, the difficulties of communicating experience have been exaggerated, as Burt pointed out. On the "public" side, exact replications are rather infrequent.

R. S. Peters (1953) pointed out that both Titchener and Watson were part of the "observationalist" tradition of science. Titchener held that we directly observe experience; Watson, that all we can observe is behavior. Later behaviorists such as Hebb say that we infer our knowledge of private events from the behavior we see. In the case of adult human beings, this behavior is usually verbal; but in the case of nonverbal beings, it may be, for example, discrimination learning. This, however, raises the question of what it is that we infer. To say that a child, an animal, or another adult has "an image" seems a curiously artificial statement, unless we have from our own experience some notion of what sort of a thing an image might be. It seems odd, in other words, to suppose that "image" has exactly the samé sort of intervening variable status as, say, "filter."

I am aware, of course, that a forceful movement, represented by writers such as Lacan, denies the whole possibility of applying to human beings principles of objective observation that derive from the physical sciences. (See, for example, Caws, 1968.) But this argument would lead us too far afield. Within the observationalist tradition, Peters criticized the insistence on method which was characteristic of both Titchener and Watson. Science is not such that there is any one method that guarantees success. Nor, indeed, are there methods that must be utterly proscribed. Rather, there are criteria for assessing any method as more or less useful. To put it another way, scientific "method" is a much broader concept than the "methods" of introspection or behavioral analysis. These are better regarded as techniques, being two of those in the scientist's repertoire. As Peters (1953) put it: "All that is required for an inquiry to be a theoretical science is that conscious attempts should be made to overthrow hypotheses [p. 719]." There seems no reason, in practice, why introspections cannot be used for this purpose, even though they be held to be logically distinct from other sorts of observation. If introspection conflicts with itself or with other data, this is no different from any conflict of results.

It might be argued that it is simply a matter of what we are interested in. Presumably both Titchener and Watson were happy for the other to pursue his own path, but each insisted that "you must not call it psychology." But to define a science either by its subject matter or by its techniques seems unhelpful. There is no way of saying in advance what will turn out to be relevant or useful. Rather, science as a whole is the enterprise of establishing statements that correspond with reality, and any one science is simply a group of loosely related enquiries, put together for convenience or because of demonstrable connections. Psychologists, it seems to me, are people who are

interested in the behavior and experience of men and animals. Like every scientist, the psychologist says with Molière *"je prends mon bien où je le trouve."* Introspection gives us information about experience. It yields some data otherwise inaccessible. It may besides bring to light facts that might otherwise be overlooked, or stimulate us to ask new questions. Like any technique, it has peculiar difficulties, especially when used in odd circumstances. These, however, are the natural hazards of science. I would suggest that the battle that began over 60 years ago, and which some are apparently still fighting, might now be amicably concluded.

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Looking Back and Ahead in Psychometrics

HAROLD GULLIKSEN *Educational Testing Service and Lindquist Center for Measurement, University of Iowa* [1]

In presenting the history of psychometrics, I shall do so in terms of the various investigators who worked in this area, even though I would, in general, agree with James Thurber in doubting the "great man theory" of the development of science. He pointed out that some people think it was a great day and a critical event when Benjamin Franklin sent up his kite and brought down lightning, demonstrating its fundamental similarity to electricity (Thurber, 1937). Others feel, however, that this event was not particularly critical, believing that if Franklin had not done this, somebody else would have made the same discovery; and we can see that this was exactly what happened with the harnessing of steam and the invention of the gas engine. Franklin did not make these discoveries, and sure enough somebody else did.

"Looking back in psychometrics" for me takes me to 1929 when I was a graduate student at Ohio State University, and Thurstone gave a seminar on the theory and applications of the law of comparative judgment and the method of paired comparisons. Numerous topics, such as art, aesthetics, ethics, subjective values, etc., had previously been dismissed with "What can you do about field or topic X? It is all a matter of opinion, and opinions disagree." I was tremendously impressed by the idea that now there was a clear-cut theory and experimental procedure for a rigorous treatment of those areas that are entirely a matter of opinion, and it was essential for the use of the method that the opinions should disagree (see Thurstone, 1959, for a collection of his articles on scaling).

Following the advice of Albert Paul Weiss, the senior professor in experimental psychology at Ohio State, I attented the University of Chicago summer school in 1929 and took a six-week course with Thurstone in which he covered test theory, scaling, factor analysis, and, I believe, mathematical learning theory. A week or two was spent on each of the four topics, and that was that.

A decade earlier, E. L. Thorndike (1918) perceived the necessity and possibility for such developments in quantitative psychology: "Whatever exists at all exists in some amount. To know it thoroughly involves knowing its quantity as well as its quality [p. 194]."

Computers

Indicating the developments of the last 40 years requires mention of the electronic computer. I was a research assistant for a year working on Thurstone's first study of primary mental abilities. The computational work in resolving a battery of about 50 tests into seven primary mental abilities meant that I was supervising a group of about 20 computer clerical workers for about a year. I recall Thurstone lamenting that his PhD candidates would not be able to do factor analysis dissertations because it would not be practical to employ such a crew for each PhD thesis. A few years ago, a research worker in the U.S. Civil Service in Washington, D.C., wanted some help in analyzing a set of attitude scales which he had given to different types of persons working under Civil Service, in order to see how the jobs could be changed to make them more attractive. He came up one afternoon, with his data on punched cards, and we started about 4:00 p.m. to run the preliminary error-detecting program, and we corrected the cards whenever errors were found. In all, including the scaling, correlations, factor analyses, and rotations, although the job was somewhat larger than the primary mental abilities one, we were finished about 3:00 a.m. the next morning.

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Requests for reprints should be sent to Harold Gulliksen, Division of Psychological Studies, Educational Testing Service, Princeton, New Jersey 08540.

Testing

During the past 40 odd years, we have come a long way, as you all know, since the publication of Thurstone's (1931c) first test theory text. In reliability theory the widely used K-R 20 and K-R 21 were developed (Kuder & Richardson, 1937), and we have now progressed to Kristof's (1972) reliability for vector variables.

Latent structure models have been developed by Lazarsfeld (1950, 1954) and by Birnbaum (see Lord & Novick, 1968). Lazarsfeld (1950, 1954) also pointed out that data on joint occurrences (Do persons who have a watch, for example, also have a fountain pen?) could be analyzed to detect underlying "latent classes." Bert Green (1951) developed a rigorous matrix solution for the latent class problem and demonstrated that it correctly determined the latent classes using artificial data for which the solution was known. As far as I am aware, no one has found a set of real data that clearly demonstrates the existence of independent, homogeneous latent classes as defined by Lazarsfeld. It seems to me, however, that Green's solution is important because of the clever use he made of third-order joint occurrences to give an analytical solution for the rotation problem. Although no one has used such a procedure, it appears plausible that other theories could be developed in which Green's use of third-order joint occurrences would give a precise analytical solution for the rotational problem.

Rasch (1960, 1966) presented a theory to test for the presence of a one-factor ratio scale in test data. Melvin Novick, Charles Lewis, and others have worked on Bayesian procedures. These and other developments have been presented and summarized by Lord and Novick (1968) in *Statistical Theories of Mental Test Scores*. The theory and practical applications of tailored testing are being investigated by Lord (1971), Cronbach and Gleser (1965), and others.

The foregoing developments, however, are still largely in the theoretical field. I hope they will have greater impact on the standard aptitude and achievement tests. As far as I am aware, there has been little or no impact on teacher-constructed tests used in grading classes. Instead, there is today what seems to be a serious movement away from any type of measurement in education, rather than an attempt to use better measurement methods.

In aptitude test development, the rule is to take validity coefficients seriously without raising the question, "Should this validity be high or low?" During World War II, while working on aptitude and achievement test development for the Navy, Norman Frederiksen and I obtained considerable experience in this area. At the gunners' mates school, we found that the validity of the reading test was high, and the mechanical knowledge and mechanical comprehension tests had low validity. Checking on the tests originally used for grading revealed that the course as initially taught and graded emphasized verbal memorizing and repeating of material from the manuals and did not require that the students know how to disassemble and assemble the guns, or how to detect and correct malfunctioning. We worked for about six months and developed identification and performance tests that measured the objectives given to us by the gunners' mates school. These tests ensured that students were judged on the basis of their ability to disassemble and assemble the guns and to detect and correct malfunctioning, which were the objectives of the course as stated to us by the instructors. The initial validity pattern with high validity for the reading test and low validity for the mechanical knowledge and comprehension tests was not reasonable in view of the stated course objectives. The validity pattern which was found after introduction of the new testing program, based on the objectives stated by the instructors, indicated that the students were coming much closer to learning what the instructors wanted them to learn. On the basis of grades on the new achievement testing program, the validity of the reading test took a nose dive, and the mechanical comprehension and mechanical knowledge went up. The same thing happened in basic engineering, where the arithmetic test showed highest validity initially.

What had happened in the basic engineering course was that 1 of the 12 weeks was devoted to shop arithmetic, and the grades for that week ranged from very high to very low. The other 11 weeks were devoted to actual practice in the shop, using lathes, drill presses, saws, etc. The student's performance was rated by instructors who watched him work and checked various pieces he turned out—grading them on closeness of agreement with initial specifications. The grades for these 11 weeks were found to have very low variability, being mostly from 80 to 85 on a scale of 100. Apparently the instructors did not think that the students showed a high level of performance, but

they were usually reluctant to give a very low mark. It is a well-known fact of test theory that if one grade with high variability is averaged with a number of other grades of low variability, the final average will correlate highest with the one component with large variability. The problem here was to develop grading procedures for the 11 weeks of shop work that were more objective than the instructor's judgment and that would be sensitive to the students' variability if they did vary in performance. Nicholas Fattu worked for a year developing gauges to measure the products quickly and accurately, and, on the basis of the achievement measures, the validity of the arithmetic test dropped and that of the mechanical aptitude tests went up. Similar results were obtained in the torpedoman's and other schools. Again, we achieved the same result as in the gunners' mates school. The validity pattern found after the introduction of the new performance testing program, based again on the school objectives as stated by the instructors, was a reasonable one, indicating that the students were approximating the objectives desired by the instructors. Some of this work has been written up in Stuit (1947), especially Chapters 12, 13, and 15. The interesting point demonstrated here is that a pattern of validity coefficients over a set of aptitude tests can indicate, by its reasonableness, or unreasonableness in agreeing with course objectives, whether or not the teaching, testing, and grading procedures are in agreement or in disagreement with the objectives of the instructor.

I think school and college grades are in need of similar scrutiny. For example, the spatial relations test of the College Board showed good validity for grades in some engineering drawing classes and poor validity for other engineering drawing grades. Such results would be expected if these courses, which were all given the same name, engineering drawing, were in fact quite different and were graded on different bases. One possibility is that the engineering drawing courses for which the spatial relations test showed good validity were courses that required the student to use spatial visualization in his course work. The other courses with the same name, engineering drawing, but with low validity for the spatial relations test were courses taught, tested, and graded so that the student did not need to use spatial visualization to be successful in the course. A general discussion of such problems under the title "Intrinsic Validity" can be found in Gulliksen (1950).

An article by Plotkin in the March 1972 issue of the *American Psychologist* discussed problems in the area of the validity of tests brought to the fore by the Equal Employment Opportunities Act of 1964.

I feel sure that Benjamin Shimberg and his associates, who are working at the Educational Testing Service on such things as tests for auto mechanics, will not accept the conclusion that the major important quality for an automobile repairman is high verbal ability, on the basis of validity studies, but will see to it that the criteria are changed so that the important abilities are mechanical skill, trouble shooting ability, etc.

Some years ago, in looking over validity coefficients for the Differential Aptitude Test, I noticed that for one school the best predictor of grades in Latin was the clerical test (.47). For the other tests of the Differential Aptitude Test, the correlations with Latin grades ranged from a low of −.37 for mechanical reasoning through −.02 for verbal reasoning, to a high of .19 for sentences. It was pleasing to note that this was not generally true for all of the schools studied. But it would be even more pleasing if some steps had been taken to alter the teaching and grading procedures in that school. Other studies by the Psychological Corporation showed that higher educational level goes with higher clerical ability (see Bennett, Seashore, & Wesman, 1959, pp. 48, 79; 1966, pp. 5–42). This may indicate an undue emphasis on clerical ability for college selection.

In 1939, Truman Kelley noted that only the verbal and quantitative abilities seem to be important as far as academic work in schools and universities is concerned. He spoke of the numerous abilities that even then were being isolated by factor analysis and indicated his fear that "many of the factors thus far 'found' approach pretty close to the limit of no importance [p. 142]." This may well be true of many of the 150 or so factors in the French (1951) monograph, but before reaching any such conclusions, the school's teaching, testing, and grading procedures should be studied carefully and revised where necessary. My own judgment would be that when this is done properly we will find that verbal and quantitative do not exhaust the list of useful abilities.

In 1901, Clark Wissler, while getting his PhD with James McKeen Cattell at Columbia University, investigated the validity of a number of tests for predicting grades at Columbia. The validities ranged from −.02 for reaction time to .19 for logi-

cal memory. During the seven decades since then, aptitude and standardized achievement tests have advanced tremendously. However, I think the evidence is that college grades are now about the same as they were at the turn of the century. Wissler (1901) reported that the correlations of grades ranged from a low of .30 for rhetoric and French to a high of .75 for Latin and Greek, which I believe would be very similar to the correlations obtained today.

During recent decades there have been a few attempts at improving the quality of college exams and grades. An example is the work of the examining office at the University of Chicago during the 1930s and 1940s under the direction of L. L. Thurstone and Ralph Tyler. The Chicago faculty later abandoned this program.

The great need we have now is not for the improvement of aptitude tests, but for improvement in the criteria against which they are evaluated, including not only grades for four years in college, but activities and achievements during the 40 years after college.

Scaling

In the early 1930s, Thurstone's interests changed from scaling to factor analysis. For the next decade, development in the scaling area was very slow. Marion Richardson and I asked Gale Young and Alston Householder about the problem of determining dimensionality and a coordinate system for a set of points from the interpoint distances. They solved the problem and published the Young and Householder (1938) paper on multidimensional scaling. The applications of this method by Marion Richardson and Klingberg appeared at about the same time. Otherwise, not much appeared until Torgerson (1951) developed the theory and verified a section of the Munsell color system in his thesis. Messick (1956) applied the method to attitude scales a little later. Since then, the development of scaling theory has been tremendous: the law of categorical judgment, the method of successive intervals, and statistical tests for fit of data to theory. These developments are presented in Torgerson's (1958) *Theory and Methods of Scaling*. Since then, there has been the extension to take care of individual differences, which makes the methods far more useful in attitude measurement and other applications in social psychology (see Carroll & Chang, 1970; Helm & Tucker, 1962; Tucker, 1972; Tucker & Messick, 1963). Luce and Tukey (1964) developed the theory of conjoint measurement and showed the independent foundation on which psychological measurement rests. Bock and Jones (1968) gave rigorous estimation procedures. The theory has been developed by Suppes and Zinnes and Tversky and others (see Krantz, Luce, Suppes, & Tversky, 1971) so that measurements from psychological scaling are not dependent on other methods (see Luce, Bush, & Galanter, 1963a, 1963b, 1965a, 1965b). Indow and his group in Japan, Ekman and his group in Sweden, and Stevens in the United States have been active in the development of theory and applications of scaling.

Applications of scaling techniques to linguistics and free recall are illustrated by the work of John B. Carroll (1971) and Friendly (1972). My own work (Gulliksen & Gulliksen, 1972) has also illustrated the application of scaling and factor techniques to attitudes toward work and leisure in cross-cultural comparisons. Coombs (1964) and his co-workers developed a nonmetric multidimensional unfolding procedure, using this method to study confusions of Morse code signals, and showed two dimensions in the subset of 10 signals studied.

As indicated above, there have been numerous applications of scaling techniques by research workers in various academic university settings. However, when we consider the various applied fields in which linear and multidimensional scaling could be used, the picture is different from the development of theory and applications in the academic setting. The various polling organizations report nothing but total percentages, sometimes broken down by various pre-formed categories, such as education, sex, rural–urban, etc. I have never seen a single instance in which a factor analysis of a set of observations is given, so that various points of view, or clusters of opinions, can be found. Robert Tryon (1955) reported a factor analysis of voting areas around San Francisco and found that the various indices available formed a three-factor system. He suggested that voting might well be associated with these factors, so that one would get better prediction by repeating such a study in connection with a new election poll and using these factors as independent variables in adjusting the polling results. As far as I am aware, no polling group has paid any attention to such possibilities.

Green and Carmone (1970) and Green and Rao (1972) showed how multidimensional procedures could give valuable information in consumer sur-

veys. Applications in behavioral sciences were given in Shepard, Romney, and Nerlove (1972). Applications in marketing research were presented by Bass, King, and Pessemier (1968). The use of scaling methods in studies in perception by Carroll and others are reported in Carterette and Friedman (1974). Bell laboratories has compiled a bibliography of recent studies and applications of multidimensional scaling (Harris, 1972). It is pleasing to note that the scaling techniques have recently been utilized in a number of research areas and applied fields.

Factor Analysis

With respect to factor analysis, the initial papers presenting the principal components and other methods were published by Thurstone (1931a, 1931b, 1933) and Hotelling (1933), following earlier work by Spearman and Holzinger. Thurstone presented his problem to Bliss in mathematics and Bartky in statistics one noon at the Chicago Quadrangle Club. He explained that he had a square symmetric array of numbers and wanted to express it in terms of summed products of a smaller array. Their reaction was, "Oh, you mean the square root of a symmetric matrix." In this way Thurstone learned that matrix theory existed and was relevant to the factor problem, so he embarked on a year or two of tutoring and published *The Vectors of Mind* (1935), followed later by *Multiple Factor Analysis* (1947), giving a concise summary of the crucial aspects of matrix theory and their use in factor analysis. Prior to the advent of electronic computers, approximations such as the centroid method, with largest correlations used as initial estimates of communalities, were widely used because of their practicality.

I remember Samuel Wilks remonstrating about this. He said, "We know a good method, the principal components, based on least squares, that gives a best fitting reduced rank matrix. Why can't you use that instead of these ad hoc approximations whose properties are unknown?"

Since then, Lawley and also Jöreskog (1970) with Gruvaeus and van Thillo have presented the theory and associated practical computer methods. Kaiser's (1970) little jiffy is very widely used. Tucker has given us the procedures for double-centered matrices (Tucker, 1956), the interbattery matrix (Tucker, 1958), and three-mode and multimode analysis (Tucker, 1966a). Harris (1962, 1963) presented relations among factor theories and cautions that should be observed when attempting to measure change. Guttman (1971) presented some extensions and applications of his facet theory. Horst (1961) gave possible applications of generalized canonical correlations. McDonald (1967) gave us his nonlinear factor analysis. Arbuckle (1970) developed a procedure using the Toeplitz matrix as the error matrix instead of a diagonal matrix, so that the factor procedures may be applied to matrices where it is reasonable to assume "stationary error," as in analyzing nerve potentials.

Factor analysis theory and associated electronic computer programs are in a reasonably well developed state. As to applications of the methods, it has been mentioned previously that numerous aptitude test batteries have been analyzed, so that psychologists have some reasonable notions regarding the basic abilities represented in aptitude tests.

The factor methods have, however, been only sketchily used in other fields where they would be extremely valuable. Harman (1967) devoted about a page of his text to indicating applications in economics, sociology, physiology, etc., but the impact of these factor studies on the fields indicated has been minimal.

Schiffman and Falkenberg (1968) presented an interesting study of matrices with stimuli designating rows, by retinal cells designating columns, or stimuli by taste neurones, that give interesting pictures of the structure of these sensory systems. In the study of retinal cells, the stimuli spread out in a curve—violet, blue, green, yellow, orange, and red—while in the same space the retinal cells clustered three in the blue area, four in the green area, and four in the red area. The factor analyses demonstrated that some cells were highly sensitive to blue and not to other colors; some were highly sensitive to green, and others sensitive to red. This result is in agreement with a three-color (blue, green, and red) theory of color vision. If a four- or a two-color theory represented the mode of functioning of the retina, one would expect to find four or two clusterings of cells. For taste, a definite three-dimensional structure was obtained, but the details are not so clearly interpretable.

Memory is another field in which factor analysis would be extremely valuable. For example, Paul Kelley's (1964) study demonstrated that memory span is a factor which includes visual and auditory material, as well as nonsense and meaningful material. However, when one deals with longer lists, so that it takes a number of repetitions to memorize

them, then rote memory differentiates from memory for meaningful material. That is to say, when Ebbinghaus introduced the nonsense syllable, as he thought to simply control for the irrelevant factor of possible differences in previous associations, he was unwittingly shifting to measurement of a different ability. Recently, there has been great emphasis on what is termed *free recall*, which means that the material, though meaningfully organized, is presented randomly, to see the extent to which the subjects will make use of the organization in recall. Again, as with Ebbinghaus, it is assumed that this is simply another interesting procedure for tapping the memory function. However, as far as I am aware, no factor studies have been made including the free recall, the rote, and the meaningful memory where the order of presentation must be the order of recall. We do not know whether the free recall ability is the same as the previously established rote or meaningful memory, or whether a new ability has been introduced with this new procedure. Stake (1961) presented evidence indicating the possibility that the change from free study such as Ebbinghaus (1885) used, to the memory drum, introduced by Müller and Schumann (1894) merely as an added experimental control, may have altered the ability being measured.

Eight indices of "excitatory potential" (a useful hypothetical construct) were used by Lloyd Humphreys (1943) in a conditioned eyelid experiment. He found two factors. Acquisition *amplitude* and extinction *amplitude* loaded on one factor, while acquisition and extinction *latencies* loaded on a different factor, along with extinction frequency. Acquisition frequency loaded equally on both factors. That is to say, the experimenter's selection of one or another from a set of possible indices may really be changing the hypothetical construct being measured.

For decades it has been asserted that "intelligence is the ability to learn" (e.g., Binet, 1909, especially p. 146; Buckingham, 1921, especially p. 273; Dearborn, 1921; Peterson, 1926, especially pp. 268 & 276; Pyle, 1921). This view that intelligence is the ability to learn has been critically examined (e.g., see Peterson, 1926; Simrall, 1947; Woodrow, 1946). Psychologists have devised numerous clever tasks in rote learning, meaningful learning, concept learning, motor learning, etc., such as mirror drawing, pursuit rotor, reversal learning, etc., which require an ability to learn, that can be measured by time, errors, or trials taken to reach some criterion, or by parameters of some learning curve fitted to the data. In only a few cases have such studies also included a few of the standard test scores that may be related to intelligence.

Studies in this area by Duncanson, Stake, Allison, Manley, Games, and Bunderson, reviewed in Robert Gagné's (1967) *Learning and Individual Differences*, have indicated that there are a number of different abilities represented by the different learning tasks, as well as a number of different abilities represented by the intelligence or ability tests. So far, *some* of these abilities seem to be unique to measures of learning, or to test scores, but there are some factors that have loadings on both the learning scores and the test scores. A clear answer as to the relation between aptitudes as measured by tests and learning abilities as measured by various learning tasks devised by psychologists is not available at present. The topic is in need of much further research.

For the last 40 years there has been a profusion of factor analyses of batteries of aptitude tests, but there are numerous other areas in learning, memory, physiology, nerve potentials, economics, political science, sociology, etc., in which factor analysis would be extremely valuable and in which only a few studies have been made.

Learning

With respect to mathematical learning theory, Thurstone (1930a) presented a mathematical derivation of an equation of the learning curve based on an urn analogy, which turned out to also be derivable from Thorndike's Law of Effect (Gulliksen, 1934). Thurstone (1930b) also showed how this theory could be applied to determining a functional relationship between learning time and length of task, and to separating learning ability of the individual from the difficulty of the task, using factor methods. I presented an analytical procedure that separated learning ability from initial performance (Gulliksen, 1942).

During the 1950s, a variety of learning models were presented based on stimulus sampling ideas (Estes, 1956), stochastic processes (Bush & Mosteller, 1955), stepwise increases or decreases in strength of correct and incorrect responses (Audley & Jonckheere, 1956), and various models suggested by Bower, Trabasso, Atkinson, Suppes, and others.

In general, these more recent models tended to have two characteristics:

1. Response strengths, or response probabilities, changed by finite amounts with each trial. The substitution of differentials for deltas was believed to be an extremely inappropriate step that must be avoided.

2. In order to obtain good parameter estimates, it was usually assumed that all subjects in a group could be regarded as giving estimates of the same parameter values so that the record of the group of learners was analyzed to determine one set of parameters.

There are several questions introduced here that it seems to me should be subjected to careful experimental investigation, rather than being settled by assumption:

1. We now have a variety of stochastic, or finite, step models, and also older continuous models. Both of these types of models should be tried out on various types of learning data. It is perfectly possible that different theories will be best for different types of tasks. The same type of theory may well not fit conditioned escape response, maze learning, visual shape discrimination—with attention to transposition, paw retraction to avoid a shock, conditioned emotional reactions such as rapid breathing, etc. For example, there is evidence that conditioned paw retraction does not transfer from the right to the left brain in split-brain animals, while increased breathing rate transfers very rapidly. We need now a large number of studies in which various stochastic and continuous models are tried out on various types of learning data.

2. I feel that the primary stress should be on using learning parameters that are psychologically meaningful. By this I mean parameters determined for each individual such as difficulty of task, learning ability, initial preference, and final performance on the task. Such parameters are, in general, different for each individual and characterize important individual differences. These parameters seem to me to be meaningful in understanding differences between learning tasks and differences between individuals in learning these tasks. Parameters such as number and length of runs of errors, average and variance of learning parameters, and number of alternations have been frequently or usually used with stochastic models. Such parameters characterize the group as an entity and do not differentiate the differing characteristics of different individuals. They seem to me to be parameters selected because they fit with the stochastic models, rather than because they have any interesting psychological significance in understanding the learning process or the differences between learning tasks and between learners.

Bush and Mosteller (Chap. 15 in Bush & Estes, 1959) gave a comparison of eight different learning models, with respect to their agreement with Solomon and Wynne's data on shock avoidance by 30 dogs given 25 trials each. The comparisons are entirely in terms of means and standard deviations of distributions of a number of variables, such as number of trials before first and second avoidance, total number of shocks, number of alternations, number of trials before the first run of four avoidances, etc. Such parameters characterize the group as a whole and need to be supplemented by parameters that indicate the differing abilities of different individuals. Instead, it is assumed either that the basic parameters are the same for all animals, or else that the parameter varies according to some specified distribution. This seems to me to be an approach dictated basically by the characteristics of the stochastic approach, rather than by the psychologically interesting properties of learners and learning tasks. Determination of parameters for each learner offers a much better way of understanding the learning process, in terms of parameters of individuals, and parameters of the tasks, such as initial ability and learning ability, and difficulty of the task.

3. Merrell (1931), Sidman (1952), and Estes (1956) pointed out the difficulties involved in using group or average learning curves, yet obtaining a single set of parameters for the average learning curve is still a usual procedure. Is it legitimate to regard learning parameters as the same for all subjects in a group, or do some subjects have definitely better learning ability or initial performance than others do? Again, the answer may be different with different types of learning problems and with variations in difficulty of the problem. There are at least two possible approaches that should be tried on this problem of individual differences in learning parameters. One approach proposed, and tried out on some sets of data by Tucker (1966b) and by Weitzman (1963), is a principal components analysis of a matrix of learning curves. The method gives a set of k learning parameters for each individual, and k generalized, or master learning, curves. In the special case in which k is equal to one, then it is legitimate to use the group or average learning curve.

4. Parameter estimation for individual learning curves for the finite step models is a difficult problem. Procedures have been devised by Ramsay (1970), Wainer (1968), and Best (1966) for parameter estimation for individual learning curves. Using Monte Carlo data with known parameters, Ramsay (1970) found that the input parameters were not recovered except for the limited case of only two parameters, initial probability of a correct response, and the effect of reward of a correct response on the strength of the correct response. Ramsay also felt that negative parameters, which allowed a decrease in response strength, should not be permitted because this might lead to negative response probabilities. Best's (1966) procedure allowed for the possibility that the strength of the incorrect response would be decreased by punishment for an error. If the fitting problem is satisfactorily solved, then various stochastic and continuous models could be compared with respect to parameter determination for individual rather than group learning curves.

5. One of the great handicaps in the study of learning has been the impossibility of obtaining evidence on reliability by replication. When a learning curve has been obtained, a second one on the same problem for the same individual is impossible, because he already knows the solution and cannot learn it again. If one tries a different problem, there is the question of how similar the two problems are, and also the question of positive or negative transfer. If one tries the same problem with another individual, then there are the possibilities of different learning abilities for the different individuals. Sperry (1961, especially p. 1753; 1964, especially p. 48) felt that his work with split-brain preparations offered an opportunity for replication from left to right brain with what was essentially a duplicate subject. So far the evidence here is conflicting. Meikle, Sechzer, and Stellar (1962), working with cats suspended in a harness and learning to lift the front paw to avoid a shock signaled by stroking the shoulder, found (for three animals) a very good linear relation between number of trials to criterion for right-brain and left-brain learning. Phillip Best (1966) analyzed visual discrimination data from an experiment by Meikle and Sechzer (1960) and found a strong linear relation between first- and second-side learning in split-brain cats. By contrast, Ian Steele Russell (Russell & Kleinman, 1970), working with functionally split-brain rats on a conditioned escape response, found that for a given difficulty of the problem there was a zero correlation between trials to criterion for left versus right brain. He pointed out that this is consistent with the view of learning as a finite step process. Recent work by Gulliksen and Voneida also found marked dissimilarity between right-brain and left-brain learning in split-brain cats.

Another problem is raised by the probability learning situation. When, for example, one stimulus is rewarded 70% of the time and the other rewarded 30% of the time, some investigators report that the subjects choose the stimuli about 70% and 30%, respectively. This behavior is known as *matching* and would result in $.7 \times .7 + .3 \times .3 = .58$ success. Choosing the "70%" stimulus all of the time would result in 70% success. This is known as maximizing behavior. Stimulus sampling theory predicts matching. Maximizing is frequently found, however. Wainer (1968) found that maximizing behavior was the rule and succeeded in modifying the stimulus sampling theory so that with different parameter values it would predict either maximizing or matching. His data gave good agreement with the generalized theory and showed maximizing rather than matching. He devised methods of fitting parameters to individual curves.

Richard Rose (Rose, Beach, & Peterson, 1971) at the University of Washington recently reviewed the major studies in this field and concluded that "probability matching though widely accepted by psychologists is not found when individual records are examined, instead of group averages. The individual response probabilities are much further away from matching or other theoretical values than would be permitted by the most generous interpretation of extant theories [p. 11]." In my view this points to the desirability of estimating parameters for individual rather than group curves.

In this review, I wish to make clear the difference between assuming distributions of parameters for the group and *also obtaining parameters characteristic of each individual*, which is the procedure usually used in factor analysis and testing (and which can be used in scaling), as contrasted with the customary procedure in stochastic learning theory (illustrated by the analysis of the Solomon-Wynne data) which assumes distribution functions and parameters for the group and stops there without any attempt to detect the best versus average versus poorest learners. I think such an approach that makes no attempt to characterize differentially the different individuals in the group is inappro-

priate especially for studying learning in which one of the central interests would be individual differences, if they exist. Also, if learning tasks were found in which all individuals of a species performed alike except for random differences, this would be very interesting and important, but can be ascertained only if individual parameters are obtained and found to differ only randomly from individual to individual.

In the field of mathematical learning theory, it seems to me that a great deal of work still needs to be done on parameter estimation for individual learning curves and in comparing various stochastic and continuous models. By contrast, in the fields of test theory, scaling, and factor theory, the theory including parameter estimation, significance testing, and variance components analysis procedures are reasonably well developed. Test theory, though adequately utilized in standardized testing programs, has not yet had much impact on the teacher-constructed tests and on grading procedures. As indicated above, it is necessary to keep in mind the objectives of the instruction. The various testing and grading procedures should be examined to see if they reflect adequately the objectives; and, as pointed up in the account of work for the Navy, interesting clues regarding the adequacy or inadequacy of testing and grading procedures can be obtained from studying the pattern of high- and low-validity coefficients for different aptitude tests. Recently, scaling, especially multidimensional scaling, has received considerable attention from workers in certain applied fields and in some research areas, but its use could be more widely extended, as for example in election polls. Scaling methods together with factor analysis of the data could give information on possible differing opinion patterns of different individuals that might well be more informative than limiting the analysis to group averages and percentages. Numerous batteries of aptitude tests have been factor analyzed—but application of factor analysis to economics, sociology, physiology, etc., is just beginning to get under way.

Quantitative psychology, which could be reasonably adequately covered by a six-week course in 1929, has moved a long way in the directions indicated by Thurstone (1937) in his Dartmouth address as retiring first president of the Psychometric Society, "Psychology as a Quantitative Rational Science."

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Distribution by Sex of Authors and Editors of Psychological Journals, 1970–1972

Are There Enough Women Editors?

MARTHA TEGHTSOONIAN *Smith College*[1]

The present-day counterpart of the female suffrage movement faces a situation at once more complex and more ambiguous than that which existed 50 years ago. Since the passage of the Nineteenth Amendment in 1920, there no longer are major legal barriers to equality for women in this country. Yet, accumulating evidence indicates a widespread failure of women to achieve equality with men in professional status, job quality, and salary (see, e.g., Chase, 1971; Levitin, Quinn, & Staines, 1971; Rossi, 1970). An extensive survey of departments of psychology, reported by the Task Force on the Status of Women in Psychology (Survey, 1971), showed a steady decrease in the percentage of women from undergraduate majors (45% female), through graduate students and PhDs, to assistant, associate, and full professors (7% female). The fact is that proportionally more men than women psychologists continue up the ladder of professional advancement.

The finding that women occupy lower responsibility and lower salary positions proportionally more often does not in itself demonstrate sexual bias in hiring and promotion. There are two classes of explanations for such an outcome. One class attributes the differences to characteristics of women themselves: lower aspirations, lower productivity, and lesser degree of professional commitment, for example. These explanations assume that the reinforcement contingencies for professional behaviors are defined and administered without regard to the sex of the performer, and that different outcomes therefore depend only on differences in job performance. The other class of explanations attributes the difference to the policies of institutions and professional organizations: different pay scales for women, differential hiring and promotion practices, and nepotism rules, for example. These explanations assume that there are two sets of reinforcement contingencies for professional behaviors, one for men and one for women, and that the one for men is designed to reinforce job performance at a higher rate than the one for women. Data bearing on these hypotheses are not extensive; however, Astin (1972), showing that academic women in psychology hold low-status positions and are paid low salaries relative to their male colleagues, concluded: "Although it could be argued that the woman psychologist's lower rank, lower salary, and lack of tenure are a consequence of merit consideration . . . , these analyses suggest strongly that such is not the case [p. 380]." In particular, she showed that inequalities in reward remained when men and women were equated for publication rate.

The present study investigated one type of professional behavior—publication of research in psychological journals—and one type of reinforcer—appointment to journal editorship. That publishing research is considered an important component of the psychologist's job goes without saying: Salary, promotion, and professional standing may depend on it. It seems reasonable to assume that the post of journal editor constitutes a mark of recognition from one's colleagues of the excellence of one's work, and an indicator to others in and out of the profession of high professional standing.

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[1] Some of the data on which this article is based were reported at the meeting of the Eastern Psychological Association, Boston, Massachusetts, April 1972, under the title "Sexual Bias in the Selection of Editors of Psychological Journals?" The author is grateful to Ann Peterson, who tabulated the 1971 data, and to Smith College, which made it possible for her to do so through a Sloan Foundation Summer Research Grant. The author also thanks Robert Teghtsoonian for his criticisms of the manuscript and for suggesting the analysis of citation rate.

Requests for reprints should be sent to Martha Teghtsoonian, Clark Science Center, Smith College, Northampton, Massachusetts 01060.

Can women psychologists who maintain the same rate of publication as their male colleagues have the same expectation of editorial appointment?

In an earlier period, 1929–1949, it was unusual for women psychologists to become journal editors. According to Mitchell (1951), 20% of the Fellows of APA during that interval were women, but of the 187 editor years of APA journals, a woman accounted for only 1! A sample of psychological journals in 1970–1972 was examined to discover whether the situation had changed in the intervening 20 years.

Eleven psychological journals publishing experimental reports and considered by the author to be of moderate to high prestige were selected to represent a variety of fields. They are referred to as Sample A (see Table 1). If a journal published three to six issues in a year, all were included; if there were more than six, only six issues during the year were included. An editor was defined as one whose name appeared on the cover of an issue as (*a*) editor (hereafter designated as principal editor); (*b*) associate editor; or (*c*) consulting editor, advisory editor, member of an editorial board, etc. Separate tabulations were made for the first two categories and for all three categories combined. Editor's first (or middle, if first was represented by an initial) name was classified as "usually given to a female" or "usually given to a male." Consultation of membership directories and letters of inquiry permitted the eventual classification of all editors. (For reasons discussed below, editors with foreign affiliations, about 3% of the sample, were excluded.)

The results are shown in Table 1. In the sample of 120 editor years for principal and associate editors, only 1 was accounted for by a woman. In this respect, there has been no essential change since Mitchell reported in 1951. In the sample of all editors, totaling 322, 316, and 319, for 1970, 1971, and 1972, respectively, the number of women was 14, 14, and 19, that is, 4.3%, 4.4%, and 6.0%. Journals varied rather widely, with 0%–12% of the editors being women. (The modal *number* of women editors per journal was 1. Is this evidence of tokenism?)

As women comprise 24% of psychologists (Untitled, 1971) but only 5% of editors, it is clear that women psychologists are a great deal less likely than their male colleagues to become editors. When Mitchell presented similar findings in 1951, she asserted that they showed discrimination against women. Boring (1951) replied that the fault lay, not with APA, but with women themselves, who preferred applied fields in psychology, who failed to be "professional fanatics," and who failed to seek prestige through the publication of books. The question, then, is whether there is a bias toward men in the selection process for editors, or whether unbiased selection operates on a pool of potential editors in which there are proportionally fewer women than there are in the profession as a whole. Several factors that would operate to reduce the number of potential female editors can be hypothesized: (*a*) *publication*—women may publish less than men; (*b*) *contribution to published research*—women may less frequently be major contributors; (*c*) *quality of research*—women may publish research of poorer quality; (*d*) *age*—women publishing research may be younger than their male colleagues; (*e*) *rate of publication*—women may publish fewer papers per author than do men. Subsidiary data analyses were carried out to check these hypotheses. In addition, the generalizability of the findings for percentage of women editors, and for Hypotheses *a* and *b* were checked by performing the same tabulations for a second sample consisting of all 1972 APA journals publishing primarily research reports.

Some Possible Explanations of the Small Number of Women Editors

PUBLICATIONS

If women do not publish as much as men, this would readily account for a lower probability of women being selected as editors. The percentage of authors who are women was tabulated for each of the journals. Authors of all articles except book reviews and monographs became part of the sample. Authors' names were classified in the same way as editors' names. (Since most editors have American affiliations, all authors and those few editors with foreign affiliations were eliminated from the sample. Overall, about 3% of the editors and 15% of the authors listed foreign affiliations.)

Initially, about 7% of the authors' names were unclassifiable. For the 1970 sample only, a letter explaining the purpose of the study was sent to each unclassifiable author, with a prepaid, addressed postcard and a request to return it after checking the appropriate sex. Unclassified names eventually represented .28% of the 1970 sample; the return rate on postcards was 90%. Originally

TABLE 1

Number of Editors, Authors, and First Authors and Percentage Who Were Women for Eleven Psychology Journals in 1970 and 1971 and for Sixteen Journals in 1972

Sample	Journal	Year and volume number	Editors		Authors		First authors		APA division	
			N	% female	*N*	% female	*N*	% female	No.	% female
A	*American Journal of Psychology*	1970:83	19	.0	88	17.1	49	8.2	3	10.1
		1971:84	10	.0	60	13.3	42	9.5		
		1972:85	11	.0	68	10.3	39	12.8		
B	*Developmental Psychology*								7	35.8
		1972: 6	30	23.3	148	31.8	74	29.7		
A, B	*Journal of Abnormal Psychology*	1970:75, 76	37	2.7	218	11.0	118	20.2	8	21.3
		1971:77, 78	17	.0	162	7.4	82	7.3		
		1972:79, 80	17	.0	145	7.6	72	8.3		
B	*Journal of Applied Psychology*	—								—
		1972:56	24	4.2	154	7.8	88	5.7		
A, B	*Journal of Comparative and Physiological Psychology*	1970:72, 73	39	5.1	253	8.7	125	3.2	6	8.3
		1971:74, 75	41	4.9	190	11.6	103	10.7		
		1972:78, 79	39	3.9	166	12.7	89	10.1		
B	*Journal of Consulting and Clinical Psychology*	—							12 13	20.4 16.5
		1972:38, 39	49	2.1	337	11.6	169	7.1		
B	*Journal of Counseling Psychology*								17	15.6
		1972:19	23	13.0	193	14.0	96	11.5		
A, B	*Journal of Educational Psychology*	1970:61	18	5.6	119	17.0	63	15.9	15	22.9
		1971:62	20	5.0	163	18.4	78	11.5		
		1972:63	19	5.3	141	14.9	80	7.5		
A	*Journal of Experimental Child Psychology*	1970: 9, 10	33	12.1	121	30.6	69	30.4	7	35.8
		1971:11, 12	32	9.4	112	32.1	62	25.8		
		1972:13, 14	32	10.9	128	29.7	77	29.9		
A, B	*Journal of Experimental Psychology*	1970:85, 86	52	5.8	266	10.5	144	12.5	3	10.1
		1971:88–90	59	6.8	281	11.0	137	10.2		
		1972:92–94	62	8.1	262	15.3	142	15.5		
A	*Journal of Experimental Research in Personality*	1970: 4	14	7.1	72	8.3	40	5.0	8	21.3
		1971: 5	13	7.7	55	3.6	34	3.0		
		1972: 6	13	3.8	56	17.9	28	14.3		
A	*Journal of Experimental Social Psychology*	1970: 6	32	3.1	62	14.5	31	9.7	8	21.3
		1971: 7	31	6.5	94	6.4	55	5.5		
		1972: 8	28	7.1	81	16.0	39	10.3		
B	*Journal of Personality and Social Psychology*	—							8	21.3
		1972:21	27	3.7	155	16.8	87	14.9		
A	*Journal of the Experimental Analysis of Behavior*	1970:13	41	2.4	66	4.5	41	4.9	25	11.9
		1971:15, 16	43	2.3	137	9.5	82	7.3		
		1972:17, 18	37	5.4	133	12.0	79	8.9		
A	*Journal of Verbal Learning and Verbal Behavior*	1970: 9	24	.0	150	19.3	87	13.8	3	10.1
		1971:10	30	.0	146	24.0	77	24.7		
		1972:11	28	3.6	149	21.5	79	17.7		
A	*Perception and Psychophysics*	1970: 8	13	.0	129	20.1	68	14.7	3	10.1
		1971:10	21	.0	123	13.0	67	14.9		
		1972:11, 12	33	6.1	136	16.2	74	13.5		
	Weighted mean									
A		1970		4.3		14.2		11.7		
		1971		4.4		13.9		12.1		
		1972		5.8		15.8		13.8		
		1970–1972		4.9		14.6		12.5		
B		1972		7.2		14.3		11.8		

unclassifiable names were slightly more likely to be male (88.2%) than eventually classifiable names (85.7%). The difference is small enough, and the labor required to obtain complete classification so great, that for the years 1971 and 1972, unclassifiable names were simply eliminated from the sample.

Table 1 shows that, in 31 of 33 comparisons, the percentage of authors in a given journal year who are female is larger than the percentage of editors for that same journal year who are female, sometimes substantially so. Over the whole sample, 14.2%, 13.9%, and 15.8% of the 4,532 authors were women, for 1970, 1971, and 1972, respectively.

This value of about 15% can be compared, first, to the 24% of APA members who are female. Why should the percentage of women authors be lower than that of women psychologists? A possible explanation is suggested by the division memberships listed in Table 1, where each journal is accompanied by the APA division whose interests it most closely reflects; except for Division 7, the percentage of women members is less than 24. The expected number of women authors can be calculated by multiplying the number of authors for each journal by the percentage of female membership in the related division; the result is 706, or 16% of the total, very close to the obtained value. By inference, then, the average woman in the divisions represented in Table 1 publishes research about as frequently as the average man.

The value of 15% women authors can also be compared to the 5% of editors who are women: Women are not selected as editors as often as one would expect from their numbers as authors.[2]

CONTRIBUTION TO PUBLISHED RESEARCH

It may be argued that women are less frequently initiators of research, more frequently assistants and technicians. This argument received some support from the finding that, in this sample, 55.4% of the males were first authors, but only 46.4% of the females were first authors.

However, as Table 1 shows, for 29 of the 33 comparisons, the percentage of first authors who are women for a given journal year is greater than the percentage of editors who are women in that journal year. The percentage of all first authors in the sample who are female is 12.5%, compared to the 4.9% of all editors who are female.

QUALITY OF RESEARCH

A hypothesis to account for the discrepancy between the proportions of women editors and authors is that the work published by women is on the average of lower quality. A readily obtainable and reliable index of quality of research is frequency of citation; Myers (1970), investigating the relation between journal citations and scientific eminence, concluded, "The evidence . . . shows that psychologists who are judged to be scientifically eminent on the basis of a variety of other independent criteria are also those most frequently cited in the current journal literature [p. 1047]." This finding suggested that the quality of contributions from women psychologists could be assessed by comparing the citation rates for men and women authors: If women match men in the quality of research, their work should be cited as frequently.

This hypothesis was studied for one journal, the *Journal of Experimental Psychology*. As an APA journal, a journal of high prestige, and one that publishes research from a variety of fields, it also seemed to be a journal of interest to many psychologists. For the years 1961 (Volume 61) and 1971 (Volumes 87 and 88), citations were counted, following the rules listed by Myers (1970): (*a*) No self-references were counted; (*b*) only one citation per author was counted from the list of references for a given article; (*c*) one citation was counted for each author of multiple-author references. Additionally, no references to unpublished work were included in counting citations. The name of the author for each citation was classified by sex. A name originally unclassifiable (because only initials were given and the sex of the author was unknown to the classifier) was checked by looking at the cited reference if it was available.

For 1961, there were 826 citations, of which all but 9% were classified eventually. Of the 826, 42% were from references 1–5 years old; 31%, 6–10 years old; 10%, 11–15 years old; the remainder, 17%, were older than 15 years. Of the 753 classifiable citations, 9.8% were female. Figure 1 shows percentage of female citations for five-year intervals preceding 1961. In the volume from which these citations came, there were no women editors, while 15% of the authors and 9% of the first authors were women.

[2] A similar disproportion between percentages of editors and authors who are female was reported by Schuck (1970) for the *American Political Science Review* from 1960 to 1969.

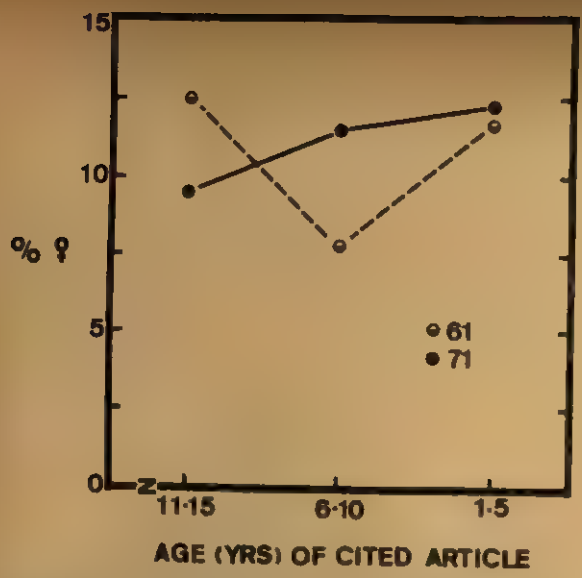

Figure 1. Percentage of female authors of articles cited in the *Journal of Experimental Psychology* in 1961, Volume 61, and 1971, Volumes 87 and 88, classified by the age of the article at the time it was cited.

For 1971, there were 1,847 citations, of which 12% remained unclassified. Of these 1,847 citations, 51% were from references 1–5 years old; 29%, 6–10 years old; 10%, 11–15 years old; the remainder, 10%, were older than 15 years. Of the 1,618 classifiable citations, 11% were female. Figure 1 shows percentage of female citations for five-year intervals preceding 1971. In the volumes from which these citations came, there were 7% women editors, while 11% of the authors and 10% of the first authors were women.

Both in 1961 and 1971, the percentage of cited authors who were women was about 10. The percentage of women authors dropped from 15 to 11, but the percentage of women first authors remained the same, at about 10. Thus, women are cited in the *Journal of Experimental Psychology* about as frequently as one would expect from the proportion of women authors, although proportion of women first authors is less variable and perhaps more closely related to citation rate. There is no support here for the contention that women publish inferior work, and thus none for the hypothesis that a paucity of women editors reflects the poorer quality of women's research.

How far the results for this one journal can be generalized to other journals is unknown. At the least, one can say that the failure of the "lack of quality" hypothesis to account for disproportionately few women editors for one journal makes it a less attractive hypothesis to account for the more general finding.

AGE

It has been suggested that the women who comprise 15% of the authors in these journals are, on the average, younger than the men who account for the other 85%. If a man has been professionally active longer than a woman, he is more likely to be chosen for an editorial position, other things being equal. There is no direct evidence on the age hypothesis. However, for the *Journal of Experimental Psychology*, percentages of women editors and authors were tabulated for 1966 (Volume 71). Thus, for 1961, 1966, 1970, 1971, and 1972, respectively, we have percentage of women authors: 14.9%, 13.4%, 10.5%, 11.0%, and 15.3%; percentage of women first authors: 9.2%, 7.2%, 12.5%, 10.2%, and 15.5%; percentage of women editors: .0%, .0%, 5.8%, 6.8%, and 8.1%. That is, there is no indication that the proportion of women authors has been increasing, as might be anticipated given the hypothesis that current women authors are younger than their male colleagues. It is also noteworthy that the citation rates for women remain relatively constant over time, as Figure 1 illustrates. There is thus no support for the notion that, while the proportion of women authors has remained constant, the quality of their research has improved. These data indicate that about 10%–15% of the work in the *Journal of Experimental Psychology* is contributed by women, that about 10% of the work cited is contributed by women, and that this has been true for at least the past 12 years. Over the same time period, the proportion of women editors has risen slowly, but has not yet reached the level to be expected on the basis of women's contributions.

RATE OF PUBLICATION

Perhaps women publish papers at a lower rate than do men. Two journals with a high percentage of women authors, the *Journal of Experimental Child Psychology* and the *Journal of Verbal Learning and Verbal Behavior*, were selected. For each journal, each name in the author indexes from 1965 to 1969 was classified as male or female; unclassifiable names were classified by directory search and inquiry only for multipaper authors; authors with foreign affiliations were not eliminated.

Each name was then classified according to the number of papers published in that journal in five years. Table 2 shows the percent frequency distributions for male and female authors for the two journals. Although the distributions for men and women were quite similar, a larger proportion of women than of men published only one paper, and there were proportionally more men who published four or more papers.

For each journal separately, the conditional probability that a male author would be an editor, given that he had published *n* papers, was calculated; for both journals, as *n* increased, the probability of being an editor approached 1. For each *n*, the conditional probability was multiplied by the number of women authors who had published *n* papers; summing over *n*, one obtains an estimate of the number of women editors expected on the basis of their publication rates in the absence of sexual bias in selection. For the *Journal of Experimental Child Psychology*, 8.1 is the expected number of women editors, compared to the actual numbers of 4, 3, and 3.5, for 1970, 1971, and 1972, respectively. For the *Journal of Verbal Learning and Verbal Behavior*, the expected number is 3.7; the actual numbers are 0, 0, and 1, for 1970, 1971, and 1972, respectively.

TABLE 2

Percent Frequency Distributions by Author's Sex of Number of Articles in One Journal from 1965 to 1969

n	*Journal of Experimental Child Psychology*		*Journal of Verbal Learning and Verbal Behavior*	
	% male	% female	% male	% female
1	68	83	73	79
2	22	12	14	12
3	7	5	6	6
≥4	3	0	7	3

COMPOSITION OF THE SAMPLE OF JOURNALS

In selecting Sample A, I followed the lead of other authors who have made similar selections on grounds that the journals represented a variety of fields and were of interest to psychologists (e.g., Jakobovits & Osgood, 1967; Mace & Warner, 1973; Myers, 1970; Over & Smallman, 1973). It may be objected, however, that since the basis for selection is not precisely specified, the sample is of unknown composition, and thus the results are not generalizable. To answer this objection, a new sample of all APA journals in 1972 was specified (with the exceptions of the *Psychological Bulletin*, the *Psychological Review*, and the *American Psychologist*, which have no editorial boards of fixed membership, and *Contemporary Psychology* and *Psychological Abstracts*). This is referred to as Sample B. The same classification of editors and authors by sex was carried out on this new sample. The results are shown in Table 1. The nine journals had 24 editors, principal or associate, of whom none were women. Of the total of 289 editors, 20.5 (one served for half a year), or 7.2%, were women. The total number of classifiable authors was 1,701, of whom 244, or 14.3%, were women; on the basis of APA division membership (excluding the *Journal of Applied Psychology*, for which there is no corresponding division), one would predict 18.6% female authors. The total number of classifiable first authors was 897, of whom 106, or 11.8%, were women; 54.5% of men were first authors, but 42.6% of women were first authors. For eight of the nine journals, the percentage of first authors who were women was greater than the percentage of editors who were women.

A comparison to Sample A shows that Sample B, the APA journals, has a higher proportion of women editors—7.2% to 4.9%— and a very similar proportion of women authors and first authors —14.3% to 14.6% and 11.8% to 12.5%, for B and A, respectively. The conclusion reached for Sample A is not changed by specifying different criteria for inclusion in the sample: In 1972 APA journals, women were not selected as editors as often as one would expect from their numbers as authors or as first authors.

Some Conclusions and a Suggested Index of Sexual Equality

There are several conclusions to be drawn from these data. The most general are those based on two samples of journals: Sample A, 11 journals in 1970–1972, and Sample B, APA journals in 1972:

1. Although there is variation from field to field, women are represented as authors of published research about as often as one would expect from their division membership in APA.

2. For Sample A, 5% of the editors are women, while 15% of the authors and 12% of the first authors are women. For Sample B, 7% of the editors are women, while 14% of the authors and

12% of the first authors are women. Women are not represented as editors as often as one would expect from their numbers as authors.

Some other conclusions are more tentative, being based on smaller samples of one or two journals:

3. For one journal (the *Journal of Experimental Psychology*) and two years (1961 and 1971), the percentage of citations to women authors is consistently about 11% for a period up to 15 years prior to the date at which an article is cited; this percentage corresponds closely to the percentage of women first authors for the journal. There was no trend, from 1961 to 1972, of increasing proportions of women authors in this journal, nor any trend of increasing citation rates for women authors. Neither relative age of women authors nor relative quality of women's research can account for the discrepancy between percentage of women editors and percentage of women authors for this journal.

4. There are some differences in publication patterns between men and women. Based on the full sample of journals, men are more likely than women to be first authors of published research. Based on a sample of two journals from 1965 to 1969, women are more likely than men to have published only one paper, whereas men are more likely than women to have published four or more papers, in a particular journal over a five-year period. The differences in publication patterns lead one to expect that the percentage of editors who are female will be somewhat less than the percentage of authors who are female. However, the magnitude of the discrepancy seems too great to be accounted for by these differences.

AN INDEX OF SEXUAL EQUALITY

To facilitate comparison between journals with differing percentages of women authors, an index can be constructed for each journal by comparing the obtained percentage of women editors with the percentage to be expected in the absence of sexual bias in the selection process. The percentage of women authors probably somewhat overestimates the expected value for editors, and the data are too fragmentary to permit a correction for rate of publication. However, a correction in the appropriate direction can be accomplished by taking percentage of first authors who are women as an estimate of the expected percentage of editors who are women. (This approximation gains plausibility from the fact that the percentage of women authors corresponds closely to the percentage of women cited authors for at least one journal.) Then the ratio (*percentage women editors*)/(*percentage women first authors*) takes values from 0 to 1, with a value of 1 indicating complete equality of opportunity for women in editorial appointments (when the expected value exceeds the obtained value, presumably by chance, the index value is defined as 1). In 1970, for Sample A as a whole, this index of sexual equality is .37; for the individual journals, the median value is .35. In 1971, the index of sexual equality is .36 for Sample A as a whole; the median for individual journals is .36. In 1972, the index of sexual equality is .42 for Sample A as a whole; the median for individual journals is .36. There was little or no change in opportunity for women in Sample A from 1970 to 1972.

For Sample B (APA journals) in 1972, the index of sexual equality for the sample as a whole is .61; the median index of sexual equality for individual journals is .52. (The comparable figures for non-APA journals in Sample A for 1972 are .36 and .36.) APA journals currently exhibit less sexual bias in selection of editors than the other journals sampled, but they do not yet offer complete equality to women.

The present data suggest nothing about *how* such bias in the selection of editors might operate. White (1970) discussed the difficulties faced by women in becoming part of a professional "establishment." Perhaps women psychologists are less likely to be suggested or considered by colleagues for the post of editor for some of the reasons outlined by White and by Lewin and Duchan (1971): the reluctance of men to "sponsor" women professionally, or to engage in informal professional interactions with them; the tendency of men to doubt that women (but not men) can handle professional commitments without neglecting family commitments, or to question whether women (but not men) will be compatible with male colleagues.

The analyses presented here suggest that selection procedures for editors of two samples of psychological journals for 1970–1972 were not sex blind, and that, as a consequence, similar publication records for men and women had different outcomes. And it is a plausible conjecture that the higher probability that a male psychologist will receive the recognition of an editorial appointment also increases the probability that he will maintain a high publication rate. There are not enough women editors, not only from the standpoint of

women psychologists themselves, who must find the lack of recognition for professional achievement discouraging, but also from the standpoint of the profession, which ought to be concerned to maximize the productivity of all psychologists, not just the three quarters who are men.

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New England Psychological Association

Proceedings of the Thirteenth Annual Meeting

BARBARA ROSS *Secretary-Treasurer*
University of Massachusetts at Boston

The Thirteenth Annual Meeting of the New England Psychological Association was held November 9–10, 1973, at the University of Massachusetts at Boston. There were 314 registrants.

The Steering Committee and Officers planned the program and selected the topics. Chairmen organized their respective sessions. Celia Moore, University of Massachusetts at Boston, served as Local Arrangements Chair; Mortimer H. Appley, University of Massachusetts at Amherst, as Elections Committee Chair; Robert C. Birney, Amherst College, as Student Liaison Officer; and Arthur M. Kaplan, University of Maine, as Program Coordinator.

Officers for 1973–1974 are: President, Mortimer H. Appley, University of Massachusetts at Amherst; President-elect, Robert C. Birney, Amherst College; Past President, Ethelyn H. Klatskin, Yale University Child Study Center; Secretary-Treasurer, Barbara Ross, University of Massachusetts at Boston. Newly elected members of the Steering Committee (1974–1976) are Zella Luria, Tufts University; Freda Rebelsky, Boston University; and Seymour Wapner, Clark University.

All accredited institutions of higher learning in New England were again invited to nominate outstanding undergraduate junior or senior psychology majors for possible election as NEPA Undergraduate Fellows and Honorary Student Members. The students selected will be announced to members in the spring letter.

In addition to the traditional program format, Working Lunches were held on Saturday on a variety of topics, including a session for chairmen of psychology departments. A book exhibit was also available.

Ethelyn Klatskin, President, chaired the annual business meeting of the Association. Reports were heard from committee chairmen and the Secretary-Treasurer. Following the business meeting, the Donald G. Marquis Memorial Lecture, "A Century of Psychotherapy, Plus or Minus a Decade," was given by Roger Brown, Harvard University.

The Fourteenth Annual Meeting will be held November 15–16, 1974, and is tentatively scheduled at the University of Massachusetts at Boston.

PROGRAM

Paper Sessions

Peripheral Vision

JOHN L. KOBRICK, U.S. Army Research Institute of Environmental Medicine, Natick, Massachusetts, Chair

Effects of Environmental Stress Factors on Peripheral versus Central Visual Response. JOHN L. KOBRICK.

Overview of Problems in Peripheral Vision Research and Some Approaches. LEWIS O. HARVEY, Massachusetts School of Optometry, Boston.

Effects of Large Moving Fields: Peripheral versus Central Stimulation. RICHARD M. HELD, Massachusetts Institute of Technology.

Temporal Factors in Visual Processing. RUTH RUTSCHMANN, Queens College of the City University of New York.

New Pathways in Attribution Theory

WALTER SWAP, Tufts University, Chair

Attribution and Emotion. JUDITH RODIN, Yale University.

Cognitive Bases of Stereotyping Behavior. SHELLEY TAYLOR, Harvard University.

Attribution. LESLIE MCARTHUR, Brandeis University.

The Rights of Hospitalized People

GEORGE ALBEE, University of Vermont, Chair

Overview of a Dehumanizing System. GEORGE ALBEE.

The Hospitalized Person as Research Subject. STEVEN GOLDSTEIN, University of Vermont Medical Center.

The Impact of the Resident Rights Movement on Institutional Behavior Therapy Programs. BERNARD SAPER, Bangor Mental Health Institute and University of Maine, Orono.

Victimizing the Poor. WILLIAM RYAN, Boston College.

Recent Epidemiological Findings: Some Implications for Patients' Rights. LENORE K. MORRELL, Laboratory of Community Psychiatry, Harvard University.

Discussant:
MARJORIE BARNETT, Mental Patients Liberation Front, Boston, Massachusetts.

Maternal Behavior

J. S. ROSENBLATT, Institute of Animal Behavior, Rutgers University, Chair

A Theory of Maternal Behavior Derived from Animal Studies. J. S. ROSENBLATT.

Synchrony in the Early Mother–Infant Relationship. EVELYN THOMAN, Biobehavioral Sciences, University of Connecticut.

Clinical and Theoretical Aspects of Early Mother–Infant Interaction. KENNETH ROBSON, Tufts New England Medical Center.

Person–Environment Assessment and Its Implications for Community Psychology: Theory, Research, Practice, Training

STUART E. GOLANN, University of Massachusetts at Amherst, Chair

Conceptual Approaches and Empirical Data on Person–Environment Assessment: A Current Summation. HAROLD L. RAUSCH, University of Massachusetts at Amherst.

Assessment of Individuals in a School Setting: An Ecological Conception of Psychological Evaluations. EDISON J. TRICKETT, Department of Psychology and Institution for Social and Policy Studies, Yale University.

Taking Ourselves Seriously: Implications for Training. DAVID M. TODD, University of Massachusetts at Amherst.

Discussant:
IRA GOLDENBERG, Harvard University.

Cognitions, Intervention, and Public Policy

ERNESTO POLLITT, Growth and Development, Nutrition and Food Science, Massachusetts Institute of Technology, Chair

Conceptual Issues Concerning Intervention. CHARLES P. GERSHENSON, Florence Heller Graduate School for Advanced Studies in Social Welfare, Brandeis University.

Child Development and Public Policy: Intervention by Action and Inaction. FREDA REBELSKY, Boston University.

The Nature of Cognitive Development and the Issue of Intervention. JEROME KAGAN, Harvard University.

Working Lunches

Chairmen of Psychology Departments. RICHARD LOUTTIT, University of Massachusetts at Amherst, Chair.

Behavior Modification. PETER WISH, Framingham State College, Framingham, Massachusetts, Discussion Leader.

Counseling Psychology. Division 17 of APA, Conversation Hour. J. ALFRED SOUTHWORTH, University of Massachusetts at Amherst, Discussion Leader.

Historical Development of Psycholinguistics. ARTHUR BLUMENTHAL, Harvard University.

Human Ethology: Observational Studies of Human Behavior. GEORGE MICHEL, Boston University.

Massachusetts Psychological Center. HERBERT J. HOFFMAN, Discussion Leader.

The Biomedical Model in the History of Psychology. OTTO MARX, Boston University School of Medicine, Discussion Leader.

Models of Man: Deception, Role Playing, Obedience. DONALD MIXON, University of Massachusetts at Boston.

Psychoeducational Programs. RONALD L. SEIFER, Saratoga County Mental Health Center, Discussion Leader.

Psychological Tests, Occupational Selections, and Equal Employment Opportunity Commission Guidelines. PAUL COSTA, University of Massachusetts at Boston, Discussion Leader.

Undergraduate Students in Social Service Agencies. BERTON FLIEGEL, University of Massachusetts at Boston, Action Program, Discussion Leader.

Hospitalized Children. BARBARA POPPER, Children in Hospitals, Boston, Massachusetts, Discussion Leader.

Teaching Psychology in Community Colleges and High Schools. DANIEL VAN LEUVAN, Nichols College, Dudley, Massachusetts, Discussion Leader.

The Use of Surveys in Social Policy Evaluation. LAURENCE G. BRANCH, Survey Research Program, University of Massachusetts at Boston, Discussion Leader.

Representation of Information in Human Problem Solving. STEVEN SCHWARTZ, University of Massachusetts at Boston, Discussion Leader.

Presidential Address

In lieu of the Presidential Address, Dr. Klatskin arranged a special lecture in honor of Donald G. Marquis, Past President of NEPA and an outstanding scholar in the field of psychology. The Donald G. Marquis Memorial Lecture given by ROGER BROWN, Harvard University, was entitled "A Century of Psychotherapy, Plus or Minus a Decade."

Comment

What Is the Source of the Fullerton–Cattell "Principle"?

While working on a commissioned chapter for a forthcoming handbook, I became intrigued by a rather common tendency of writers about psychophysics to attribute to Fullerton and Cattell (1892) the statement "Equally often noticed differences are equal unless always or never noticed" (cf. Bock & Jones, 1968, p. 24, who give a page reference; Guilford, 1954, p. 40; or Torgerson, 1958, who stated that the work of Fullerton and Cattell led to a statement similar to the quotation, p. 155). I can find no such statement in Fullerton and Cattell, and to my impression, the whole spirit of their monograph is quite the contrary. In fact, Cattell (1893) concluded a discussion of psychophysics with the following: "I conclude, consequently, that we cannot measure the intensity of sensation and its relation to the energy of the stimulus either by determining the error of observation or by estimating amounts of difference . . . ."

As far as I can discover, and I should like to be corrected if I am wrong, the first attribution to Fullerton and Cattell was by Thurstone (1932) who had, interestingly, already demolished the "principle" without attribution (Thurstone, 1927). As a matter of fact, the one reference to Thorndike (1910) in the 1927 publication probably gives the correct clue. (Actually, this is an "implied" reference. The exact citation is not given.) In reporting his scale of handwriting, Thorndike (pp. 5–6) stated explicitly the assumption attacked by Thurstone. This was the first statement of the point that I can find, although it seems obvious that the idea was "in the air." Perhaps until an earlier reference is found, we should give credit, if it be credit, where it is due, and refer to "Thorndike's Principle"!

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F. Nowell Jones
University of California, Los Angeles

On the Diversification of Graduate Study in Psychology

Having a little free time, I went through *Graduate Study in Psychology 1974–75* (APA, 1973) and, as an exercise in what I now realize was futility, attempted to categorize the various listings as to degree areas cited by each school. I made no differentiation between master's and doctoral programs, and what to my wondering eyes appeared were over 75 different fields of study in which a graduate student in psychology could specialize. I recognize that this is an age of specialization, but this finding was overwhelming. If psychology continues in this manner, it would seem obvious that we are going to become so specialized that those of us in the academic area will wind up being able to teach one course and no more, and those in the professional areas will be able to carry out only one function.

As a professional, I am confused when I read through the handbook and find so many different areas of specialization, and I can only imagine the feelings of a potential student. I realize that I am exaggerating to some extent, but I sincerely believe that we have a problem that requires some solution.

REFERENCE

American Psychological Association. *Graduate study in psychology, 1974–75.* Washington, D.C.: Author, 1973.

Charles C. Mitchell
East Carolina University

The Artifact as Artifact: A Reply to Epstein et al.

Recent research (Epstein, Suedfeld, & Silverstein, 1973) attempted to discover the parameters implicit in the subject–experimenter contract and to assess the degree to which violation of this implicit contract affects experimental performance. The study was conducted in three stages. The first two stages provided important information on the substance of subjects' expectations about psychological experimentation. The third stage studied the effect on task performance of experimenter lateness, a behavior considered by subjects as both undesirable and in-

appropriate. The intention was to contribute to the literature on experimental artifacts (e.g., Barber, 1972; Rosenthal, 1966). However, various statistical and interpretative oversights render the conclusions of this third stage the true artifact.

In each of two experimental sessions, subjects performed a modified WAIS digit-symbol task for four 2.5-minute blocks. In Session 2, the 40 subjects were divided randomly into four groups: One group performed the task as scheduled (no lateness—NL); a second group performed after an imposed 20-minute rest period—the experimenter was not late (waiting control—WC); the third group was forced to wait 20 minutes because of the experimenter's "justified lateness" (JL); and the fourth group was forced to wait 20 minutes when the experimenter was late without explanation (unjustified lateness—UL).

According to the experimental hypothesis, breach of the experimental contract, culpable lateness, should be reflected in the performance differences between the JL and UL groups. *In fact, no such differences were found.* On the basis of a questionnaire returned by the subjects after the experiment, however, the UL group was divided into those who reported that the experimenter had been late (UL-A, $N = 5$) and those who did not (UL-N, $N = 5$). (Note that failure to report lateness as an inappropriate event does not mean that subjects were not aware of the lateness, as the authors assert. These subjects might not have perceived experimenter lateness as inappropriate or might simply have ignored it.) Subsequent analysis of the performance data uncovered a significant "decrement" for the UL-A group. This led to the authors' conclusion that performance was affected by perceived culpable lateness and that care must be taken to avoid unwanted interactions between subjects' expectations and "extraneous" experimenter behavior in the collection of experimental data. Furthermore, it is claimed that subjects in the UL-A group reported significantly more dissatisfaction toward psychological experimentation than did the NL control group. The justification for these conclusions must be scrutinized carefully.

On Reported Dissatisfaction Differences

1. The UL-A group is seen as possibly brighter, more committed, and more observant than the other groups (Epstein et al., 1973, p. 220), so the factors underlying the satisfaction difference are unclear. *No* comparison using the present data can support the authors' contention about reported dissatisfaction: the UL-A subjects were chosen on the basis of a postmanipulation observation. To conclude that they were less satisfied with psychological experimentation after the manipulation, it is necessary to know what their attitudes were *prior* to the manipulation to infer that it had an effect. The use of this group renders untenable any assumption about random assignment and any conclusions drawn from it (Campbell & Stanley, 1966; Glass & Stanley, 1970).

2. The use of the t test to compare the satisfaction of NL and UL-A groups is plainly inappropriate. In the Methods section of the original article, three contrasts were mentioned explicitly for the performance data. It would seem that these contrasts are equally important in analyzing reported dissatisfaction. For example, differences between the NL and WL groups could be attributable to the experience of waiting per se. Subjects who value their time may well resent unnecessary delay. Consequently, an analysis of variance and computation of multiple contrasts are required to test the significance of any comparison. Indeed, under the present circumstances, where 10 pairwise t tests are possible, the probability that at least one will be significant at the .05 level is .40. Granted the loss of power with the appropriate test and the weak significance of the reported t value, the correct test might not have shown a reliable difference.

It is not unlikely that any "sensitization" (Epstein et al., 1973, p. 220) is more apparent than real.

On Performance Decrement

There is equivalent reason to be skeptical about the "performance decrement."

1. It is not clear why Epstein et al. were entitled to choose an ad hoc control group of the six highest scoring Session 1 performers to demonstrate that the UL-A performance decrement in Session 2 did not derive from a ceiling effect. The choice of this group represents yet another after-the-fact decision, impermissible in a rigorous statistical design without appropriate safeguards. Conclusions based on their performance are unsupported at best. Furthermore, Epstein and his colleagues did not demonstrate the comparability of this control group to the UL-A group. With so few subjects, a real difference in ability might not be caught by a statistical test on the Session 1 data. (It is difficult to prove the null hypothesis of no differences.) The authors summarized their conclusions but reported no data, yet their conclusions rested more strongly on the performance of those six subjects than on the group data they did report. Since subjects were initially assigned to three well-defined control groups, it is to the performance of these groups that the authors must appeal to support their conclusions, or else collect additional data.

2. A plot of the Session 2 group means reveals that the "decrement" in performance derives from the score in Block 4. Had this score, which is still higher than that of any other group in the *entire* study, been more in line with the other three UL-A scores, it is not likely that any singular decrement would have been found. Though it is agreed that the Block 4 mean for the UL-A group represents a decrease, it is not clear that it derives from the experimental manipulation. The authors offered no reason why the decrement does not appear until 7.5 minutes after

the start of Session 2. An alternative explanation for these phenomena, which fits the data at least as well as the one offered by Epstein and his colleagues, is not that overt or covert sabotage takes place, but that the UL-A subjects simply tire. In absolute terms, 150 symbols per 2.5-minute block is rather astounding. This is one symbol per second, the maximum rate coded for the WAIS subtest itself (Wechsler, 1955; in fairness, WAIS subjects are not allowed to practice, but they are only required to perform for 1.5 minutes, not 10). Nevertheless, the UL-A subjects, who supposedly are "retaliating," continue to work at this blistering pace for 7.5 minutes. It is not unreasonable to propose that 7.5 minutes of high-level cognitive functioning at a boring and repetitive task would induce fatigue and a corresponding decrease in performance rate. The "fourth-quarter fade" need not be documented for any athletic coach or long-suffering fan. The other subjects, operating at a more comfortable level, would not be expected to show a similar "fade."

3. The Block 4 UL-A score may be simply a random fluctuation. Without a replication, it need not be considered representative of the "true" Block 4 score. When a conclusion which has potentially far-reaching implications for the conduct of psychological research depends on the performance of five subjects for 2.5 minutes, it should be examined closely to rule out alternative explanations. No such disqualification is justified on the basis of the data presented.

4. The sole support for the existence of a performance decrement is provided by the covariance analysis. The authors referred to the "examination of the data" to determine that the UL-A group was unique in displaying a performance decrement. Did they examine the raw means? If not, why are the covariate adjusted means not presented? Without knowledge of these means, it is difficult to make dependable inferences. Was a statistical test performed to support the existence of a unique UL-A decrement? If so, why was it not reported? (It is possible that the covariance analysis for the Session 2 scores was performed only on the UL-A and UL-N groups. If so, it is not legitimate to conclude that the UL-A group is unique in showing a performance decrement. There would be no assurance that the UL-A group is significantly different from the NL, WC, and JL groups.) The meaning of these analyses cannot be discovered from the reported results.

5. The authors seemed to neglect entirely (in drawing their conclusion about performance decrement) that the UL-A and UL-N groups are essentially drawn from different populations, as demonstrated by performance differences at the .01 level for Session 1 scores. Meehl (1969, expanded in 1970) and Lord (1969) have noted the difficulties in attempting to correct for these differences by covariance analysis. Especially when the underlying variables responsible for high UL-A performance (e.g., IQ, motor skill, commitment) are the variables which determine assignment to the UL-A group, the effect of the experimental manipulation and the effect of these uncontrolled nuisance variables must remain confounded. The ascription of any differences to perceived culpable lateness is at the least overzealous.

Although it may be demonstrable that violation of the implicit contract between experimenter and subject can vitiate the results of psychological experimentation, such a demonstration is not provided by the research considered here.

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James M. Kilkowski
University of Minnesota
Michael W. Kruger
University of Michigan

Ecological Knowledge and Perception of Environmental Modifiability

Maloney and Ward (1973) presented an objective scale for the measurement of ecological attitudes and knowledge. I would like to elaborate on two specific points made in their article: (*a*) Maloney and Ward found that their measure of environmental knowledge did not correlate significantly with actual commitment. These investigators attempted to explain this lack of a relationship by reasoning that the subject matter of ecology is difficult to understand, and thus it is difficult to develop an ecological knowledge scale with high probability or "easy" items. (*b*) Maloney and Ward stated that their subjects' high verbal commitment to ecological issues "should make fertile soil for the modification of ecologically relevant behaviors [pp. 585–586]."

My research examining correlates of citizen involvement in voluntary,

antipollution activities indicates that (*a*) an ecological knowledge scale providing a wide range of item difficulty has been developed and (*b*) the changing of environmental behaviors based on certain attitudes may be more complex than is suggested in the Maloney and Ward article. I will briefly describe a study in which I hypothesized that people active in antipollution activities would score higher on a test of ecological knowledge and would perceive to a greater extent that something positive could be done to clean up the environment than those not so involved.

The subjects were 96 male and female adults in a southwestern metropolitan area. One third of the subjects were selected randomly from the membership list of a local antipollution group (members). Another third were selected from those people who had been sent a letter of notification of the group but did not join, and the final third had not been notified of the group (nonmembers). Subjects were matched for sex and socioeconomic status (occupation and residence).

As part of a larger study, a series of scales were administered to each subject during an individual interview session. Included with the measures were an ecological knowledge test, an attitude scale to measure perceptions of the degree to which pollution could be lessened (modified), and an involvement activities checklist. The knowledge test was the 27-item, multiple-choice Test of the National Environment, designed and used by the Columbia Broadcasting System, Inc., in a national survey. Each question answered correctly receives 4 points (range 0–108). The Environmental Modifiability Test constructed for the study (Levenson, 1973) was patterned after items loading on a system modifiability factor found in research dealing with the motivational dynamics of black youths (Gurin, Gurin, Lao, & Beattie, 1969). A high score on the Modifiability Test indicates that the subject feels that pollution can be lessened (range 0–36). The Involvement Checklist is a self-report measure consisting of 15 activities. The subject checks those things he has done to express his concern over environmental problems (range, 0–15).

As expected, results indicated that the Test of the National Environment was sensitive to group differences (members: $M = 83.6$, $SD = 8.9$; nonmembers: $M = 71.6$, $SD = 13.2$; $F = 18.43$, $p < .001$), and scores on the knowledge test were significantly correlated with the number of antipollution activities checked by the subjects ($r = .54$, $p < .01$).

Contrary to the second hypothesis, however, those who were involved in nine or more antipollution activities scored significantly lower on the Environmental Modifiability Test ($M = 23.6$, $SD = 6.7$) than those who were involved in four or fewer activities ($M = 27.5$, $SD = 5.2$; $F = 4.01$, $p < .05$). Similar results were found for members versus nonmembers, the latter group being more optimistic about the possibilities of cleaning up the environment (Levenson, 1973).

Two reasons are proposed to explain why those who were less involved perceived more chances for eliminating pollution. First, the items on the Modifiability Test focus on the power of society to cure environmental problems using technological means. This emphasis is consistent with a prevalent value orientation in the United States—a belief in man's dominance over nature (Kluckhohn & Strodtbeck, 1961). During the interview sessions, however, many of those who were involved in antipollution efforts voiced quite opposite views, expressing the belief that man must learn to live in harmony with the environment.

Second, the items on the Modifiability Test tend to accentuate the ease with which pollution can be lessened. Yet those who were the most involved maintained that their efforts were part of a very difficult and complex undertaking which would eventually result in fundamental changes in economic and social values. It appears, then, that the involved subjects' lower scores on the Environmental Modifiability Test should be interpreted as indicating dissapproval of additional technological interference and disagreement with simplistic perceptions of environmental issues.

From these correlational findings, causation cannot be ascertained. However, one may speculate that an optimistic assessment of the chances of eliminating environmental degradation, coupled with high verbal commitment to ecological issues, may in fact encourage a *lack* of action. If such is the case, then the numerous antipollution campaigns, emphasizing the ease with which the environment can be restored, may result in the undesired effect of less citizen involvement in ecological efforts.

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Maloney, M. P., & Ward, M. P. Ecology: Let's hear from the people. *American Psychologist*, 1973, **28**, 583–586.

Hanna Levenson
Texas A&M University

Announcements

New Members and Associates

The name of *Bruce W. Jorgensen* should be added to the list of new Members elected to initial membership in the APA, as of January 1, 1974, making the new total 2,907 (see February issue, pp. 135–147, for original lists).

American Board of Professional Psychology

The *American Board of Professional Psychology*, during the period December 12, 1973, to February 21, 1974, awarded its diploma in the specialities of Clinical and School Psychology to the following:

CLINICAL

Melba Hurd Duncan
Elizabeth Ann Kolin
Selwyn Lederman
Samuel Roll
Wolfgang Schwarz

SCHOOL

Barbara Ruth Slater

In the February issue (p. 153), ABPP listed *Bruce T. Saunders* as a newly awarded Clinical Diplomate. It should have read Diplomate in School Psychology.

Deaths

Norma H. Austin, date unknown
Blake Crider, date unknown
Gerald J. Dowd, April 1, 1973
George H. Estabrooks, December 30, 1973
Margaret Frank, October 17, 1973
John Howland, January 7, 1974
Ruth Porter Lawler, date unknown
Leroi Madison, August 18, 1973
Max Meenes, January 5, 1974
Margaret S. Quayle, June 18, 1973
Blanche M. Rousseau-Evans, January 1974
George C. Seeck, August 31, 1973
Maurice J. Silver, January 30, 1974
Edward J. Sweeney, February 22, 1974
Wellington A. Thalman, January 5, 1974
Neil J. Van Steenberg, February 3, 1974

Fellowships; Predoctoral and Postdoctoral Programs

College of Law and the Psychology Department of the University of Nebraska—Lincoln: Announce the law–psychology graduate studies program leading to both a regular law degree, the Juris Doctor, and a PhD in psychology. This program will typically take a student five years to complete both degrees. The student may take his PhD in any of four areas: general-experimental, social-personality, community-clinical, or psycholegal studies. For further information contact Bruce Dennis Sales, 209 Burnett Hall, University of Nebraska —Lincoln, Lincoln, Nebraska 68508.

Division of Psychology, Department of Psychiatry, University of Rochester Medical Center: Now accepting applications for postdoctoral fellowships in psychology with specializations in (*a*) adult clinical psychology, (*b*) child clinical psychology, and (*c*) community psychology. For further information and application forms write to Robert H. Goldstein, Director of Clinical Training, Division of Psychology, University of Rochester, Medical Center, 260 Crittenden Boulevard, Rochester, New York 14642.

San Bernardino County Department of Mental Health: Now accepting applications for 20 full- and half-time, one-year clinical and counseling, pre- and postdoctoral internships and residencies beginning July 1, 1974. Humanistic, crisis intervention, multidisciplinary community mental health system. Central Unit with 225 employees, contractual affiliation with 22 local community agencies and clinics throughout the county. Individually designed clinical experiences, training seminars, supervision. For information and applications write to Stephen B. Lawrence, Chief Psychologist, Director Psychology Training, San Bernardino County Department of Mental Health, 780 East Gilbert Street, San Bernardino, California 92404.

Society for Research in Child Development: Interdisciplinary research institute from June 17 to July 12, 1974, at the University of Delaware on the topic of reading and child development. Emphasis will be on research hypotheses and strategies that will contribute to an understanding of the reading process. Applications are invited from advanced doctoral students and young faculty in the behavioral sciences, education, and pediatrics. Travel and living expense support provided. Applicants should contact Frank B. Murray, College of Education, University of Delaware, Newark, Delaware 19711.

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Southeastern Psychological Association: May 1–4, 1974; Hollywood, Florida

For information write to:

Edward H. Loveland, Secretary-Treasurer
School of Psychology
Georgia Institute of Technology
Atlanta, Georgia 30332

Midwestern Psychological Association: May 2–4, 1974; Chicago

For information write to:

James H. McHose
Department of Psychology
Southern Illinois University
Carbondale, Illinois 62901

Southwestern Psychological Association: May 2–4, 1974, El Paso, Texas; April 17–19, 1975, Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Rocky Mountain Psychological Association: May 8–11, 1974, Denver; 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque

For information write to:

Irwin Cohen
Veterans Administration Hospital
Mental Hygiene Clinic
1055 Clermont Street
Denver, Colorado 80220

New England Psychological Association: November 15–16, 1974; Boston, Massachusetts

For information write to:

Barbara Ross
University of Massachusetts
100 Arlington Street
Boston, Massachusetts 02116

Conference on Neurobiology of CNS–Hormone Interactions: May 14–16, 1974; Chapel Hill, North Carolina

For information write to:

Walter E. Stumpf and Lester D. Grant
111 Swing Building
University of North Carolina
Chapel Hill, North Carolina 27514

Delaware Valley Group Psychotherapy Society: May 18, 1974; Philadelphia, Pennsylvania

For information write to:

James Pedigo
101 Wooded Lane
Villanova, Pennsylvania 19085

American Society of Adlerian Psychology: May 24–26, 1974; Chicago, Illinois

For information write to:

Eugene J. McClory
Executive Secretary
110 South Dearborn Street
Suite 1400
Chicago, Illinois 60603

Association for the Care of Children in Hospitals: May 29–June 1, 1974; Chicago, Illinois

For information write to:

Myrtha Sice
Publicity, 1974 ACCH Conference
Children's Memorial Hospital
2300 Children's Plaza
Chicago, Illinois 60614

American Association on Mental Deficiency: June 2–7, 1974; Toronto, Canada

For information write to:

George Soloyanis
American Association on Mental Deficiency
5201 Connecticut Avenue, N.W.
Washington, D.C. 20015

Association for the Psychophysiological Study of Sleep: June 6–9, 1974; Jackson Hole, Wyoming

For information write to:

David Foulkes
University of Wyoming
Box 3291
University Station, Wyoming 82071

Society for Psychotherapy Research: June 13–15, 1974; Denver, Colorado

For information write to:

Allen E. Bergin
Faculty Office Building
Brigham Young University
Provo, Utah 84602

Conference on Humanistic Education: August 21–23, 1974; Carrollton, Georgia

For information write to:

David Ryback
Department of Psychology
West Georgia College
Carrollton, Georgia 30117

Association for Humanistic Psychology: August 25–28, 1974; New Orleans, Louisiana

For information write to:

John Levy
Association for Humanistic Psychology
325 Ninth Street
San Francisco, California 94103

Association for Advancement of Behavior Therapy: November 1–3, 1974; Chicago, Illinois

For information write to:

David H. Barlow
305 East 45th Street
New York, New York 10017

American Society of Clinical Hypnosis: November 5–10, 1974; New Orleans, Louisiana

For information write to:

F. D. Nowlin
American Society of Clinical Hypnosis
800 Washington Avenue Southeast
Minneapolis, Minnesota 55414

Psychonomic Society: November 21–23, 1974; Boston, Massachusetts

For information write to:

Frederick A. Mote
Department of Psychology
University of Wisconsin
Madison, Wisconsin 53706

International Conventions

Eighth International Congress of the International Association of Workers for Maladjusted Children: April 29–May 3, 1974; Lausanne, Switzerland

For information write to:

Secretariat: 8° Congres IAWMC
Avenue de Valmont 35
1010 Lausanne, Switzerland

Cheiron: The International Society for the History of Behavioral and Social Sciences: May 31–June 2, 1974; Durham, New Hampshire

For information write to:

Rand B. Evans
Department of Psychology
Conant Hall
University of New Hampshire
Durham, New Hampshire 03824

Thirty-Fifth Annual Meeting of the Canadian Psychological Association: June 12–14, 1974; Windsor, Ontario

For information write to:

Miriam E. Bunt
Department of Psychology
University of Windsor
Windsor, Ontario, Canada

First Worldwide Conference on Medical Sexology: July 3–6, 1974; Paris, France

For information write to:

Hans Lehfeldt
Society for the Scientific Study of Sex
784 Park Avenue
New York, New York 10021

Fourth Annual Conference of the European Association for Behaviour Therapy: July 11–15, 1974; London

For information write to:

Lawrence E. Burns
Department of Psychology
Birch Hill Hospital
Rochdale, Lancaster, OL12 9QB, England

Eighteenth International Congress of Applied Psychology: July 28–August 2, 1974; Montreal

For information write to:

Secretariat of the 18th International Congress of Applied Psychology, Inc.
C. P. 242
Station Youville
Montreal, Quebec, Canada

Eighth Congress of the International Association for Child Psychiatry and Allied Professions: July 29–August 2, 1974; Philadelphia, Pennsylvania

For information write to:

Albert J. Solnit
Yale University
333 Cedar Street
New Haven, Connecticut 06510

Fifth International Conference on Humanistic Psychology and Second Indian Conference on Humanistic Psychology: August 1–4, 1974; Vishnakapatnam, India

For information write to:

Carmi Harari
285 Central Park West
New York, New York 10024

Second International Conference of the International Association for Cross-Cultural Psychology: August 6–10, 1974; Kingston, Canada

For information write to:

J. W. Berry
Psychology Department
Queen's University
Kingston, Ontario, Canada

Fourth International Conference on Social Science and Medicine: August 12–16, 1974; Elsinore, Denmark

For information write to:

P. J. M. McEwan
Centre for Social Research
University of Sussex
Falmer, Brighton
Sussex, BN1 9RF, England

International Conference on Dimensions of Anxiety and Stress: September 2–5, 1974; Athens, Greece

For information write to:

Irwin G. Sarason
Department of Psychology NI-25
University of Washington
Seattle, Washington 98195

First International Congress on Obesity: October 9–11, 1974; London, England

For information write to:

Kim Solly
1st International Congress on Obesity
22 Montagu Street
London, W1H 2BR, England

Sixtieth Annual Convention of the International Association of Pupil Personnel Workers: October 27–31, 1974; Atlantic City, New Jersey

For information write to:

Joan E. Gaeng
Montclair State College
Upper Montclair, New Jersey 07043

Fourth International Congress of Psychosomatic Obstetrics and Gynecology: October 27–November 2, 1974; Tel Aviv, Israel

For information write to:

Organizing Committee, Congress of Psychosomatic Obstetrics and Gynecology
P.O. Box 16271
Tel Aviv, Israel

Fifteenth Interamerican Congress of Psychology: December 14–19, 1974; Bogotá, Colombia

For information write to:

Liuz F. S. Natalicio
Secretary General
Interamerican Society of Psychology
P.O. Box 88 UTEP
El Paso, Texas 79968

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29–January 1, 1975; Nairobi, Kenya

For information write to:

S. H. Irvine
College of Education
Brock University
St. Catharines, Ontario, Canada

International Conference on Psychological Stress and Adjustment in Time of War and Peace: January 6–10, 1975; Tel Aviv, Israel

For information write to:

Norman Milgram
Department of Psychology
Tel Aviv University
P.O. Box 16271
Tel Aviv, Israel

Tenth International Congress of Gerontology (and Geriatrics): June 22–27, 1975; Jerusalem, Israel

For information write to:

The Congress of Gerontology
P.O. Box 16271
Tel Aviv, Israel

Eighty-Second Annual Convention of the American Psychological Association

Convention Information

THEODORE G. DRISCOLL, JR. *Convention Manager*
CARL N. ZIMET *Chair, Board of Convention Affairs*

This is New Orleans! Air conditioning . . . Al Hirt . . . Andrew Jackson . . . antebellum plantations . . . antiques . . . Antoine's . . . Arnaud's . . . Audubon Park . . . bananas Foster . . . Basin Street . . . Battle of New Orleans . . . bayous . . . Bourbon Street . . . breakfast at Brennan's . . . Café du Monde . . . café au lait and beignets . . . Canal Street . . . cemeteries . . . chicory coffee . . . Commander's Palace . . . Court of the Two Sisters . . . courtyards . . . Creole cuisine . . . dixieland . . . female impersonators . . . five-cent telephone calls . . . French Market . . . French Quarter or "Vieux Carre" . . . funeral marches . . . Galatoire's . . . Garden District . . . gumbo . . . hot and humid . . . Jackson Square . . . jazz . . . Jean Lafitte . . . Jim Garrison . . . lace balconies . . . "ladies" of the night . . . Lake Pontchartrain . . . levees . . . Longue Vue Gardens . . . the Longs of Louisiana . . . Mardi Gras . . . mausoleums . . . Old Absinthe House . . . oysters Rockefeller . . . Pat O'Brien's . . . pecan pralines . . . Pete Fountain . . . prawns . . . Preservation Hall . . . Ramos gin fizz . . . river boats . . . St. Charles streetcar . . . streetcar named Desire . . . Sugar Bowl . . . Superdome, finished or unfinished . . . topless dancers . . . voo-doo.

And that is where the 1974 Convention of the American Psychological Association will be held. The dates of the Convention are **August 30** through **September 3.** While August is not the ideal time to be in New Orleans, the attractions of the city should compensate for the heat and humidity. Meetings are scheduled in the **Fairmont Hotel,** the **International Hotel,** the **Marriott Hotel,** the **Monteleone Hotel,** the **Royal Orleans Hotel,** and the **Rivergate Exhibition Center.** The bulk of the Convention program is scheduled between 9:00 a.m. and 6:00 p.m. during the five days of the meeting. Because hotels are fairly close to one another, APA is not planning to run a shuttle bus among the hotels. Public transportation buses run up and down Canal Street every few minutes at a cost of 15 cents per person.

Canal Street, New Orleans' main thoroughfare, separates the French Quarter from the business district and uptown area of the city. There are historical sights, shops, nightclubs, and restaurants within the French Quarter. In order to sample the cuisine in local restaurants it would be wise to make reservations for all meals. A handy reference on inexpensive and good restaurants is *The New Orleans Underground Gourmet* by Richard H. Collin.

Transportation from the New Orleans International Airport to the downtown area is available by taxicab or limousine. It takes about 30 minutes to make the trip. The taxicab fare is approximately $9 plus tip for one to three persons; there is an additional charge of $3 per person for more than three. Limousine service is available directly to the hotels at $3 per person. Public transportation is also available from the airport for a fare of 35 cents, but it may be necessary to transfer to another bus within the city in order to get directly to your hotel. See you in New Orleans!

Housing

In cooperation with APA, all of the hotels have set aside, at guaranteed rates, substantial blocks of rooms. These hotels have guaranteed rates for the Convention only when registration is made through the APA Housing Bureau on the official Hotel Reservation Form. Also, these rates are guaranteed only when the Advance Registration Form and Hotel Reservation Form are returned *prior to August 1, 1974.* After August 1, every effort will be made to assign rooms at the guaranteed rate, but such assignments cannot be certain, and it is quite unlikely that late requests can be honored.

Members should also bear in mind that with the large number of rooms used, some members may not be assigned a room in the specific hotel they requested nor the type of room at the rate requested. It is advisable to return the Advance Registration Form and the Hotel Reservation Form as early as possible. These forms appear at the end of this section.

All housing forms are sent from the Central Office to the New Orleans Convention Bureau for room assignments. We anticipate a three- to four-week lag between receipt of request in the APA office and your receiving confirmation from the hotel. If after four weeks you have not heard from New Orleans, please contact *di-*


New Orleans
Business District
and French Quarter
Downtown Hotels
© 1970 Greater N.O. Tourist
and Convention Commission
INTERSTATE 10
WILLOW
CLARA
MAGNOLIA
S. ROBERTSON
FRERET
LA SALLE
S. LIBERTY
SIMON BOLIVAR
S. SARATOGA
S. RAMPART
DRYADES
BARONNE
CARONDELET
ST. CHARLES AVE.
PRYTANIA
MAGAZINE
MELPOMENE
TERPSICHORE
THALIA
ERATO
CLIO
EARHART BLVD.
PONTCHARTRAIN EXPRESSWAY
TO INTERSTATE 10
TO MISSISSIPPI RIVER BRIDGE
UNION STATION
POST OFFICE
CIVIC CENTER
LAFAYETTE
POYDRAS
PERIDIDO
GRAVIER
TULANE AVE.
S. CLAIBORNE AVE.
N. CLAIBORNE AVE.
S. ROBERTSON
N. ROBERTSON
N. VILLERE
MARAIS
TREME
CROZAT
BASIN
RAMPART
UNIVERSITY
LOYOLA AVE.
O'KEEFE AVE.
BARONNE
GIRUD
JULIA
ST. JOSEPH
HOWARD AVE.
LEE CIR.
CHURCH
CAMP
LAFAYETTE SQUARE
PICAYUNE
MAGAZINE
BELTRAME
NOTRE DAME
COMMERCE
S. PETERS
FULTON
S. FRONT
DELTA
CONSTANCE
TCHOUPITOULAS
POEYFARRE
CALLIOPE
CANAL ST.
CLEVELAND
LOUIS
CONTI
LAFITTE
ST. PETER
ORLEANS
MUNICIPAL AUDITORIUM
BEAUREGARD SQUARE
ST. CLAUDE
N. RAMPART
BURGUNDY
DAUPHINE
BOURBON
GOV. NICHOLLS
BARRACKS
ESPLANADE
TOULOUSE
BIENVILLE
IBERVILLE
ROYAL
CHARTRES
JACKSON SQUARE
DECATUR
FR. MKT. PL.
N. PETERS
N. FRONT
WELLS
FRENCH MARKET
MISSISSIPPI RIVER
RIVERGATE
INTERNATIONAL TRADE MART

rectly the New Orleans Convention Bureau, 334 Royal Street, New Orleans, Louisiana 70130.

Wheelchair Accessibility

To facilitate making hotel reservations for those people concerned with wheelchair accessibility, Division 22, the Division of Rehabilitation Psychology, compiled the following information on the accessibility of some of the Convention hotels. Wherever possible, the information has been corroborated by site visits made by knowledgeable persons. Detailed information about the accessibility of various churches, restaurants, museums, and stores may be obtained by writing for a free copy of *Guide to New Orleans for the Handicapped* from Louisiana Chapter, National Society for Crippled Children and Adults, Inc., 843 Carondelet Street, New Orleans, Louisiana 70130.

Rivergate Exhibition Center

Main entrance: Accessible
Restrooms: Accessible
No sleeping rooms

International Hotel

Main entrance and elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Two meet accessibility requirements and *must be requested.*

Monteleone Hotel

Garage entrance and three elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Hotel has 56 accessible sleeping rooms which *must be specifically requested.* They are rooms on the fifth floor in the following series: 53, 56, 57, 58, 63, and 69.

Fairmont Hotel

One of the main entrances to the hotel is ramped.
Public restrooms: Women's—accessible
Men's—inaccessible
Sleeping rooms: Inaccessible

Royal Orleans Hotel

Main entrance: Assistance available to negotiate the steep ramped entrance from garage and staired main entrance.
Elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Inaccessible

Marriott Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Royal Sonesta Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Persons requesting wheelchair-accessible accommodations should check the appropriate box on the Hotel Reservation Form as well as indicate the hotel and type of accommodation requested.

Registration

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting, but advance registration represents a saving to the registrants as well as the opportunity to obtain desired hotel space and rates. At its meeting on January 18–20, 1974, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For those registering in advance (prior to August 1, 1974):

$30—Nonmember
$20—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For those registering on-site in New Orleans:

$35—Nonmember
$25—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Key to Map of New Orleans

1. Fairmont Hotel
2. International Hotel
3. Marriott Motor Hotel
4. Monteleone Hotel
5. Royal Orleans Hotel
6. Bienville House
7. Bourbon Orleans Ramada
8. Braniff Place
9. Chateau Motor Hotel
10. Dauphine Orleans Motor Hotel
11. de la Poste Motor Hotel
12. Delta Towers Hotel
13. Downtowner du Vieux Carre
14. Downtown Howard Johnson's Motor Lodge
15. Governor House
16. La Salle Hotel
17. Le Pavillon
18. Le Richelieu Motor Hotel
19. Place d'Armes Motor Hotel
20. Prince Conti Motor Hotel
21. Provincial Motor Hotel
22. Royal Sonesta Hotel
23. Saint Louis Hotel
24. Tamanaca (Best Western) Downtown Motel
25. Thunderbird (Quality Inn)
26. Vieux Carre Motor Lodge

Students and other authorized exemptions from these fees appear on the Advance Registration Form.

The convention badge, with name and institutional affiliation, will be mailed in advance of the Convention to those who preregister. Advance registrants will need only to obtain a badge holder at the APA registration area in New Orleans to complete the procedure—thus avoiding possible delays.

Complete member and nonmember registration facilities will be maintained at three locations—the Fairmont Hotel, the Marriott Hotel, and the Rivergate Exhibition Center—according to the following schedule:

Thursday, August 29, 3:00 p.m. to 9:00 p.m.
Friday, August 30, 8:30 a.m. to 5:30 p.m.
Saturday, August 31, 8:30 a.m. to 5:30 p.m.

From Sunday, September 1 through Tuesday, September 3, registration facilities will be available only at the Rivergate Exhibition Center. Hours will be 8:30 a.m. to 5:30 p.m. on Sunday and Monday; 8:30 a.m. to 12 noon on Tuesday.

Placement

The APA Convention Placement Office will be located in the Rivergate-South Hall. Hours will be from 9:00 a.m. to 5:00 p.m., August 30–September 3, except 1:00 p.m. closing day. Fees required are as follows:

Applicants—$2 for reproducing (1) availability information. $3 for use of Convention Placement facilities includes a copy of the *Position Openings Bulletin*. (Nonmembers will be charged $20 for use of Convention Placement facilities.) July 19, 1974, is the deadline for applicant listing. Appropriate fee must accompany listing.

Employers—$5 for reproducing (1) Job Description. $10 for use of Convention Placement facilities includes a copy of the *Availability Notices Placement Bulletin*. July 26, 1974, is the deadline for employer listing. Appropriate fee must accompany listing.

Employers: Please note the addition of the GENERAL OPENINGS category to be used by organizations having widespread needs for psychologists who may be recruited through a central office.

Prelisting in the more widely distributed printed booklet is encouraged, but Xerox forms will be available for on-site listing. The forms, accompanied by the appropriate listing fee, must be received by the date indicated above. Please do not include the Convention Placement facilities fee which will be collected ONLY when you register at the Placement Office. After the above date, Xerox forms will be available for listing, and requests should be directed to the address below.

A complete set of applicant availabilities will be available to employers: $15 for each category—please indicate choice. After the Convention, order from the Convention Placement Office, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Mail orders for the printed *Availability Notices Placement Bulletin* and the *Position Openings Bulletin* are $1.50 per copy (available August 7). Your check (no billing) may be sent to the above address.

Convention Personnel

You may wish to contact one or more of these people before or during the Convention:

Board of Convention Affairs: Carl N. Zimet, Division of Psychology, University of Colorado School of Medicine, Denver, Colorado 80220.

Convention Manager: Theodore G. Driscoll, Jr., Driscoll and Associates, 7109 Masters Drive, Potomac, Maryland 20854.

Assistant for Convention Management: Candy Won, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Convention Placement Office: Jim Beauter, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Convention Child Care Service

Provision has been made for a professional child care service during the 1974 Convention. CON-SERV has been engaged and will offer an extensive program.

Parents desiring to use child care facilities must preregister no later than July 15, 1974. To preregister, complete the form at the bottom of this page. Please note that upon filing of the preregistration form and payment of the advance deposit as indicated on the form, CON-SERV will provide complete detailed information about the services offered and final registration materials necessary to plan for your children's comfort and safety. If for any reason your plans change, the deposit will be refunded in full if CON-SERV is notified of cancellation by August 10.

Some of the essential features of this service, as planned, are indicated below:

- The facility will be located in Braniff Place Hotel and will be open from 8:00 a.m. to 6:30 p.m., August 30–September 3, 1974. While the cost will be about $1.50 per hour per child, APA will subsidize $.75 of this cost. Hence, the cost to parents during the daytime will be $.75 per hour per child.
- If there is sufficient need, the facility will remain open after 6:30 p.m. until 12 midnight to accommodate parents who may wish to leave their children in the facility during evening hours. The parents will bear the full cost during these hours. If the need for evening child care does not justify keeping the facility open after 6:30 p.m., personnel at the facility will make every effort to help individual parents locate qualified babysitters to stay with children in their hotel rooms for those who might need this service. In any event, every effort will be made by CON-SERV to assist parents with evening child care arrangements.
- During the regular daytime hours, a variety of optional tours will also be offered at modest cost to parents. APA will subsidize part of the cost to the extent of $.75 per hour per child as though the child were in the facility.
- In order to evaluate the child care services at past conventions, the Task Force on Convention Child Care would like you to answer the following questions: (*a*) What is your general reaction to past child care facilities? (*b*) What was the least-liked aspect of past facilities? (*c*) What was the most-liked aspect of past facilities? (*d*) What was needed and not provided in past facilities? Please attach your answers to the preregistration form.

Preregistration Form: Child Care Service—APA Convention
New Orleans, Louisiana, August 30–September 3, 1974

Preregistration form must be returned no later than July 15, 1974.

Name of Parent or Guardian: _____

Street Address: _____

City, State, Zip: _____

Name(s) of children to be registered in the child care facility:

Name	Age

If your child has a specific problem or handicap that we should be aware of, that is, speech impediment, retardation, epilepsy, etc., please let us know so that we may make the necessary provisions. _____

Please check this box if you are interested in tour information for your child ☐

_____
Signature of Parent or Guardian

Please return this form to **Ms. Diane Snow, Con-serv, 1318 Arabella Street, New Orleans, Louisiana 70115,** and enclose a check in the amount of $7.50 for one child or $15.00 for two or more children as listed above. Make checks payable to "Con-serv." Indicate amount enclosed:

$_____.

NOTE: This is only your preregistration form. Upon receipt of this form you will be furnished complete registration materials.

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR REGISTRATION ONLY. THE REVERSE SIDE IS FOR HOTEL RESERVATIONS.

General Convention Information — Deadline: August 1, 1974

1. Please indicate those APA divisions to which you belong . . . . . None. . . . . See below for list of divisions.

Check your most senior status regardless of whether you belong to a division

Fellow . Member . . . . . Associate

2. Please indicate the number of persons who are accompanying you to this Convention but who are not completing a registration form and are not accounted for on another registration form. . ..... Adults . Children

How many of them are planning to attend any meetings? .....

3. Please provide the address to which the convention badge is to be mailed. Be sure to fill in this space, even if it is the same as the information given elsewhere on this form.

Street Address

City, State, Zip Code

Registration Fee Status **(check one):**

$30: ☐ Nonmember

$20: ☐ APA Member, Fellow, Associate ☐ Foreign Affiliate ☐ High School Teacher Affiliate

Exempt: ☐ APA Dues Exempt Member ☐ Student ☐ Nonmember Invited Participant ☐ Nonmember Spouse/Dependent of Registrant

Badge Information **(Please Print):**

Institution/Company Affiliation

City and State

PLEASE PRINT (CLEARLY) NAME BELOW FOR VISIBLE LOCATOR

Title	Last Name	First Name	Initial

[illegible] fees are only applicable for advance registration prior to August 1, 1974. After that date advance registration cards received in the APA Central Office will be returned and those wishing to register will have to do so on-site in New Orleans. If possible use this card rather than photocopied forms. Use another card for additional persons who wish to register. Additional cards can be obtained in any issue of the *American Psychologist* from April through July.

PLEASE COMPLETE BOTH SIDES OF THIS FORM

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting but advance registration represents a saving to the registrant. At its January 18-20, 1974, meeting, the APA Council of Representatives approved the following registration fees for the 1974 Convention.

For Those Registering in Advance Prior to August 1, 1974:

$30-Nonmember

$20 APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For Those Registering On-Site in New Orleans:

$35 Nonmember

$25 APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Send completed form and check for registration fee to:

American Psychological Association
1200 17th Street, N.W., Room 601
Washington, D. C. 20036

Make checks payable to APA.

List of divisions for Question 1 on registration form:

1 - General, 2 - Teaching, 3 - Experimental, 5 - Evaluation and Measurement, 6 - Physiological and Comparative, 7 - Developmental, 8 - Social and Personality, 9 - SPSSI, 10 - Psychology and the Arts, 12 - Clinical, 13 - Consulting, 14 - Industrial, 15 - Educational, 16 - School, 17 - Counseling, 18 - Public Service, 19 - Military, 20 - Adult Development and Aging, 21 - Engineering, 22 - Rehabilitation, 23 - Consumer, 24 - Philosophical, 25 - Experimental Analysis of Behavior, 26 - History of Psychology, 27 - Community, 28 - Psychopharmacology, 29 - Psychotherapy, 30 - Hypnosis, 31 - State Psychological Association Affairs, 32 - Humanistic, 33 - Mental Retardation, 34 - Population, 35 - Psychology of Women.

Please fill out this form in COMPLETE detail particularly ARRIVAL and DEPARTURE dates and type of ACCOMMODATIONS.

SEND APPLICATION AS SOON AS POSSIBLE to address shown at bottom of page to insure desired accommodations at convention rates. DEADLINE: August 1, 1974.

[ ] Check here if NO accommodations are requested.

Choice of Hotel

First:

Second:

Third:

Arrival at a.m. p.m.

Departure at a.m. p.m.

Type of Accommodation Requested:

Note: Single rooms are at a premium. Please share a twin bedroom wherever possible.

Single Room @ $

Double Bedroom @ $

Twin Bedroom @ $

Parlor Suite @ $

☐ Check here if wheelchair-accessible room is required.

I will be sharing a room with..
If any of the above will be attending the convention, they must register also unless otherwise exempt.)

Mail confirmation to: Name

Address

City State Zip Code

Telephone Number

Mail this form and check for registration fee to: AMERICAN PSYCHOLOGICAL ASSOCIATION
1200 Seventeenth Street, N.W., Room 601
Washington, D.C. 20036

No telephone reservations will be accepted. Advance Registration and Hotel Reservation forms will be returned if not accompanied by the check for registration fee where required.

PLEASE COMPLETE BOTH SIDES OF THIS FORM

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR HOTEL RESERVATIONS ONLY. THE REVERSE SIDE IS FOR ADVANCE REGISTRATION.

HOTEL AND RATE INFORMATION

HEADQUARTERS HOTELS

Hotel	Single	Double	Twin	3rd Person
Fairmont	$26	$32	$32	$8
International	26	32	32	8
Marriott	26	32	32	3
Monteleone*	23	32	32	4
Royal Orleans	26	34	34	6

OTHER HOTELS

Bienville House	24.50	25.50	28	
Bourbon Orleans	24-32	28-36	36-45	4
Braniff Place	18 50-20	22 50-24.50	24 50-26 50	3
Chateau	17-22	20-24	21 26	
Dauphine Orleans	22.75	27 75	32.75	
de la Poste	26	30	32-35	
Delta Towers	18	24	24	4
Downtowner	22	30	30	
Downtown Howard Johnson	22	27	27	
Governor House	16-17	20-22	25-27	3
La Salle	13	15.50	17	2
Le Pavillon	26	32	32	5
Le Richelieu	22	26	28	
Place D'Armes	18	20-22	22-26	
Prince Conti	18	24-34	24-34	
Provincial	18-21	22-25	23.50-25	
Royal Sonesta	26	32	32	8
Saint Louis	—	—	32	
Tamanaca	15 75-17.75	17 75-19.75	21.75-23 75	
Thunderbird	16-18	18-20	19 22	
Vieux Carre	20 50-21 50	24 50-29.50	26.50 30 50	

*Wheelchair-accessible rooms available See text for details

Rates for suites are regular rates. Rates vary with each hotel and range from $45 to $250.

Persons requesting family plan accommodations should indicate their requirements on a separate sheet of paper and attach it to the housing form. The Housing Bureau will fulfill these requirements if such accommodations are available.

Divisions are assigned to meeting facilities as follows:

Fairmont: 1, 2, 5, 10, 15, 16, 27, 30
Marriott: 3, 6, 7, 14, 17, 20, 23, 25, 28
Monteleone: 24, 26
Royal Orleans: 19, 21
Rivergate-Exhibition Center/International Hotel: 8, 9, 12, 13, 18, 22, 29, 31, 32, 33, 34, 35

Rivergate Exhibition Center: Placement, Exhibits, Convention Locator System

oyers of Psychologists:

If you have or anticipate openings for psychologists, you may wish to list them with the APA Convention Placement Service. The Job Description Form below is for this purpose. Additional copies supplied upon request.

A service fee of $5.00 per insertion will be required for listing in the **Convention Placement Bulletin.** FEE MUST ACCOMPANY COPY—no billing by APA—DEADLINE JULY 26. If you recruit at the Convention, an additional $10.00 fee will be required for use of the Placement Office facilities. This fee payable ONLY when you register at the Placement Office.

The FINAL FORM of your job description below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for publication. Please list your job data of the kind and in the order illustrated. Do not skip any lines. Thank you.

TRATIVE
RMAT

Job title (caps) — Number of openings — Degree required or preferred — Date job begins

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CLINICAL PSYCHOLOGIST: 1 PhD or MA lacking only PhD thesis. Now or Jan. 1975
$14,000-16,000/12 months. Internship + 2 yrs experience in mental health setting; teaching experience desirable.
Teach intro clinical psych to interns, nurses in university hospital: perform diagnostic & therapeutic activities in univ mental health clinic; do research in area of own interest as time allows. Joint appointment in clinic & psych dept possible.
JA Smith, Chairman, Dept Psych, Univ of Erie, Centerville 3, NY.
See him Aug. 30, 31, Sept. 1, 2, am & pm, or write.

Tentative interview schedule (Aug. 30-Sept. 2, am &/or pm, Sept. 3 until 1 pm are the available times).
If none, enter name to whom inquiries may be mailed.

ING COPY—Draft your job description here, following the above guidelines.

1
2
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L FORM—TYPE YOUR FINAL JOB DESCRIPTION WITHIN BOUNDARY ONLY.

Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research ☐ General O nings

Detach on dashed line and return to:
EADLINE: JULY 26, 1974
EE MUST ACCOMPANY COPY

Convention Placement Office, American Psychological Association
1200 Seventeenth Street, N.W., Washington, D.C. 20036

Rivergate Exhibition Center—South Hall, New Orleans, Louisiana

The FINAL FORM of your qualifications below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for printing. A service fee of $2.00 will be required for listing in the **Convention Placement Bulletin. Fee must accompany copy**—no billing by APA—**Deadline is July 19.** A further charge of $3.00 will be made at the Convention for those members who use the Placement Office facilities. **The fee will be $20.00 for nonmembers.**

Please list your qualifications of the kind and in the order illustrated. Do not skip any lines. Thank you.

STRATIVE
ORMAT

Name (caps) — If known, convention address, phone — Home Address

member
mber →
ent →
ience

LEO SCHOPPMEYER, Fairmont, New Orleans, 529-7111. 3456 Shadygrove Drive,
Anytown, Ohio. APA member. PhD 1958 U of Minn.
1963-present: sr clinical psychologist, Fairview Psychiat Hosp, Ohio, diag &
ther wi children & adults, exp'l resch on commun rehab program, teaching
interns. 1960-63: clinical psychol, Brooklyn Mntl Hlth Clinic, NYC, diag &
ther incl supvsn of spec classes for retarded. 1959-60: clin psycho, VA Hosp,
Mpls, Minn., obj & proj test admin, supvsd therapy after 1 yr internship.
1 paper, 2 articles. Desire supervisory clinical psychology position in
forward looking community clinic setting. $18,500. East or midwest.
See Aug. 30, 31, Sept. 1, am & pm, or write.

s,
cations

Tentative interview schedule (Aug. 30-Sept. 2, am &/or pm, Sept. 3 until 1 pm are the available times).
If not attending, so indicate.

RK COPY—Draft your qualifications here, following the above guidelines.

1
2
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NAL FORM—TYPE FINAL COPY OF YOUR QUALIFICATIONS WITHIN BOUNDARY ONLY.

1. Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research
2. Check: ☐ Member ☐ Associate ☐ High School Teacher Affiliate ☐ Foreign Affiliate ☐ Student Journal Group ☐ Non-Member

Detach on dashed line and return to:
DEADLINE: JULY 19, 1974

Convention Placement Office, American Psychological Association
1200 Seventeenth Street, N.W., Washington, D.C. 20036

DIVERSITY

From introductory psych to factor analysis, Saunders has the texts that meet your curriculum requirements.

Leavitt:
DRUGS AND BEHAVIOR

Ideally suited for undergraduate courses in psychopharmacology, this new text covers principles of drug action, drug effects on various types of behavior, the use of chemotherapy in disorders of emotional health, and the effects of various foods on behavior. Emphasizing a behavioral approach, the author discusses: drug toxicity, profiles of drug users, therapy for drug abuse, sensation and perception, learning and memory, and creativity. *By* ***Fred Leavitt,*** *California State University, Hayward. About 275 pp. Illustd. Ready late Summer.* ***Order no. 5695.***

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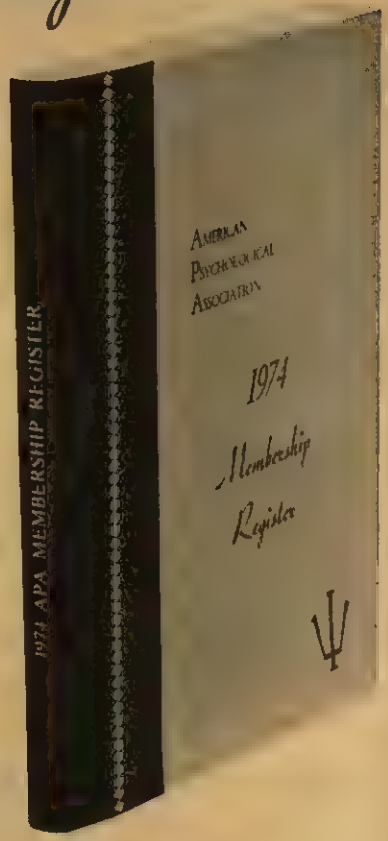

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Open a New Window

Validities and Values in Psychological Measurement

ROBERT M. GUION *Bowling Green State University*[1]

In the musical, Mame tells young Patrick that excitement and a full life come not from unimaginative adherence to routine but from change and new perspectives. She advises him to "open a new window," to "travel a new highway that's never been tried before . . . ." She is too shrewd to leave it entirely to him; she herself opens the windows and doors to introduce new challenges.

Maybe it is too fanciful to place Congress in the role of the uninhibited Mame, or the field of industrial and organizational psychology in the role of the naive Patrick; nevertheless, Congress did open a new door for us when it passed the Civil Rights Act of 1964. It was opened wider by the Office of Federal Contract Compliance and the Equal Employment Opportunity Commission when they issued regulations for the validation of tests and other selection procedures. Their documents directed readers to the *Standards for Educational and Psychological Tests and Manuals* (APA, 1966), but readers found the *Standards* wanting in clarity. So, in the form of a joint committee, the American Psychological Association, the American Educational Research Association, and the National Council on Measurement in Education walked through that door to survey a highway that, even if "tried before," seemed strangely obscure.

Presidential addresses come in two forms. Some are research reports; those of us without long-term research programs offer exhortations. This is the latter type. My exhortation is a plea to psychologists to look over new vistas, but to do so with less than Mame's impulsive enthusiasm. I advocate thorough and careful exploration of new terrain lest we, like Mame's Beauregard, "fall off a damned Alp." My comments are based on my work during the last two years as a member of the joint committee. I speak only for myself, not for the committee. I want to share with you my personal conclusions about the meaning and implications of what we have said about validity. My comments necessarily are elementary because elementary concepts seem so often ignored.

Definitions of Validity

Validity, in the history of testing, has been a confused concept, although the basic ideas have been present from the beginning. Criterion-related, content, and construct validities were all implicit when Galton said,

> One of the most important objects of measurement . . . is to obtain a general knowledge of the capacities of man by sinking shafts, as it were, at a few critical points. In order to ascertain the best points for the purpose, the sets of measurements should be compared with an independent estimate of the man's powers. We may thus learn which of the measures are most instructive [DuBois, 1970, p. 22].

The criterion problem was acknowledged by Hull (1928): "the most formidable problem encountered by the aptitude psychologist is the location of a trial group of subjects from whom a *valid* and reliable quantitative criterion of *aptitude* may be obtained [p. 374, italics added]."

The construct problem was in Brigham's (1930) complaint:

> Most psychologists working in the test field have been guilty of a *naming fallacy* which easily enables them to slide mysteriously from the score in the test to the hypothetical faculty suggested by the name given to the test. Thus, they speak of . . . perception, memory, intelligence, and the like while the reference is to a certain objective test situation [pp. 159–160].

Brigham was particularly critical of combining independent tests and giving the composite a unitary name. If tests are independent, he argued, they

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[1] Presented under the title "Open A New Window" as the presidential address to the Division of Industrial and Organizational Psychology at the meeting of the American Psychological Association, Montreal, Canada, August 29, 1973.

Requests for reprints should be sent to Robert M. Guion, Department of Psychology, Bowling Green State University, Bowling Green, Ohio 43403.

are measuring different things and therefore should not be given a common name. If he were addressing you, his title might come from a different song: "When will they ever learn?"

Constructs, content sampling, and relations with criteria were all intertwined in discussions of validity as the extent to which tests measure what they "purport" to measure (Boynton, 1933; Hunt, 1937; South, 1938). The 1954 *Technical Recommendations* clarified things somewhat, but the "clarification" sparked controversy, such as the opposing positions of Bechtoldt (1959) and Loevinger (1957). Content validity was largely ignored outside of educational circles; industrial psychologists certainly paid little attention to it until the term was thrust upon them in federal regulations. Nearly 20 years after the "clarification," debate still rages over the meanings of these terms. What's more, because of legal issues we have a different term, *job-relatedness*, which must somehow fit into the scheme.

I offer here my understanding of what these terms mean, and I shall illustrate that understanding with examples from a study of packers.[2]

Workers on this job packed their products—golf balls—a dozen to a carton. Two types of assessment were considered as possible predictors. There was a series of anthropometric measures, of which I shall concentrate only on the simplest, arm length. The other was a home grown dexterity test. The criterion was a brief work sample: the time required to pack a set of eight cartons.

CRITERION-RELATED VALIDITY

Criterion-related validity is the extent to which scores on one variable, usually a predictor, may be used to infer performance on a different and operationally independent variable called a criterion. For convenience we often speak of criterion-related validities in terms of correlation coefficients, but the statistic has nothing to do with the definition.

We can describe two of the measures in the packing study in conventional criterion-related terms. Arm length correlates with speed in packing with a coefficient of −.46; the validity coefficient for the dexterity test is −.41. These seem quite satisfactory.

The names of the predictors are not important. The information about validity is in the correlation coefficients and the regression equations on which they are based. The information I have given would have been as complete had I simply said that variables A and B have validities of −.46 and −.41, respectively. The nature of the measurement is not what is important to this statement. The important fact being reported is that these variables can be used to predict job performance within the limits of accuracy defined by the correlation coefficient.

The criterion in this conventional validation study is quite different from the sort of criterion desired by early testers. In contemporary practice, a criterion usually grows out of a given problem. The problem here is straightforward: How can we select people who will package golf balls more quickly? Pioneers in mental measurement phrased a different question: How can we find a measure in real life that will reflect the attribute our test is measuring? They would have accepted job performance as a criterion for dexterity, but looking for a criterion for a measure of arm length would have seemed awfully silly to them.

The point is that criterion-related validities serve two distinctly different purposes. In some cases, as in the early history of testing, the emphasis is on the test. In other cases, as in selection research, the focus is on the criterion. In the first type, one refers to the validity of the test scores. In the latter case, however, the reference is to the validity of the relationship. In the packing study the validity coefficient tells us nothing at all about one measurement other than that it tends to be related to another measurement. It gives as much information about the validity of the criterion as of the validity of the test. This is an uncommon way to look at these correlations, but it is useful; it may point to some understanding of the performance measure. Understanding criteria is every bit as important as understanding tests.

CONSTRUCT VALIDITY

When we speak of understanding, we are talking about construct validity, the degree to which scores may be used to infer how well a stated hypothetical construct describes individual differences among the people tested. Construct validity is not expressible in such simple terms as validity coefficients; it is a judgment based on many kinds of information: procedures followed in developing the test, results

[2] David P. Jones of Bowling Green State University and I plan to report the study elsewhere; here I refer only to those parts of the study that illustrate certain ideas.

of experiments testing specific implications of the construct, and patterns of correlations with other measures. The data used to judge the construct validity of a measure may also help to validate the construct; as data accumulate, ideas about the construct may be modified. Describing a construct is more than merely naming it.

Can any of the measures in the packing study be evaluated in terms of construct validity? What construct is being measured by the criterion? If I suggest that the construct we wanted to measure is productivity, as different from production, then I must define what productivity implies. Productivity may imply, for example, endurance at a high rate of performance over an extended period of time in varying conditions of work. Does the timing of a job sample for a few minutes imply endurance? I think not. We cannot defend the criterion in terms of construct validity unless we define the construct simply as short-term speed in packing golf balls—and in such a tautology we are more interested in reliability.

Can we praise the construct validity of the dexterity test? It so happens that we can. The construct of manual dexterity has been established in experimental and factor-analytic literature. This test has been found in factor analysis to have a substantial dexterity loading (Bourassa & Guion, 1959). This is not enough evidence, but it is some; it places this particular test in a well-established network of relationships.

What about the construct validity of the cloth tape measure used to indicate arm length? Here again is a tautology. What sort of a construct, other than physical distance from one end of an arm to the other, could we possibly have in mind? Of course we could say distance is a construct, simply declare the measurement valid, and forget the whole thing. Actually, construct validity is not a very useful idea for physical measurement. There are well-established, well-defined units by which length is measured. A tape measure is not evaluated by experimental or correlational studies placing their results in some nomological net. It is evaluated in terms of its reliability and of the accuracy of its units. These are quite different questions (Guion, 1965).

Let us examine the same three measures from the point of view of content validity. Content validity refers to the fidelity with which a measure samples a domain of tasks or ideas; it is the degree to which scores on the sample may be used to infer performance on the whole.

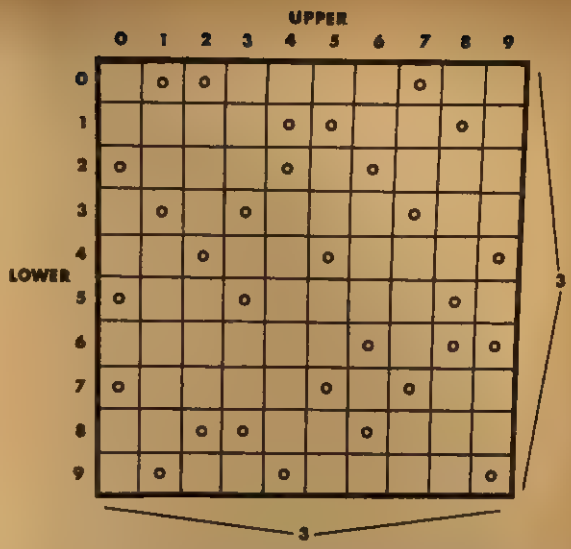

Figure 1. Sampling of arithmetic problems with equally weighted domain.

Consider a test of addition of two single-digit integers. Figure 1 defines the content domain; there are 10 possible digits for the top number and 10 for the lower. The resulting matrix allows 100 possible combinations. If the content domain is defined with every combination weighted equally, then an arithmetic test of less than 100 problems will be a valid sample of the domain if equal numbers of cells are selected for each row and each column of the matrix. With three cells in each column and each row, randomly chosen within this constraint, Figure 1 specifies a 30-item test that will be judged adequate in terms of content validity.

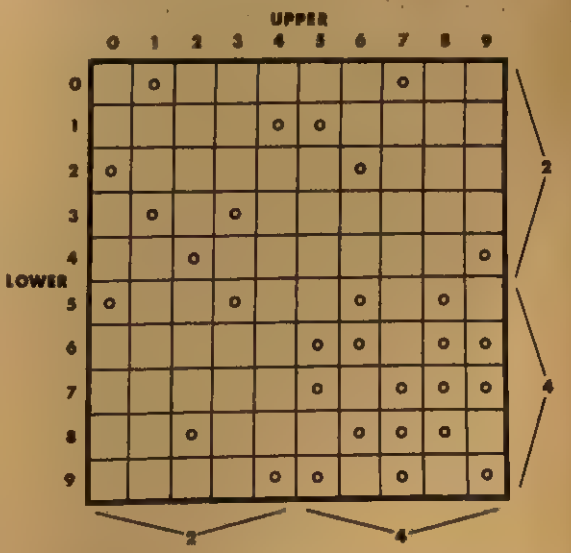

Figure 2. Sampling of arithmetic problems with larger integers more heavily weighted.

It is up to those concerned about content validity to define a domain appropriately for their situation. If someone defines a domain using the same matrix, but differing in the weights to be given, the 30-item test indicated here would have inadequate content validity. Figure 2 shows the changed definition: Problems using the smaller numbers have only half the weight of those using the larger ones. Relatively more items must be selected from the lower right quadrant to yield adequate content validity according to this definition of the domain.

In real industrial problems, content domains are harder to diagram, but the principles are the same. To speak of content validity, there must be a content domain from which to draw a sample. It must be analyzed in terms of elements; elements must be weighted in some way (perhaps in terms of such characteristics as frequency of occurrence, importance, or complexity); and content validity must be judged in terms of how well the domain as defined has been sampled.

The content validity of our packing criterion depends on the definition of the domain of packing tasks. If I define the domain as packing fragile items, this measure lacks content validity, for golf balls are not fragile. If the job of packing golf balls includes crating the cartons, sealing the crates, and stacking them on pallets, and *if I define the content domain as including all of these activities*, then *this* work sample lacks content validity. If, however, I define the content domain simply as the repetitive packing of cartons of golf balls as quickly as possible, then this work sample has substantial content validity.

Can we talk about the content validity of the measure of arm length? Some people will. I will not. Merely recording the number of inches encompassed in the outstretched arm does not, as far as I can tell, sample any defined domain of content. The measure is therefore not reasonably described in terms of content validity.

What about the content validity of the dexterity test? The *construct* of dexterity has been operationally defined with several kinds of related tasks; these tasks could define a content domain. The present test calls on only one of these tasks, so it is not a very good sample. Yet if I am satisfied that I am measuring the construct adequately, I am not likely to worry about content validity. Personally, I tend to think of content validity as a special case of construct validity. Where the task chosen adequately measures the construct, the need for representative sampling of tasks is not apparent to me.

Let us take stock. What has or can be said about validity? First, for any measure there are many validities, not just one. It is and has long been recognized as silly to speak of *the* validity of a test. Second, it is an erroneous shorthand to speak of the validities of the measures themselves; what we really refer to are the validities of *inferences* from the measures (Cronbach, 1971). When we speak of *criterion-related* validity we refer to the use of test scores to infer criterion levels. When we speak of *construct* validity, we refer to the use of test scores to infer degrees to which a particular construct describes the persons, organizations, or objects measured. When we speak of *content* validity, we refer to the use of test scores to infer levels of achievement the persons, organizations, or objects would exhibit in the total domain. These are different facets or "aspects" of validity, and all three kinds of inferences should be valid for most tests. However, the kind of validity statement we seek in any given measurement situation depends on the kinds of inferences we wish to make. This is fundamentally a value judgment. Third, in industrial psychology, the most frequently valued inference is the inference about future performance on a valued criterion. The valued criterion is usually (although not necessarily) either a sample of a performance domain or a performance construct that is identifiably different from the constructs measured by predictors. That is, in employment testing, the validity of major interest is the validity of the *hypothesis* of a predictive relationship between test scores and job performance. Fourth, the validities of criteria need to be investigated. Finally, I suggest that an employment test may provide a basis for inferences that have criterion-related validity, or construct validity, or content validity, or all of these, and still not be job related.

JOB-RELATEDNESS

I am not willing to equate "validity" and "job-relatedness." Criterion-related validity is evidence of job-relatedness only if the criterion measure is a valid measure of overall job performance, an element or sample of performance, or a construct related to job behavior.

Construct validity is evidence of job-relatedness only if the construct is related to the job. To de-

fend an employment test on the basis of construct validity, one must first argue from job or need analysis that a particular construct is related to job behavior. Then he must show that the test has acceptable validity for measuring that construct. What he has at this point is the hypothesis that the construct, as operationally defined by the predictor, can be used to infer levels of valued job behavior. This is a hypothesis of criterion-related validity, and it fairly begs to be tested, "where technically feasible."

If it is clearly feasible to do the criterion-related study, it should be done. Where it is clearly *not* feasible to do the study, the defense of the predictor can rest on a combination of its construct validity and the rational justification for the inclusion of the construct in the predictive hypothesis. Where the issue is not clear, we will argue. My position is that I would rather trust the use of valid measures of predictor constructs in a well-developed hypothesis than a typical, done-for-convenience criterion-related study. I think my preference offers better evidence of job-relatedness. There are some highways, Patrick, that are better not traveled; they include those with small *ns*, low variances, or questionable criteria.

So far, these comments implied an assumption that inexperienced people will be hired who will have to learn the job. The employment test is therefore a *predictor* of performance, not a *measure* of it.[3]

No such assumption is made when content validity is invoked. Either experienced workers will be hired, or applicants will be expected to have already mastered certain prerequisite components of the job through prior training.

The job-relatedness of content sampling depends on the definition of the content domain. In the most extreme case, one might define job content by listing every nontrivial task performed. A representative sample from this complete catalog of tasks clearly has both content validity and job-relatedness. Despite its virtue, however, it can only be used to select people who already know how to handle the job. If the domain is defined more narrowly as a skill or area of knowledge that can reasonably be expected in applicants and is prerequisite to learning necessary additional skills or information, and if a test is a valid sample of this content, then the test is job related to whatever extent the domain it samples is job related.

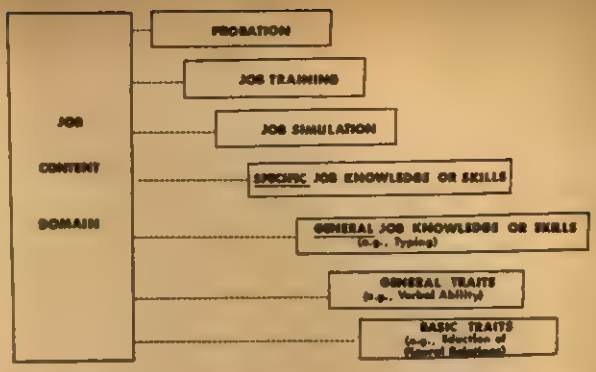

Figure 3. A progression of methods for sampling job-relevant content for the assessment of candidates.

How much abstraction is permissible before one questions whether a test is sampling *job* content? Figure 3 may clarify this problem. The large block on the left represents the defined content domain. The smaller blocks represent different ways to sample from that domain. The most direct sample, and therefore the closest, is a probationary period; what could have higher content validity than a well-planned probationary period? Next is the completion of a training program designed to teach a representative sample of job tasks. The third, a simulation exercise, can be a good sample of job content.

As we move to the right, to the use of conventional tests, the sampling is progressively less direct. The farther one goes in assessment from a direct sampling, the greater the inferential leap necessary to relate test content to job content. Where shall we draw the line? How far can we go and still claim to be sampling the content of the job? For the present, I will say only that the greater the inferential leap required, the less relevant content validity is in assessing the job-relatedness of a test; that is, the domain being sampled validly is a less satisfactory definition of the *job* domain.

I have argued that a measure can be described

[3] In the *Griggs* decision, the Supreme Court has said, "What Congress has forbidden is giving these devices and mechanisms controlling force unless they are demonstrably a reasonable measure of job performance." At first glance, in the context of this discussion, the quoted phrase might be interpreted as requiring content validity. This is an unreasonable interpretation. One cannot "measure" job performance among applicants who have not yet performed on the job. One can *predict* job performance, or one can measure performance on tasks that are samples of the job. Either of these is a job-related use of tests and probably satisfies the intent of the phrase.

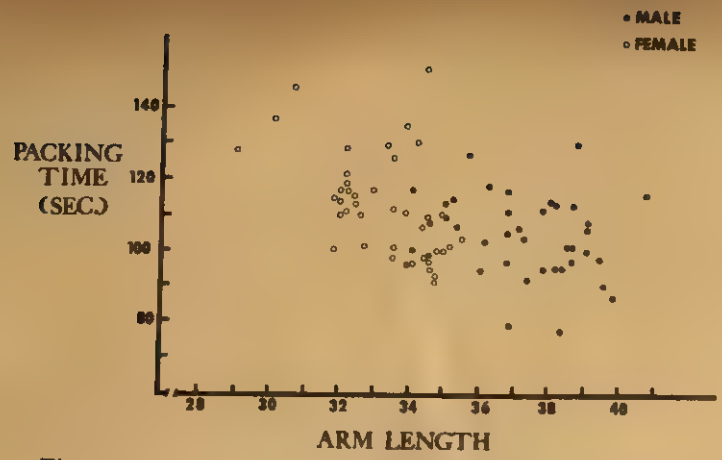

Figure 4. Scatterplot: prediction of speed of performance from arm length.

as valid and still not be job related. I will also argue that a requirement may be job related even where the measurement concepts of validity are not applicable. The usual example of a bona fide occupational qualification, the requirement that the men's room attendant be male, may soon be archaic, but I think it illustrates the point.

Certain educational requirements may also be job related even where the validity of inferences as described in the *Standards* is not especially relevant. Consider only one example: the requirement of a degree in mechanical engineering for one who is to design heavy equipment. Possession of the degree does not reflect any homogeneous construct. Its content validity for a job-content domain is probably low; it includes performance in nonengineering subjects, many of the engineering subjects studied may be irrelevant to the particular job, and many aspects of the job may not have been reflected in the work toward the degree. In short, the knowledge and skill required to obtain the degree is a sample of a different domain from that of the job. Yet, personally, I would consider a degree in engineering related to the job of designing heavy equipment.[4]

When I say this, I am implying a hypothesis that people with the degree are more likely to be able to perform the required tasks than are people without the degree. This is a testable hypothesis, but the interests of society are not well served by hiring a lot of nonengineers to design heavy equipment just to do a criterion-related validation study. Moreover, the logic of the hypothesis seems so clear to me that I would accept the requirement as job related without any further evidence of the validity of the hypothesis.

In short, I view job-relatedness as the extent to which the hypothesis of a relationship between the hiring requirement and job behavior can be accepted as logical. If there are gaps in the logical arguments, or if it is based on weak assumptions, then evidence of validity may be useful to support claims of job-relatedness. But validity and job-relatedness are not the same.

Values in Valid Hypotheses

Job-relatedness is, of course, a digression from the implications of the *Standards*, so let me return to the main theme.

If we recognize that most criterion-related validation is actually the validation of hypotheses arising from real situations, then we must also recognize the influence and nature of the values implied by the hypotheses we choose to examine. If we examine values carefully and if we take them seriously, we may change the hypotheses to be tested. Look again at the packing study.

If we really value speed in packing cartons, we should consider something other than raw validity coefficients. I am sure it has occurred to you that there *is* something else to consider. Arm length is a valid predictor, but it may also be discriminatory against some women, who, being shorter than men, have shorter arms. Now, discriminating against women may be illegal, but in itself it does not violate our production value unless it also reduces

[4] This is not a general endorsement of educational requirements, many of which are highly questionable.

the speed of packing golf balls. Figure 4 shows the scatterplot of arm length against packing time. Solid dots represent data from males; open dots represent data from females. The right-hand part of the chart represents mainly men, the left mainly women, and the composite yields a higher correlation than either because of the increased variance. In a sense, the predictor variable is sex. Arm length, however, is a valid predictor only for women, not for men. Figure 5 presents the regression lines from the same data: Tall women are discriminated against when the composite regression line is used.

It is time to level with you. The packing study is a laboratory project in which golf balls were fed by gravity into a tray 32 inches from the subject's seat. For most men, reaching the tray presented no problem; for most women it was quite a reach because we planned it that way. The study was repeated with the tray brought closer: 24 inches. Figure 6 shows that the situation was reversed; for men with long arms, the work space was too cramped for fast packing. Arm length became a valid predictor for men. For women, the "test" of arm length had no validity.

The project was intended mainly to point out two things. First, despite the adverse impact, a silly arbitrary predictor can be valid and its use probably legal. Second, if an employer in such a situation genuinely valued improved performance more than his managerial prerogative to test, he would develop alternate hypotheses and design the validity out of the job. The validity study would be used, not as a basis for selection, but as information about the criterion. If performance is dependent on arm length, then the tray can be set at an optimum distance for each person, male or female. And for the redesigned job, the "predictor" is no longer valid.

Validity and Values in Other Areas

It would be tempting to stop now if I were not afraid that people would misunderstand. People might think I have been talking about employee selection; I have *not* been. I have been talking about the problem of assessing validity (*a*) in measuring things we value and (*b*) in the hypotheses we pose for explaining them. Consider some rather different measurement areas.

ORGANIZED CLIMATE

First, consider that construct, or family of constructs, known as organizational climate. It is generally hypothesized by students of the subject that changes in the climate of an organization will produce changes in such value-laden variables as organizational stability, productivity, satisfaction, and the like. The testing of these hypotheses requires valid measurement on both sides, but let us skip nimbly past the tiresome criterion problem and focus only on the measurement of the climate. In fact, let us narrow the problem a bit more and specify that we shall discuss measurement of perceived organizational climate.

As some of you know, I have argued that perceived organizational climate only pretends to be a new construct when it is really only the rediscovery of the job satisfaction wheel (Guion, 1973). According to Schneider (1973), that is not necessarily so, even if generally true at present. According to his analysis, the perception of organizational

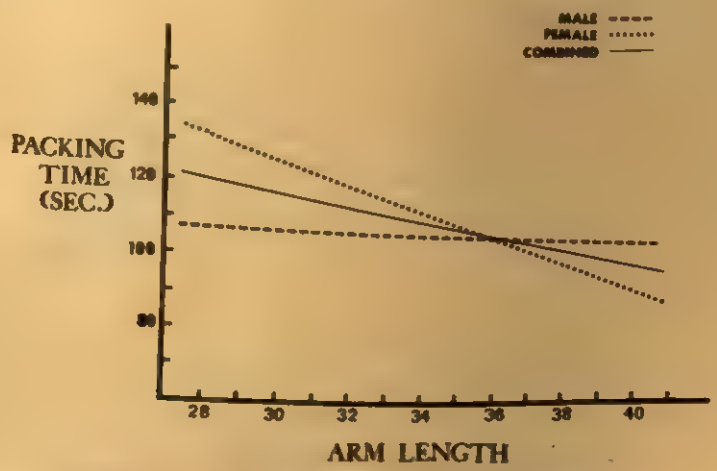

Figure 5. Regression lines in original condition.

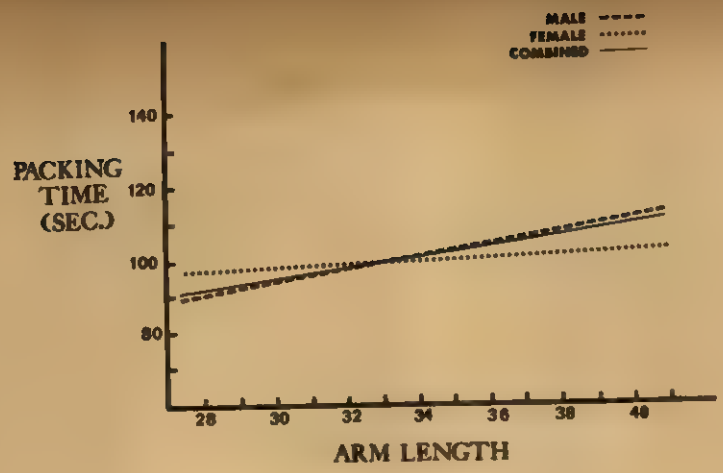

Figure 6. Regression lines in redesigned task.

climate is the result of concept formation; it is cognitive in nature. In contrast, the perception of job satisfaction is affective—an interpretation of the perceived environment in terms of the individual's own values, needs, or desires. Job satisfaction is a highly personal, subjective construct; different people react to the same organizational stimuli with different kinds and degrees of affect. Organizational climate, however, should be consistently perceived by different people; it is more objective. Climate is an organizational characteristic; satisfaction is an individual characteristic.

Suppose we were to take Schneider's distinctions as a starting point and develop a descriptive measure (or set of measures) of organizational climate. In the light of the concepts of validity, how should we proceed?

First, we should define a *content* domain. Since we plan to differentiate two constructs, content domains should be defined for each. The content of organizational climate must be defined as possible objective, cognitive descriptions of aspects of the work environment, such as what leaders do. The corresponding content domain of job satisfaction must be defined differently as possible affective reactions to various leader behaviors. When the domains have been defined, elements of these domains can be selected for measuring each construct. If the sampling is done well, both instruments, one for measuring climate and the other for satisfaction, should be judged satisfactory in content validity. (Incidentally, although I am thinking largely of questionnaire construction, other types of measurement could be derived from the same beginnings. For example, interviewer or observer checklists could be prepared and evaluated in the same way.)

This content sampling does not, of course, say much about construct validity, but it is a fairly good start. The next step should be an item analysis, based in part on item variances. Items describing organizational characteristics should, according to Schneider's construct, have low variances; items describing individual attitudes should have high variances. Conventional analyses should also be done so that internally consistent subscales can be formed for both climate and satisfaction. The climate scales should yield scores for each organization; the satisfaction scales should yield scores for each person.

The product is an instrument for measuring organizational climate for which preliminary evidence of construct validity is accumulating from the manner of its construction and from its demonstrable independence from a valid measure of a competing construct, if that should be the case. Investigators would then develop a network of experimental and correlational studies, including criterion-related validation, to test hypotheses suggested by the theory of organizational climate. If all goes well, the results may both modify ideas concerning the nature of organizational climate and provide further evidence of construct validity. In the process, the validities of various hypotheses about the correlates or effects of climate variables would either be supported or refuted, and these results would carry much greater weight in theory and in practice than would results based on measures of less well established validity.

Let us move to a different measurement problem, one from applied experimental psychology. In 1954, Fitts sought to apply the concepts of information theory to psychomotor performance. A simple task was devised, as diagramed in Figure 7, where the subject was asked to tap alternately two target areas with a stylus. The width of each target area was always the same, identified as W. The amplitude of the required arm movement was identified as A, the distance between the midpoints of the two targets. An index of difficulty was defined by analogy to the information measure using the formula $ID = -\log_2(W/2A)$.

The question of construct validity should be asked about such a measure. Data were reported that support an interpretation of this as a valid measure of difficulty, but as far as I know, the validity of this as a measure of information, as the construct has been defined in information theory, has not been investigated.

Another effort to measure the information content of a job, and more particularly of worker capacity for processing information, is reported by Standing (1971, 1973). Working directly from *N*-gram analysis (Attneave, 1959), Standing developed a measure of the amount of information in the sequences of operations in an inspection job. No validity information was obtained, but he demonstrated an approach to measuring the information content of work. In the same study he developed a measure of cognitive complexity for individual inspectors, that is, a measure of individual differences in abilities to codify and process the information in the job. The results of his investigation give tentative support for a judgment of construct validity, but there is as yet no network of support.

Uncertainty, or information, as a component of a job or display, and the cognitive complexity of which individual workers or perceivers are capable, are important constructs to people in human engineering, consumer research, job or system design, task simplification, job enlargement, or job enrichment. The motivational hypotheses derivable from information theory seem to have unusual potential for industrial and organizational psychologists and for the states, processes, and outcomes they value highly. Unless we have valid measures of these constructs, however, the best we can offer is idle speculation.

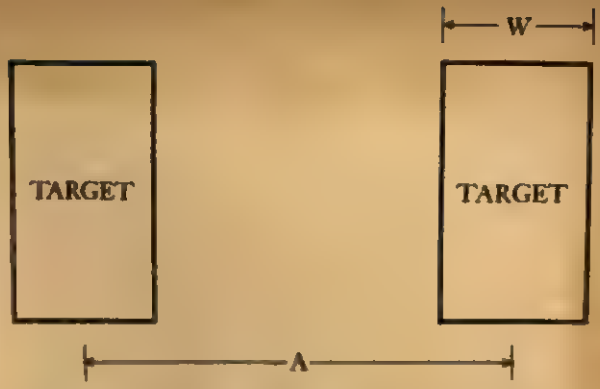

Figure 7. Schematic diagram of the task used by Fitts (1954).

OTHER MEASUREMENT PROBLEMS

Let me mention without comment some other constructs of interest to us for which our measures are often valid more by proclamation than by investigation: cognitive strategies, competence, creativity, effort, equity, expectancy, growth, leadership, long-term memory, obsolescence, organizational stability, productivity, utility, valence, work values.

Conclusion

I do not mean to imply that the whole of psychology or even the whole of industrial and organizational psychology must work with hypothetical constructs and develop evidence of validity for all measures, experimental conditions, or interventions. Quite the contrary. Just as I believe that an employment requirement can be job related without being also described as valid, I believe the science and practice of psychology can be advanced in some situations by behavioral manipulations, environmental changes, or social interventions for which the concepts of the validities of measurements are as irrelevant as they are for measures of arm length. But I also claim that nearly every branch of psychology entertains problems involving the creation of scientific or technological constructs, that measures are and must be derived for such constructs, and that the ultimate worth of the enterprise does in fact depend on the validity of inferences drawn from such measurement.

It is rather peculiar that it is fashionable from time to time to disparage psychological testing, yet many critics of testing often condone or commend work done with measures never subjected to such rigorous scrutiny as is required in the development

or use of a published test. They draw lovely pictures or diagrams and call them models, fill the models with terms they cannot operationalize, and give never a thought to the validities of the operational definitions they *do* use. Much of our field is long on numbers but short on measurement.

It is time for industrial and organizational psychology to rediscover its heritage. The field was built on excellence in measurement. Now, many of us are not particularly excellent. Two important new enterprises—new windows, if you like—are being opened by our brethren in educational psychology. One of these is the content-referenced measurement of mastery, greatly important in personnel applications. The other is the measurement of change or growth in the evaluation of the effectiveness of programs. Few things are more important than program evaluation to those who institute formal programs or speak of their "interventions." If one is going to intervene, his obligation is to find out whether that intervention has helped or hindered the organizational enterprise. We must not only again give measurement its respected place within our specialty, but we must demand that the standards of effective measurement also be respected. We must stop the sophomoric "naming fallacies" that hide the rediscovery of old wheels behind the rhetoric of new labels. In industrial-organizational psychology and in psychology generally, we must identify more clearly than ever before the dependent variables and outcomes we value fundamentally.

We must often open new windows and travel new highways to find these outcomes. But we must not fall victim to the notion that every change, every innovation, every bright idea moves us closer to cherished goals. Every new highway we travel deserves and needs to be explored with the same thoroughness a test developer is expected to use in exploring the validities of his test. The so-called "testing standards" are in fact standards of effective measurement for nearly all of the directions psychological research might take.

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Paradigm Regained?

Cognitive Behaviorism Restated

C. ALAN BONEAU *American Psychological Association* [1]

Strategies of dealing with theoretical problems as well as other games psychologists play change as abruptly and as completely as do their tonsorial styles and other displays of individual adornment or behavior. For example, one can only be impressed by the clusters of specialized research problems that arch gloriously through the higher intellectual atmosphere of scientific meetings and then like the Nehru shirt fade away to remain only in the memories of a few.

Such game-playing behavior may have a fadlike quality to it. As in other facets of life where one stays *au courant* by learning the latest dances or dining at the newest restaurants, the psychologist discusses the latest passing theories and performs the newest form of experimental manipulation controlled by the most up-to-date electronic esoterica. In playing games, however, considerable technical skill may be acquired for future reference and application.

Paradigms, to use the handy concept of Kuhn (1970), modulate such transients on the historical time line. Paradigms are more or less encompassing. Those with less ambitious, more limited viewpoints seem currently to have a higher probability of survival for a reasonable period of time. Such a statement, itself, is a time-bound generalization, but most current perspectives deal with only a very limited range of psychological phenomena.

The 40-year period ending in the fifties, however, was an era of theories of epic proportions (or pretensions). This Wagnerian period of psychologizing produced such heroes of behaviorism as Pavlov, Thorndike, Watson, Tolman, Guthrie, Hull, and Skinner, all concerned with observable behavior as the basic data of their science. Ideations, thoughts, and mental or cognitive structures were the themes of another theoretical mode. The Gestaltists—Wertheimer, Köhler, and the rest—and Freud, like the behaviorists, were gifted heroes and their theorizing was monumental.

Well, as we all know, the age of the antihero is upon us. Even for those of us who would preserve just a smidgeon of that magical aura once a prime ingredient of the dignity of kings, the honesty of presidents, and the infallibility of popes, this is probably just as well in psychology. The conceptual foundations of many theoretical frameworks rested only on the shifting sands of time.

As a result of complex factors, the epic version of psychological theory is no longer the force in psychology it once was. Recent theorists have been content to deal with minitheories, particularly in view of potential devastation should they attempt to be too elaborate. Koch's (1954) treatment of Hull is a case in point.

One possible exception to the historical generalization expressed here is the form of behaviorism espoused by one of the few extant epic heroes, B. F. Skinner. Operant conditioning as a technical procedure and behavior modification as an applied technique have achieved unprecedented renown for psychology *qua* psychology. The Skinnerian viewpoint, however, is myopic about a good deal of the material others would like to consider to be psychology. While the textbooks written by eclectic

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[1] The author is Director of Programs and Planning of the American Psychological Association. The ideas were developed and the article was written over a period of a few years when it was possible to take annual leave from APA for a few weeks or a day or two here and there. Special thanks are due to the State of Maine for its rockbound, uncrowded coast; Ann Boneau for typing several generations of what turned out to be a family project and giving up otherwise good vacation time to boot; and James L. Cole, who was in on it when it all started but was prevented by other pressures from involvement in the development. The author is deeply indebted to the anonymous reviewers who provided several useful suggestions that have been incorporated into the article. The views expressed in the paper should not be construed as representing opinions or policies of the APA.

Requests for reprints should be sent to C. Alan Boneau, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

behaviorists and others often seem a hodgepodge of unrelated subject matter on which psychologists happen to be working, the Skinnerians achieve a conceptual purity (and strength) by ignoring selectively much of the subject matter of psychology. They aver implicitly if not explicitly that such subject matter is unimportant or perhaps not even psychology.

Skinner aside, there are advantages to major frameworks, the perspectives or paradigms that relate to one another the major pieces of a science. The interconnections among the pieces become visible; prerequisite skills to enhance understanding are made apparent; strategies for teaching the discipline as a whole are evolvable; the material becomes easier to learn when it ties together; the important is delineated from the trivial; the field becomes easier to understand and to use as a basis for advancement of knowledge. In retrospect, some obvious substantive advances may fall quickly and simply out of fresh paradigms, or complex formulations may become simplified.

The author feels that these constitute good and compelling reasons to attempt periodically to advance a formulation epic in comprehensiveness. Psychology as a science has grown enormously in technical sophistication. There exists a sizable amount of simple advance polished piece by piece by a number of highly skilled and creative individuals. What is needed is a schema to utilize this material as basic building blocks of a coherent structure.

A few years ago, the author (Boneau & Cole, 1967) presented a theory to account for the behavior of pigeons making a discrimination among various spectral wavelengths. That theory utilized statistical decision theory concepts and posited that the pigeon internalizes and uses information about the outside world in the process of discriminating. The information used by the pigeon in the original formulation was limited to hypothetical processes underlying only a limited range of behavior. Subsequently, the author has generalized the formulation to include a broader range of behaviors. This generalization is presented in this article.

Initially, this impulse toward conceptual expansion was fueled by the recognition that the language system of the formulation had certain advantages over traditional behavioristic language. While behavioristic approaches tend to imply that behavior is under external control, as expressed in the widespread use of the term *stimulus control,* the decision-theory/information-processing approach, on the other hand, seems to imply that behavior is determined primarily by events within the organism and permits behavior to be based on cognitive processes of various kinds. Thus, the information-processing approach allows for an approach to the intuitive conception of the layman of personal responsibility and involvement in production of his own behavior.

Further, the decision-theory/information-processing approach to traditional, behavioristically formulated problems provides the possibility of building a bridge between such areas as operant conditioning and information processing, both of which had been developing in experimental psychology relatively independently of each other. As clarity emerged in the evolution of the formulation, it became apparent that additional gains flowed from it. The formulation illuminates some psychological conceptual areas, provides the working psychologist with greater capability for analyzing basic processes, suggests directions for further research and application, and tends to tie together widely diverse areas of psychology.

First, the conceptual scheme based on information-processing notions will be described, including a discussion of the variables of which behavior is assumed to be a function and the way they combine to produce behavior. Then to illustrate the value of the approach, there will follow an explication of the way in which the formulation can be applied to several processes that are considered generally to be psychological in nature. As we proceed, we will indicate how the many areas of psychology can be related by virtue either of their contribution to the understanding of the workings of the information-processing system or by the light shed by the system on other areas. Finally, we will attempt to evaluate what the framework accomplishes for us.

The Theoretical Framework

The theory assumes that, through extended commerce with the environment, the individual internalizes information about the external world. We start with a discussion of the internalizing process, with its structure and how changes in the structure and its content occur. Following that development, the way in which this internalized information is combined with information about motivational states to determine behavior will be indicated.

The internalized information is assumed to be structured into an *internal model of the environment* (IME). The IME is unique to the individual and is based on the individual's history of interaction with the world and with biological potential in a very personal way. The IME depends on the external world, but it is not necessarily a veridical representation for a number of reasons that will be spelled out as we proceed.

The theory identifies three dimensions of the IME that correspond roughly to the external dimensions of stimulus, response, and reinforcement with which S–R formulations deal. In this account, the three dimensions will be called *situation*, *action*, and *outcome*, each of which may have objective external observable manifestations in addition to the internalized version. To distinguish between the external and internal versions of these events requires a special symbolism. In what follows, the external or veridical situations, actions, and outcomes will be designated by the capital letters *S*, *A*, and *O*, respectively. The internal correspondents will be designated by the small letters *s*, *a*, and *o*, respectively. When necessary, subscripts will be used to differentiate between different occurrences of events on any of the dimensions.[2]

Let us consider the situation dimension first. The processes by which external physical energy is transduced through the sensory receptors of an organism and subsequently segregated and organized is the subject matter of important chapters in psychology which are relied on here. For example, all the rich panoply of visual stimulation are available as potential information in this process. Faces, symbols, colors, places, things discriminably different from other similar things are the basic situational events that become processed into the internal model of the environment. As general psychology has demonstrated, the individual has the capability to perform the appropriate perceptual activities to accomplish these remarkable differentiations and constructions.

There are a large number of physical dimensions along which external events can differ, of course, many of which have corresponding internal variation. The way in which organisms can come to respond differentially to small changes in external stimulus dimensions is important for the understanding of the total process. For our purposes here it is sufficient to note that any discriminably distinctive aspect of the total array of potentially internalizable stimulus dimensions or combinations of dimensions can form cues that are the elements of our information-processing system. The fact that individuals can learn to differentiate between similar situations is basic to the theory.

To be sure, we are finessing the epistemological problem of distinguishing external from internal reality by taking the position of a naive realist. Things, we assume, are "out there," and, further, they are what they seem to be at least most of the time (even though we know them only phenomenologically or inferentially).

A second dimension of the IME consists of actions. The way in which individual muscle twitches, nerve fiber firings, and gross muscle movements are coordinated into organized sequences is again the content of other chapters of psychology. In this account, actions do something. The somethings can consist of looking at, of walking, of performing specific kinds of movements, and the like. It is taken for granted that responses are coordinated into integrated greater wholes of movements that may with practice become skilled movements. All such integrations of behavior whether skilled or unskilled are elements in the action dimension. The fact that movements become more precise and skilled with practice has significant consequences for understanding their role in the overall behavior of the organism. This will be discussed later.

The internal action dimension is conceived of as an internalized list of potential actions that can be brought into play by the individual. For organisms such as man that have a capability for extensive manipulation of symbols and images, elements on the internal action dimension need not correspond to actions that have been performed by the individual. Rather, for registration on the action dimension, it may be sufficient to observe the actions of others or even to conceptualize new possibilities of actions that have been performed by no one. We assume in dealing with lower organisms, however, that such ability is limited and that the in-

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[2] The *SAO* terminology was utilized earlier by Irwin (1971) in a different formulation. The reader is referred to his book for discussions of several conceptual issues that arise in "cognitive" theories in general and are very germane to the present one. What is meant by a cognitive theory in the sense intended here seems implied by the definition of "cognition" proposd by Neisser (1967): "The term 'cognition' refers to all the processes by which the sensory input is transformed, reduced, elaborated, stored, recovered, and used [p. 4]."

ternal action dimension consists generally of internalized representations of externally observable actions that have been previously performed by the organism, or have been programmed into the behavioral repertoire as innate or instinctual givens.

The third aspect of the IME consists of outcomes. Outcomes are thought of as sensory inputs and thus essentially the same dimension as situation. It is convenient, however, to view outcomes as changes in situation correlated with or contingent on actions, although lack of change in situation following an action is no less an outcome. In the real world, outcomes are such things as the food pellets that follow a bar press by a rat. To the rat, the outcome is information about the occurrence of the pellet—that a change in situation has occurred with all of the associated sounds, sights, smells, and tastes that constitute the events of the internalized outcome dimension. As with other situational events, we assume a capability of the organism or individual to isolate, identify, and otherwise perceive changes in situation as outcomes and as correlated or linked contingently (whether real or imagined) to actions or other situational events. An important class of outcome inputs are those that arise from internal stimulation, for example, visceral reactions.

The basis then of the formulation is that the individual internalizes, that is, extracts information about the outside world and about his relationship to that world and constructs an internal analogue of critical portions of that information. This information can be about external situations in which the organism has found itself, actions that the organism has performed, and outcomes correlated with actions or situations. What the organism learns and what is internalized is information about when, where, and how outcomes are available. For outcomes that the organism has experienced, the internal model of the environment contains information about the actions that produce these outcomes and under what conditions. But the above statement about the information is not sufficient. It does not yet have built into it information about the uncertainty that outcomes will follow actions. Most such relationships in nature certainly are not invariable. We now examine this complication in more detail.

Let us choose probability language to denote the uncertainty. A complete description of the external world (heaven forbid!) would consist of the complete set of probabilities that all possible outcomes will occur under all possible combinations of situation and action, namely, the set of all conditional probabilities P_{ijk}, where $P_{ijk} = P(O_i|A_jS_k)$, that describe the probability that outcome O_i will occur given that action A_j has taken place in situation S_k. Stated so baldly, a complete description is a technical and theoretical impossibility.

For analysis, it is useful to restrict the *SAO* space to a limited number of possible combinations, a procedure adopted by the science of psychology to focus on the relatively pure case. Outcomes can be arbitrarily limited in equipment such as the Skinner box to, say, a food pellet sliding down a chute to the hungry animal (O_1) or no food pellet (O_2). Effective (or countable) actions can be limited to a deflection of a right-hand bar (A_1) or a left-hand bar (A_2). Situations can be restricted to the presence (S_1) or absence (S_2) of a light in the side of the box.

Also defining the "external world" constructed by an experimenter are the conditions under which outcomes are related to action and situation. An example might be in the form of a statement that the animal will receive a reward on a random 50% of bar presses on the left-hand lever only when the light is on; otherwise no reward is forthcoming.

Under this limited set of conditions, the "veridical world" of the experiment is defined by the probabilities P_{ijk} as follows:

$$P_{111}\,(O_1|A_1S_1) = .00 \qquad P_{211}\,(O_2|A_1S_1) = 1.00$$
$$P_{121}\,(O_1|A_2S_1) = .50 \qquad P_{221}\,(O_2|A_2S_1) = .50$$
$$P_{112}\,(O_1|A_1S_2) = .00 \qquad P_{212}\,(O_2|A_1S_2) = 1.00$$
$$P_{122}\,(O_1|A_2S_2) = .00 \qquad P_{222}\,(O_2|A_2S_2) = 1.00.$$

A careful study of the set of probabilities shows that the only time the food pellet (O_1) occurs is in conjunction with left lever press (A_2) when the light is on (S_1), and the probability of that occurrence is .50. Of course, with a probability of .50, O_2 occurs with A_2 and S_1, or if you prefer, O_1 does *not* occur.

The above provides a succinct description of the salient features of the *external*, "real" world as they apply to our formulation. What about the *internal* version, the IME? According to the formulation, internal representations of the P_{ijk} are built up over time as the animal "learns" the task: the situations, actions, and outcomes. This requires among other processes mentioned earlier a probability-estimating mechanism to permit the development of internal states that we will call internalized or subjective probabilities. These shall

be designated p_{ijk} to distinguish them from the external, experimenter-determined values, the P_{ijk}.

As a result of these considerations we can now describe the IME to be composed of subjective conditional probability elements and define them as $p_{ijk} = p(o_i|a_j s_k)$. One form of learning consists of modifications in the p_{ijk} with increased experience so that they may come to approach the P_{ijk} in value. A well-trained animal, for example, with long, appropriate experience, may behave as if the P_{ijk} and the p_{ijk} have identical values. That animals can do this kind of learning is well documented. See, for example, Estes (1957). Human individuals are no less capable.

There are a number of obvious distinctions between the IME and the external world itself. First and foremost, they are different versions of the same thing and in fact may not even be similar. Consider the fact that the experimenter may suddenly change the required response to be a press on the right-hand lever instead of the left. At that point the characteristics of the "real" world are rather drastically changed, but it may take considerable experience by the rat in the situation to have the new state of the real world reflected in the IME and in behavior. The IME may, in fact, never recover from such an experimental manipulation and continue to have a residual positive probability value attached to left lever presses.

In another way the IME may not accurately reflect the real world. The IME, as conceptualized here, is based entirely on the outcome history of the individual. If for some reason the individual has not performed a response necessary to achieve a potential outcome, the IME will be less articulated than the real world. Potential actions may have no internal counterpart. If the animal when placed in the box never presses the lever or does not press it hard enough, there will be an incomplete internal representation of the potential for action in the situation.

The IME also may not be a useful representation of the external world if the situation requires fine discriminations among situations as a basis for outcome differentiation or requires skilled performance of certain actions. Ignoring the discrimination or performing the action clumsily may prevent the occurrence of desirable outcomes. As a result the individual's IME is less developed than it could be or may have zeroes when the actual probability for a desired outcome is greater than zero.

In our numerical example, the stimulus light may in fact have two physically similar intensities, one of which (S_{1_a}) is always reinforced, the other (S_{1_b}) never; but they occur equally often. Unless the rat has had a history of experiences in which subtle differences were important, the discrimination may not be made and the IME will not reflect the realities of the situation.

In a very brief way, the above has been an attempt to describe the basic elements on which behavior is based according to the formulation unfolding here. There is a strong flavor of the purposive behavioral approach of Tolman (1932), and the IME of this formulation must bear a familial resemblance to Tolman's "cognitive map" (Tolman, 1948).[5] Likewise the p_{ijk}'s constitute a more precise way of conceptualizing "sign-significate relationships" or "expectancies." There is more, however, to the present formulation than "mod" clothing. The conditional probabilities allow the use of further machinery to push past conceptual shortcomings of the Tolman approach. Tolman's theorizing, for example, provided no easy way to prod the individual into action, and, according to Guthrie (1952), left the animal "buried in thought." The present formulation provides an axiomatic solution to this problem; the details are discussed next.

Outcomes often have connected with them another extremely important aspect that is essentially motivational in nature. That aspect is characterized by sensations of pleasure or pain and has been variously called *utility, value,* and a host of other names by various writers. We will call it *hedonic value* (or sometimes, for short, simply *value*). We note in passing that an outcome and the hedonic value associated with that outcome are considered to be independent, separate aspects processed as distinct items of information. The need for this distinction is that hedonic values of outcomes may change for a variety of reasons important for the theory but occasionally psychologically trivial, for example, the diminution of the value of food because of eating.

We assume that hedonic value can be measured on a unidimensional scale. For our purposes, we further assume that all outcomes can be measured against the hedonic value scale, that combinations

[5] Tolman's theorizing and extensions from it that have similarities to the present formulation are discussed in Hilgard and Bower (1966). Others seem to be closing in fast. See, for example, Mischel (1973).

of value occurring at the same time are combinable on this scale, and that the individual *can* make absolute value judgments. The scale we assume, for convenience, to range in both positive and negative directions with a zero point. The requirement of a zero seems not necessary, and the scale can be treated as dealing with relatively ordered preferences. (For example, see Premack, 1971.) For expository purposes, however, it seems useful to consider positive values as associated with attractive outcomes; negative, with repellent outcomes; and zero values, with neutral outcomes. (See also Irwin, 1971.)

The hedonic values of outcomes as we have noted are not necessarily fixed. The value of food-related outcomes, for example, increases with hunger. Also, as with olives and chamber music, one's values associated with outcomes can change through a learning process presumably involving learned emotional responses. How values have the value they have and how they change is another whole chapter of psychology. The value-outcome structure of different individuals seems to be primarily based on their classical autonomic conditioning history and is a significant factor contributing to individual differences in behavior. Hedonic value, thus, is dependent on time and should have a time (or trial) subscript. Usually for convenience, we will drop the time subscript. Hedonic value, of course, can also be highly determined by unlearned factors; for example, the value of a sweet taste is apparently a built-in hedonic operator for many species.

Hedonic value determines behavior in a straightforward way. Let us assume as an axiom that the individual will behave in order to maximize the potential hedonic value available in a situation. This is fundamentally a statement that defines a characteristic of human (and animal) nature. It is by no means new. Historically, versions of utilitarianism have been outcroppings of a pervasive stratum of philosophical thought.

It should be noted that the axiomatic nature of the assertion makes it a tautology. If behavior appears not to maximize utility, that, by definition, is a fault of the observation. Because the behavior is maximizing something by definition, it behooves the observer and/or analyzer of behavior to identify precisely what is being maximized. This feature of the theoretical statement is the basis for a conclusion that the statement is not capable of being disproved. Whether the statement is useful is the topic of this article.

How may we stipulate a procedure by which the individual determines which action to perform in order to maximize the potential hedonic value? Outcomes are not all equally probable, so we may use the conditional probabilities in the IME as weights to produce *expected* hedonic values (in the precise probability sense of that term) by multiplying the p_{ijk}'s by the values. Since outcomes are contingent on actions, we associate hedonic values with the actions associated with them. The expected hedonic value h_j associated with action a_j, for example, will be the hedonic value of outcome o_i multiplied by the probability that outcome o_i will occur if action a_j is performed in situation s_k, or

$$E[h(a_j)] = h(o_i)p(o_i|a_js_k).$$

Actually, in a situation any action may have several outcomes with different probabilities and varying hedonic values associated with them. For example, a bar press may sometimes result in a large amount of food with a high momentary hedonic value to a hungry animal. A smaller amount of food may occur with a different probability, and no food, on its own probability schedule. All bar presses require a certain amount of exertion which will have a relatively negative hedonic value. The expected hedonic value of the bar press (action a_j) for a given situation then will be the sum of all expected hedonic values of the outcomes associated with the action, assuming them to be additive, or

$$E[h(a_j)] = \sum_i h(o_i)p(o_i|a_js_k).$$

The maximization axiom requires that the expected hedonic value of actions available be evaluated and the action be performed that provides the maximum, that is, the action a_j corresponding to

$$\max [h = f(a)] = \max E[h(a_j)] = \max \sum_i h(o_i|a_js_k)$$

for all a_j and fixed s_k.

To recapitulate, potential payoff in a situation can be represented as the sum of the expected values of all outcomes associated with each available action represented in the IME. The behavioral axiom, at that point, requires that the action be performed for which the summed expected value is greatest. In a more precise way, we may state

the maximization axiom as follows: *In any situation the individual will maximize the expected hedonic value by performing that action which produces the set of outcomes for which the summed momentary subjective expected value is the highest.* It is momentary expected value because values may change over time, and it is subjective because it is based on the idiosyncratic, subjective, internalized conditional probabilities of the IME. "Highest" in theoretical terms means highest in numerical value or its analogue (but behaviorally and tautologically it means "most preferred").

Summarizing the whole theoretical machinery, it is supposed that the individual through commerce with its environment can create for itself a model of that environment containing information about known situations, about the set of known outcomes available, and about the likelihood that those outcomes occur when known actions are performed. The theory then postulates that the individual in any situation evaluates the set of possible outcomes with their various hedonic effects at that time, determines the expected hedonic value of the outcomes, and then performs the action necessary to produce the highest expected hedonic values.

This general formulation of behavior, as we have noted, was developed for a particular experimental situation by Boneau and Cole (1967) to apply to the behavior of pigeons in a color discrimination task in a Skinner box. In that application, the critical elements of the IME consisted of situations defined as points on a hypothetical dimension of hue varying according to a Thurstonian dispersion process; key pecking and no key pecking were actions; a known amount of grain and a fixed effort for key pecking were outcomes; and the internalized probabilities of reward, given each situation, were presumed to approximate veridical values for the pigeon after several thousand trials. The proportions of pecks to variously closely spaced monochromatic spectral stimuli were used as estimates of the proportions of times that the expected hedonic value for pecking was greater than the value for not picking. By inference these estimates could be used to determine parameters of the hypothetical Thurstone discriminal processes. The reader is referred to Boneau and Cole (1967) for details.

Applications

The remainder of this article has a number of goals related to an exploration of the utility of the global, epic approach, and particularly to the formulation presented here, in thinking about psychology. First, the structuring of the field of psychology from the point of view of the formulation will be considered briefly. The formulation is seen as a device that allows for the viewing of psychology as a coherent, organized panorama rather than as a collection of disparate intellectual and scientific doodles valued as a whole only by those either with sufficient faith or with a highly developed appreciation of abstract design to support a belief that it somehow all makes sense. To emphasize the scope of the approach, there will be a discussion of phenomena in behavior modification at some length and a very brief discussion of thought processes.

Let us try our hand at the big picture and how pieces of psychology relate to the system and each other. One emphasis of the IME is its focus on input and output relationships between the organism and the environment. We have seen earlier how subareas of sensation and perception and further refinements in those fields are essential to an understanding of inputs to the system and to give substance to the situation and outcome dimensions.

Likewise, the dimension of action is one that has been intensively studied, particularly in applications where the problem is one of skill learning. The whole subfield of action is broader, involving the many ways that the potential for action is internalized in the IME. For example, trial-and-error learning, observing the behavior of others, being instructed, maturation, instinct, and motor coordination differences all play a role in the understanding of actions as they relate to the IME.

The mechanisms by which contingent probabilities are acquired have been a major concern of mathematical learning theory and Bayesian approaches, among others. Including these areas as additional contributing factors to the understanding of behavior fits a few more pieces into the emerging picture. Adding the subfields concerned with motivation, with the processes underlying innate and acquired values, fills out the basics of the theoretical approach. Physiological processes related to depression or anxiety seem important here. In fact, selected portions of the biological substrate are essential parts of the curriculum and can be introduced as appropriate.

A curriculum designer might construct a cycle through the general concept of the IME and maximization notions and then iterate back through the

more specialized areas to provide depth to the conceptualization. It would be appropriate to include neurophysiological formulations insofar as they add to the understanding of the basic processes. A curriculum structured in this fashion would have much the same kind of hierarchical structure as does physics, for example, and prerequisites would indeed be in order to provide a more in-depth understanding of the five processes.

The areas of psychology dealt with above certainly do not exhaust the intellectual domain of psychology. As we have presented these areas, however, they have been propaedeutic to the understanding of the workings of the basic system. Other areas may be seen as derivative processes emerging from the functioning of the system.

Consider, for example, the pattern of persistency of life-style and behavior that psychologists call "personality." The explanation offered by the present formulation goes as follows: As the individual develops, some actions are found to maximize hedonic value because of individual aptitudes or personal characteristics, or because the environmental context, including societal and personal relationships, provides outcome supports for some actions and not for others. These selected actions become more successful over time in increasing payoffs as skill develops and as subtleties of the situation that signal variations in outcome are discriminated. The individual becomes relatively masterful in negotiating the range of situations and actions accessible in a particular life-style and thereby relatively less masterful in dealing with the congeries of situations and actions that characterize other life-styles. It becomes maximizing then to limit oneself to the range of situations and actions with which one is familiar and has mastered or made the most of. Life is thus a climb uphill to a relatively maximizing style. Likewise, one is encouraged to make use of previously mastered actions as a means of coping when thrown into new situations, or to look for instances in which well-developed actions may pay off.

It should be recognized that there exist a large number of alternative locally maximizing conditions at least potentially available to each individual. Accidents of situation, parental and other societal controls, abilities, aptitudes, and special supporting or undercutting outcome circumstances including traumatic experiences provide texture to an IME that may lead to commitment to a life-style that has the property of being self-sustaining if it provides locally maximizing opportunities.

Since maximization of hedonic value implies that the individual will perform actions that have a high probability of producing a valued outcome, the individual may more and more frequently tend to seek situations having high outcome or value potential because action itself may involve mobility toward situations conducive to reward. At the same time, the individual may become more capable of differentiating critical elements of the situation and of performing an action whenever skilled or selective action itself increases the payoff. Practice, of course, makes perfect. Moreover, outcomes themselves may through classical-conditioning mechanisms keyed to fundamental drive states achieve a changed associated hedonic value either positive or negative. As a consequence, the individual tends to lock onto particular well-known situations and highly skilled actions that have been determined by experience to maximize known available hedonic value which itself is changed by the complex of circumstances. Any change away from a known source of reward or a new variation in a highly skilled action, almost by definition, is likely to result in a step away from a locally maximizing condition, and thus downhill with respect to expected hedonic value. The maximization axiom does not tend to permit deviation from well-known maximizing actions or situations.

The IME is the individual's own unique view of the world and determines his role in it. Childhood is spent in learning what the world has to offer and what can be done about getting what is available and under what conditions. By the time the individual approaches middle age, under most conditions in our society, the IME has stabilized as the individual becomes situated in a niche that makes use of whatever skills have been developed. The individual becomes hedged in with constraints based on experience about the availabilities of outcomes and the successes of actions. The IME thus provides the consistency link required for a theory of personality and, from the viewpoint of this theory, defines, along with the value system, the personality of the individual.

The temptation, or perhaps the practical necessity, of making first statements overly simplistic must be acknowledged. As Mischel (1973) pointed out, however, there is a considerable amount of lability to be accounted for in addition to consistency for those in the business of personality

theorizing. There is no intention here of insisting on the immutability of the IME, and the system presented here seems to allow room for personality restructurings in addition to moment-to-moment fluctuations. Changes in the external environment, trauma, and the like will undoubtedly produce changes in the IME, and such changes should be included as phenomena in learning theoretical approaches. Under generally *stable* conditions, however, the notions of IME tied in with skill-learning concepts will tend to produce an individual with stable behavior-correcting properties. We also pointed out that *successful* behavior under generally stable conditions can tend to create the conditions under which such behavior is possible.

It is highly likely also that behavior will be subject to ongoing fluctuations in the value of outcomes. Hunger certainly is one example, but other conditions such as mood swings, general anxiety, and the like could create, or are symptoms of, value changes. An understanding of the nature of such states and their causes seems essential to our understanding of personality.

Although the author has not pushed the development, other aspects of psychology also seem derivatives of the basic processes of the hedonistic system. Psychopathology would seem to deal with anomalies of the IME or the utility structure and their interactions. Social psychology properly should in part be concerned with the mutual interrelationships of individual IMEs and other determiners of behavior in a group setting; sociology and organizational psychology, with the interactions between individual IMEs, on the one hand, and institutional constraints and supports, on the other.

In total then, the cognitive behavioral/hedonistic approach seems to provide a home for many of the subspecialty areas of psychology. What else can it do? Consider behavior modification from the point of view of the present formulation. The main contribution made from the translation from behavioristic to cognitive hedonistic language is that the possibility of thinking about alternative action in a systematic fashion is greatly increased. This is partly because there are more specific variables in the new view, and all of the variables are open to manipulation. Let us examine the modification of behavior in some detail.

Given a system that tends to seek self-consistency, how can modifications occur? The answer is that changes can occur in the IME or in the hedonic value structure of an individual and that these changes affect behavior. Any of the variables p, s, a, o, h may be subject to experimental manipulation. In our terms, the diagnostic technology of behavior therapy involves determining the details of the local maximizing strategies determined by the ecology and history of the individual; defining the available outcome, or behavior support and constraint, structure; assessing the strengths and weaknesses of the individual with respect to discriminative facility for dealing with situations; surveying the range of skills and aptitudes for action; and determining the associated hedonic values of the actions.

Assuming that the individual's current state of affairs is locally maximizing, the task of the behavior therapist is to prescribe a set of procedures that modify the IME so that a different portion of *sao* space becomes a new local maximum. The art of prescription involves choices among alternative local maxima and strategies to attain them. Such choices necessarily are based on ethical and moral criteria, a fact that provides a new set of complexities. Let us examine procedural details from the standpoint of the theory without forgetting this problem.

A change in the IME can arise from a variety of processes or circumstances. Do we know how to create change? In many cases we do. First, the internal situation space can expand through an increase in the number of s_k. This could come about because the physical environment becomes different for some reason, new situations (S_{k_i}) added. An old situation may become more sharply differentiated, for example, new aspects noticed or discriminated. One way of causing such a change is by telling the individual what to look for! After all the skilled poker player may have become expert at reading facial expressions as cues to the strength of the hands of his opponents by virtue of learning that such cues are important. Barring permanent amnesias or sensory losses, it is likely that the internal situation-space monotonically increases, except that limitations of processing capacity, for example, attention or memory, may require that portions of the internal situation-space be ignored at any one time. One does not always immediately remember all details of places that have not been seen recently.

Second, the potential for activity in a situation may increase. New things may be introduced into the physical space to be acted on. The individual through explicit or implicit trial and error, or by

observing others, or by being instructed may discover other possible actions in an otherwise unchanged situation. Existing actions may increase in skill and complexity, in vigor, strength, or coordination; or action may itself modify the physical situation, for example, by moving the individual to another location and thus changing the situation's potential for further activity. Athletes and musicians are examples of individuals who capitalize on highly skilled actions, although everyone develops skills of one kind or another.

Third, the outcome set may change. Old possible outcomes disappear and new ones appear. Changes may occur in outcome characteristics. Size, color, taste, quality, consistency—all may vary. The individual may learn to discriminate more clearly among sensory dimensions so that the IME may expand with no physical change. One example is the discriminatory skill developed by the wine taster.

Fourth, the probability of the outcome may vary. This can be due in part to changes in the physical situation: If rewards become scarce, the probability of the outcome diminishes. But probability may be related to the ability of the individual to deal effectively with the situation—to make appropriate discriminations, to perform skilled actions when necessary, and thus to make the most of a situation. The star salesman, for example, maximizes by increasing the probability of a sale through sales tactics directed by an analysis of the customer, all learned by experience. Thus, the individual's own outcome history may depend in a rather direct way on the complexity of his IME.

Introducing new contingency relationships between actions and outcomes is the basis for "token economies." Highly valued outcomes can be made not to occur unless specified actions take place, a state of affairs that is possible in institutional settings containing powerful controlling figures to define contingencies. In the case of the ubiquitous Skinner box, an institution of another sort, there is an all powerful experimenter aided and abetted by his relay rack.

Lastly, the hedonic value associated with outcomes may change. Changes in the physical outcome itself may eventuate in a less desirable reward, for example, if food becomes spoiled. More subtle is the process by which shifts in the acceptability, the preference, the liking for a person, thing, place occur. We are all aware of the potency of the almost unexplainable, occasionally irrational, often overwhelming determiners of behavior, the emotional attachments, pushes and pulls, that at their extremes create overriding hedonic value influences on the behavior of the lover, the patriot, the saint, the martyr, the escaping prisoner, etc. In more moderate doses these factors contribute to the human quality of human beings, gently but firmly contributing to behavior. Much of the appropriate explanatory chapter of psychology dealing with such motivational matters has yet to be written. It would seem, however, that classically conditioned autonomic responses are implicated at least in part. Behavior therapy notably focuses on such value changes, for example, in desensitization by reducing situationally produced anxiety to specific stimuli. The extent to which positive values become associated with pleasant or nurturing situations is also well known but perhaps too little understood.

The problem of maintaining performance, say, as modified by a token economy procedure, under new circumstances is one that has been noted in the behavior modification literature (e.g., see Davison, 1969). In the present analysis, performance maintenance requires that the modified behavior be maximizing under the new circumstances. Without dwelling on the problem in any detail, let us consider a few of the possibilities. If the individual is changing from an institutional setting to a natural environment (or as is often seemingly the case in modern society, from one institutional setting to another), behavior maintenance will seem to require that the IME and value structure are compatible with the new environment; that is, actions predicated on the IME and value produce a satisfactory level of favorable outcomes in the new set of situations. If the purpose of an institution is to modify behavior from what existed previously, the requirements are a modified IME, a changed value structure, or a change in the natural environment, or combinations of the three such that the specific, focused-on behaviors are maximizing. A separate possibility is a retreat from one natural environment to another that is more supportive of the existing skills and value structure of the individual. We have already considered options open to the manager of change and have intimated that value judgments involving the future of an individual are distressing complications. Perhaps the best solution is to involve the individual in the selection process!

The foregoing analysis provides a large number of factors that provide strategies for analysis and

for the design of action programs in behavior shaping. The possibilities, particularly in combination, are numerous. To change behavior through modifying the IME, one can physically change the situation, or help individuals make more subtle discriminations about the situation and its cue aspects in order to educate the individual about distinctions within and among situations, distinctions that permit a greater understanding that different cues signal different situations. One can provide the individual with other possibilities to action or to improve on those actions he already performs, that is, to increase effectiveness. It should again be pointed out that the IME is in fact a personal construct inseparable from the individual's own situation–action–outcome history. The individual may know that valued outcomes are available for those with appropriate skills, yet realize that without those skills his own personal IME does not include the possibility of the necessary action.

Observing others perform an action of which he is capable may, however, expand the IME. If that action does not produce the desired outcome, then a problem-solving effort may be required to determine what conditions are necessary. *Modeling*, as the process is called, has been shown to be an effective means of transmitting information about possible actions and contingent outcomes to observers. The assumption here that humans can observe the behavior of others and visualize a potential action of their own, or at least incorporate the possible action into their own IME, allows us to bridge what otherwise might be theoretical limitation. While the mechanisms are not yet well known, the phenomenon is well documented. Our analysis implies that in general modeling will work as an "action" for those who have found that modeling the behavior of others has been successful in the past, that is, for those for whom it has had a relatively high probability of producing an outcome with high hedonic value. Individuals with a history of failure at performing observed activities, in particular, would not necessarily follow modeling as a strategy. One has to know appropriate skills to be able to model successfully. Further, the analysis allows us to state that modeling might be adopted in cases in which the model appears "similar" in capabilities to the observer, thus implying a similarity in the subjective probability of an emulated successful action. Models who appear too different from the observer may not contribute to an increased probability that an action may be relatively effective for the observer himself.

Implicit in the foregoing is the fact that learning cannot be viewed as a single factor from the point of view of this hedonic theory. Learning to differentiate among similar situations is vastly different from becoming adept at some athletic task, although both may depend on the other, and learning to like black coffee is different yet. Tolman (1949) described more than one kind of learning tied to the pieces of his system. The distinctions between kinds of learning in this theory, while similar to those described by Tolman, are based on which of the elements p, o, a, s, k are primarily affected by manipulations. The advantage of the theory formulated here is that it makes explicit the distinctions between several varieties of learning and implies that different procedures should be utilized for inducing changes in behavior, depending on the kind of learning involved.

Ordinary school learning, for example, according to this analysis can be of several kinds. First there are a set of skills or actions such as reading, writing, and arithmetic that make it possible for the individual to participate further in school and thus open up the possibility of participating in a world with an expanded *SAO* space. Second, there are a whole set of content-oriented subjects such as geography, history, language, and literature that are instrumental in gaining access to the *SAO* space that is reserved for educated individuals and for sharing in the reward structure associated with that particular *SAO* space. In part, content learning provides new options to the individual because it can describe and differentiate new situations, actions, and outcomes for the individual. To understand the process of learning for content requires a theory of instruction and rote learning that is in a separate chapter of psychology.

Another form of learning is associated with schools. From the perspective of the present theory, all sorts of actions related to performance in school such as paying attention to teachers, doing homework, concentrating on work at hand, and learning specific methodologies are actions that themselves may under appropriate conditions become maximizing. It is helpful to have parents who maintain a reward system that recognizes school performance and supports it appropriately.

Let us now jump to a consideration of pure cognitive functioning from the point of view of the formulation. We have already discussed the pos-

sibility that reorganizations of IME may occur as a result of a variety of processes in addition to the traditional behavioristic concentration on environmental factors. We have said, for example, that observation of the behaviors of others along with the outcome contingencies are allowable inputs to modify the individual's IME under some circumstances. Individuals indeed have such capabilities. We shall explore other possible capabilities.

Why not, for example, postulate as a form of action the process of implicit trial and error? Such a process could have the individual sort through memory for possible actions not utilized before in a particular situation, making an assessment of the probabilities of outcomes of various kinds. If such trial-and-error actions indeed produce desirable outcomes, the process (or action) of making such hypotheses would itself come to be a relatively maximizing approach to life and lead to an individual who thinks before leaping or performing any other form of action.

We can even imagine a process of internally generating new possibilities for action, of reorganizations of situations, of newly created outcome possibilities. If such a process is successful in maximizing outcomes for the individual, we can imagine an individual whose behavior may become to a degree unpredictable because it is based on an internal process geared to the task of generating novel action, of restructuring the internal environment. We can even imagine that such internal processes may themselves acquire positive value and thus become relatively autonomous. A humanistic version of the human individual emerges as a distinct possibility.

Such a brief characterization of thought processes serves only to whet the appetite. The author has not had sufficient time available to explore the ramifications of the approach in any detail at all, but is intrigued by the seeming fact that thought processes as subject matter and objective behavioral phenomena can be housed compatibly in the same framework. Such a conceptualization may be objectionable to the psychologist who bases his science only on objective forms of behavior. To the psychologist who was trying to include cognitive processes in his psychology, however, the formulation provides a bridge to other areas of psychology.

Conclusion

Despite some of the connotations of a cognitive theory, there is no need to deal with concepts such as consciousness in order to get one in operation. The cognitive processor need only be a mechanism that categorizes inputs, computes probabilities, assesses values, and makes decisions, a feat that can be accomplished in a simplistic way by computers. It is recognized that there can be "unconscious" and "conscious" perceptions and that both can be valid inputs into the system. The psychological and physiological properties and their relationships are interesting problems for psychology but not central to the theory. (For a further discussion of this point see Irwin, 1971.)

What has been attempted here is the explication of the basic outline of a theoretical account of behavior that views the human individual (as well as the subhuman organism) as a gatherer, processor, and user of information rather than as a simple reactor to external carrots, whips, and the stimuli associated with them. We have been rather explicit about the nature of the information processed and have hypothesized a formal system that utilizes the several items of information in the process of generating behavior.

This account seems not to be a "theory" in the sense that that term has been used by psychologists, but psychologists in general are not accustomed to axiomatic theories. One could not "disprove" the maximization axiom. Certainly, specific pieces can be tidied up if one wishes to make certain assumptions. For example, if one assumes a fairly direct relationship between probability of observed behavior and our variable p under certain conditions, then it is possible to agonize over the details of changes in p for various experimental manipulations. The fact that it might be important to study such changes is highlighted by the theory. Thus, even though the formulation lacks the feisty properties that challenge the research world to perform an *experimentum crucis*, the heuristic value of the theory as a pointer to truly important areas for research cannot be denied, that is, if one accepts such a theoretical framework.

In a sense, the axiomatic framework has the properties of such historical paradigms as the Ptolemaic and Copernican theories, or the phlogiston and chemical theories (read Conant, 1951, for a fun account of these issues), all of which in essence are not subject to proof or disproof but to pragmatic criteria. The Ptolemaic and phlogiston theories were discarded because they were cumbersome or required ad hoc Band-Aids, and there existed neater, more attractive alternatives, not

necessarily because they were false. They were perspectives for looking at the phenomena of their science that had outlived their usefulness. What seems required to test the usefulness of the approach espoused here is a sympathetic attempt by researchers and theoreticians to utilize the framework to bring order to their theoretical efforts, particularly as these efforts may be attempts to relate disparate portions of psychology.

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Attitudes Are Alive and Well and Gainfully Employed in the Sphere of Action

HERBERT C. KELMAN *Harvard University* [1]

Gordon Allport began his classic paper on attitudes in the 1935 *Handbook of Social Psychology* with this statement: "The concept of attitude is probably the most distinctive and indispensable concept in contemporary American social psychology." He described the term as

> elastic enough to apply either to the dispositions of single, isolated individuals or to broad patterns of culture. Psychologists and sociologists therefore find in it a meeting point for discussion and research. This useful, one might almost say peaceful, concept has been so widely adopted that it has virtually established itself as the keystone in the edifice of American social psychology. In fact several writers . . . *define* social psychology as the scientific study of attitudes [p. 798].

In the years since publication of Allport's paper, attitudes have, if anything, become even more central in social psychology, largely because they have come to serve as *the* dependent variable par excellence for the major categories of social-psychological research: the sample survey, the questionnaire study, and the laboratory experiment.

In recent years, attitude research has increasingly been subjected to rather far-reaching criticism, often linked with broader critiques of the standard methodologies of social-psychological research. Within sociology, the excessive reliance on survey data has come under increasing attack, most vehemently from the ethnomethodologists. Within experimental social psychology, there has been a strong movement toward field experimentation, partly motivated by the desire to replace assessment of attitudes in the laboratory with observations of behavior in natural settings.

In my own view of social behavior, attitude is a central concept, and I regard the study of attitudes as a legitimate pursuit in its own right and as a useful component of a multimethod research strategy. For example, studies of the structure and distribution of public attitudes on various social issues or of the determinants of attitude change can greatly contribute to our understanding of societal processes. In particular, attitude assessment combined with other research procedures—such as structural indexes in studies of organizational functioning or behavioral observations in studies of child-rearing practices—can add depth and perspective to the analysis of social phenomena. Too often, however, research has focused on attitudes, not so much because of a primary interest in attitudes per se or in their contribution to complex social processes, but because the assessment of attitudes was the easiest and most convenient way of obtaining data. It is obviously more convenient to distribute questionnaires in our classes, asking students how they would be likely to react in a variety of situations, than to observe their behavior in such situations, or to ask a sample of some population about the functioning of their organization, community, or society, than to make the detailed observations and develop the elaborate indexes needed to study a social system more directly. In many of our laboratory experiments too, after investing a great deal of energy and ingenuity in manipulating the independent variables, we have used attitudes as the cheapest and most painless dependent variable for assessing the effects of our manipulations.

Inappropriate and indiscriminate uses of attitude have raised serious questions about the validity and

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Requests for reprints should be sent to Herbert C. Kelman, Department of Psychology and Social Relations, Harvard University, Cambridge, Massachusetts 02138.

usefulness of the concept. Allport, foreseeing such a possible development in his 1935 paper, wrote:

> Whether the concept is being overworked to such an extent that it will be discarded along with the past shibboleths of social science remains to be seen. It seems more probable that the ever increasing number of critical and analytical studies will somehow succeed in refining and preserving it [p. 804].

Allport's vote of confidence has, in my view, proven justified. In the intervening years there has been a great deal of theoretical work, leading to considerable refinement and elaboration of the concept of attitude. One immediately thinks of the work of Katz, M. B. Smith, the Sherifs, Hovland, Doob, Peak, Campbell, Rokeach, Heider, Newcomb, Osgood and Tannenbaum, Triandis, Rosenberg, and Fishbein. These and other theorists have provided sophisticated analyses of the concept, have explored its complexities, and have pointed to the types of measurement procedures required to do justice to these complexities. The problem has been that these theoretical refinements have not always been reflected in studies utilizing attitudes. In many studies, attitudes have not only been largely irrelevant to the purposes of the research, but have also been assessed or interpreted in ways that ignore the traditional meaning of the concept and the elaborations it has undergone.

In short, social-psychological research has relied excessively on attitudes, to the neglect of other aspects of the social behavior of individuals and of the functioning of social systems. Attitudes have often been treated as the magical key to the study of social behavior and system functioning. As a result, they have been burdened with descriptive and predictive powers that they do not possess and have been used inappropriately and simplemindedly. There is clearly a need to broaden the conceptual and methodological repertoire of social-psychological research and to examine carefully the uses and misuses of the attitude concept. I do not feel, however, that these misuses discredit the concept of attitude as such. Attitude remains a viable and potentially powerful concept, as long as we place it firmly in a context of action.

I begin in this article by examining two lines of empirical research that have provided bases for questioning the validity and usefulness of the attitude concept and point out why I do not consider them decisive: studies of the consistency between attitudes and actions, and studies of the effects of counterattitudinal or attitude-discrepant action. I then proceed to present some notes toward a conception of attitudes as imbedded in an action context.

Studies of Attitude–Action Consistency

A series of studies have been designed to explore the relationship between attitudes and overt behavior. Two measures are obtained from each subject: an attitude measure, calling for some kind of verbal response vis-à-vis a particular object; and a behavioral measure, based on observation of the subject's overt response to the object. The prototype of this line of research is LaPiere's (1934) famous study, which found a lack of consistency between the negative responses of a number of hotels and restaurants to a mailed questionnaire, asking them whether they would serve Chinese guests, and the behavior of their personnel toward a Chinese couple that had actually been served at these same establishments a few months earlier. There are cogent reasons for questioning the relevance of the LaPiere study to the issue of attitude–action consistency (see Dillehay, 1973), including the fact that his verbal measure elicited policy statements rather than attitudes in the usual sense of the term. Most of the recent investigators, however, have used more standard procedures for assessing attitudes and have devised specially structured choice situations to obtain an index of overt behavior. The assumption behind this line of research is that if an attitude does indeed represent a predisposition to act toward the attitude object in a particular way, then it ought to be possible to predict responses on the behavioral measure from responses to the attitude questionnaire. However, in his thorough review of studies of the attitude–action relationship, Wicker (1969) concluded that "taken as a whole, these studies suggest that it is considerably more likely that attitudes will be unrelated or only slightly related to overt behaviors than that attitudes will be closely related to actions [p. 65]." He went on to say that his review "provides little evidence to support the postulated existence of stable, underlying attitudes within the individual which influence both his verbal expressions and his actions [p. 75]," although he did not reject the concept of attitude as such.

This series of studies have been very useful in pointing to the complexities of attitudes and in challenging the naive assumption that attitudes are direct indicators of overt responses and that assessment of attitudes can thus serve as a cheap substitute for more elaborate studies of social be-

havior. Of particular value are those studies that go beyond the question of whether attitudes and actions are consistent, and explore some of the conditions that make for greater or lesser consistency. For several reasons, however, the low correlations between attitude and action found by many of the studies in this genre, though pointing to the limitations of the attitude concept, do not demonstrate its invalidity.

1. First, we must look at these studies in the perspective of other empirical information—based mostly on survey research—that does show systematic relationships between attitudes and actions. In the area of intergroup relations, for example, studies have shown attitudes toward particular racial, religious, or national groups to be systematically related to personal associations or to voting behavior. Studies of political action have often shown consistent differences in attitude between participants and nonparticipants in certain protest movements or political organizations. In many of these studies, it is impossible to establish the causal direction of the relationship. A notable exception is a recent study by Brannon et al. (1973). Attitudes toward open housing were elicited in the course of a larger survey and actions were assessed by way of a field experiment carried out three months later, in which the original survey respondents served as subjects. The study found substantial relationships between survey responses and willingness to sign and publicize petitions favoring or opposing open housing.

The finding of greater *consistency* between attitudes and actions in studies based on survey designs than in those reviewed by Wicker (1969), which mostly involve experimental designs, brings to mind Hovland's (1959) discussion of the greater *stability* of attitudes in the face of persuasive communications shown in survey as compared to experimental studies. The effect of attitudes on actions may be exaggerated in survey-type studies, which generally use correlational designs, because of the ambiguity of the causal relationships involved. On the other hand, survey studies typically focus on actions more or less freely chosen by the subject—how he votes, with whom he associates, what groups he joins—whereas experimental studies present the subject with a structured situation in which he has to take some kind of action. It may be that in such a structured context situational constraints play a larger role and thus tend to attenuate the effect of attitudinal differences. Attitude–action consistency may be further attenuated in some experimental studies by the use of attitudes that are less well formed and by their assessment in a classroom context (Brannon et al., 1973). In any event, the sometimes conflicting results of survey and experimental studies must be taken into account in any effort to determine the validity of the attitude concept.

2. Attitude theory does not propose that a person's underlying attitude to an object is the sole determinant of his behavior when interacting with that object. This is even more obvious when the attitude refers to a class of objects and the action refers to a specific member of that class. Major determinants of action are the social constraints under which the person operates, including immediate interpersonal demands, as well as normative expectations—conveyed by relevant reference groups—that govern behavior in the situation (Schofield, 1972). Thus, a person may well act in ways inconsistent with his attitude if consistency would require violating a generally accepted norm of interpersonal conduct (e.g., the rules of common courtesy) or deviating obviously and publicly from reference group expectations.

The assessment of attitudes also elicits behavior in a real situation, subject to various social constraints. Such constraints are obvious in LaPiere's (1934) original study, in which the measure of "attitude" was based on a public letter stating hotel or restaurant policy, but they also operate in more traditional attitude assessment situations. Even when responding anonymously, subjects are often inclined to express attitudes that conform to the views normatively prescribed within their milieus. Situational determinants are particularly likely to influence response to attitude measures in the area of race relations, where the attitude questionnaire tends to evoke the tolerance value inculcated by the society—"especially in a college population where verbal adherence to the value of tolerance is likely to be very high [Weitz, 1972, p. 15]." In the action context, on the other hand, the "situational threshold" (Campbell, 1963) for expressing tolerance may be higher. Thus, subjects conforming to the norm of tolerance on the questionnaire may deviate from their expressed attitudes when they find themselves in a concrete interaction situation. This line of analysis suggests that discrepancies between attitude and action are likely to arise in those domains in which people are subject to cross-pressures and internal conflicts,

for example, racial attitudes in white America. As Weitz (1972) pointed out, "Myrdal's 'American dilemma' is largely the societal disjunction between attitudes and behavior, with a tolerant value system conflicting with discriminatory normative patterns [p. 15]."

In sum, inconsistencies, or apparent inconsistencies, between attitudes and behavior often arise because the studies have not taken account of the social constraints that govern the situations in which the action is observed and the attitudes are assessed. When these social constraints are taken into account in interpreting some of the studies showing weak relationships between attitudes and actions (e.g., DeFleur & Westie, 1958; Linn, 1965), the findings fall neatly into place. Studies that have built social constraint deliberately into their designs as an intervening variable (e.g., Schofield, 1972; Warner & DeFleur, 1969) have produced the expected pattern of relationships between attitudes and actions. Situational constraints can also be taken into account in the types of measurement devices used. It is possible not only to use indirect or unobtrusive attitude measures to circumvent normative expectations but also to develop behavioral indexes that are relatively unaffected by situational pressures (Weitz, 1972).

3. Attitude theorists, including Allport as early as 1935, have pointed out that many attitudes and other intrapersonal factors are aroused in any given action situation, which jointly determine the person's behavior. Most studies of the attitude–action relationship have measured general attitudes toward a particular object and related these to behavior in a situation in which the subject interacts with that object or, more typically, with a specific member of that class of objects. To predict *action* toward a *specific* object in a particular *situation*, however, it would be helpful to assess the other attitudinal inputs that, along with general attitudes toward the object, determine behavior.

These include, first of all, attitudes toward the specific object with which the person is interacting. In a situation of interaction with a particular Italian, for example, attitudes toward that Italian or toward the subset of Italians to which he belongs probably account for much more of the variance in behavior than attitudes toward Italy and Italians in general. Second, attitude toward the action itself is an important factor to be considered. Fishbein (1967) has proposed a model in which the person's attitude toward the act—that is, toward performing a particular act in a given situation with respect to a given object—and his normative beliefs about the act are the two proximate predictors of behavior (or, more precisely, of behavioral intention). Attitude toward the object, according to this model, affects behavior insofar as it affects attitude toward the act and normative beliefs. In one experimental test of this model, Ajzen and Fishbein (1970) have found attitudes toward the act to be systematically related to behavior in a Prisoner's Dilemma game.[2] A third type of attitude that must be considered is attitude toward the situation. Rokeach (1969) has argued that prediction of behavior toward an object requires a systematic analysis of the interaction between attitudes toward the object and attitudes toward the situation in which the object is encountered.

Assessment of these diverse attitudes may well improve our ability to predict action. There is some evidence that predictive power is indeed enhanced when a variety of attitudes and related intrapersonal factors are taken into account (Wicker, 1971).

A further complication arises from the fact that the particular attitude object in which the investigator is interested is not the only object toward which the subject is reacting in the situation, and it may not be the most important one. The investigator may define the experimental situation as one of interracial contact, for example, and thus expect behavior to be highly correlated with racial attitudes. The subject, however, may define the situation primarily in terms of its authority structure or its task requirements, and his behavior may be most influenced by his attitude toward authority or toward the task, rather than his attitude toward his laboratory partner. We may, therefore, find only weak or even inverse relationships between racial attitudes and behavior in this situation. The low correlations between attitude and action found in many studies may thus be due to the investigators' failure to explore the entire range of attitudes that operate in the situation and to determine which of these are most relevant to the behavior observed.

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[2] Attitude toward the object—that is, the other player—showed much weaker relationships to behavior, but this is completely understandable because the subjects had no prior attitudes toward each other and their overall relationship was irrelevant to the experimental instructions and manipulations. It should also be noted that attitude toward the act, as conceived and measured by Fishbein, already reflects attitude toward the object along with other factors.

The joint operation of this congeries of attitudinal factors means that attitude toward a particular object does not have as much predictive power as some optimists have hoped it would have. It is one more reason for concluding, however, that the low correlations obtained in many studies of attitude–action consistency do not necessarily invalidate the attitude concept as such. They merely confirm the view that the use of attitude in the prediction of action requires a refined and detailed assessment of attitudes, as well as a thorough analysis of the action situation to which we hope to predict, including the social constraints that govern that situation and the variety of attitudes that are aroused within it.

Studies of the Effects of Counterattitudinal Action

The second line of empirical research that has raised questions about the attitude concept is research on the effects of counterattitudinal action. The theory of cognitive dissonance (Brehm & Cohen, 1962; Festinger, 1957) and Bem's (1967) reformulation of dissonance phenomena in terms of processes of self-perception have generated many experiments demonstrating attitude changes following upon discrepant action. These formulations have been very useful in calling our attention to a number of intriguing phenomena and providing handles for interpreting certain behavioral anomalies. Above all, they have stimulated thinking and research on a central issue in the study of social behavior: the emergence of attitudes out of an action context. They can be used, however, to support a rather narrow view of the action–attitude link, underestimating the dynamic quality of attitudes.

Within the dissonance and particularly within the self-perception framework, attitudes can readily be viewed as epiphenomenal—as cognitive adjustments the person makes after he finds himself acting in a certain way, designed to restore cognitive consistency or to label his action appropriately to himself and to others. Attitudes, in this view, do not perform a directive and dynamic function in behavior but are basically a form of self-indulgence. The experiments on counterattitudinal action, derived from these two traditions, could be used to challenge the concept of attitude as a predisposition to behavior on the grounds that observed relationships between attitudes and actions reflect the effects of action on attitude rather than the reverse. For several reasons, however, I feel that these studies do not warrant the conclusion that attitudes are epiphenomenal and lack explanatory power.

1. The studies of counterattitudinal action must be seen in the perspective of other empirical information—developed outside of a dissonance or self-perception framework—about attitude changes following upon action. A variety of studies of role playing (cf. Elms & Janis, 1965; Janis & Gilmore, 1965; Janis & King, 1954; Kelman, 1953) have shown some of the conditions under which advocacy of a counterattitudinal position in a role-playing context leads to attitude changes. Studies of the effects of role enactment in real life, such as the well-known study by Lieberman (1956), have demonstrated systematic changes in attitude as the person changed his role. Studies of intergroup contact, such as the Deutsch and Collins (1951) study of interracial housing projects, have shown that under the proper circumstances, such personal contacts may lead to attitude change. There is also some indirect evidence that the introduction of a new policy as a *fait accompli* may produce not only behavioral acceptance, but in its wake also attitude change (cf. Allport, 1954; Saenger, 1953).

In all of these studies, attitude change following upon action can be understood in terms of the usual processes by which, according to the various attitude theories, attitudes are formed and modified. That is, the nature and the consequences of the action bring to the fore challenging insights, role expectations, social supports, or direct experiences, which are the stuff out of which attitudes emerge. The data could, of course, be reinterpreted in dissonance or attribution terms, but they do not *need* to be explained in these terms. There is nothing anomalous about them from the point of view of attitude theory. Certainly they do not suggest a view of attitudes as epiphenomena and as mere cognitive adjustments. Thus, the recent experiments on counterattitudinal action represent only a limited range of the situations in which actions produce attitude change, and hence they provide an insufficient basis for generalizations about the nature of attitudes.

2. Dissonance theory is concerned with the person's reactions after he has engaged in a discrepant action. His reasons for engaging in the action are of interest to the theory only insofar as they affect the degree of justification for the action (as perceived by the actor), and hence the amount of dissonance aroused. The theory and the experi-

ments flowing from it have not been concerned with the specific motives, expectations, and situational forces—in a qualitative sense—that lead to the action. From a functional point of view, which my colleagues and I have taken in a series of experiments on counterattitudinal behavior (Kelman, Baron, Sheposh, & Lubalin, in press), the initial reasons for engaging in the action determine the kind of dilemma the person confronts and the way he proceeds to resolve it. Systematic attention to these reasons should thus allow us to make more refined predictions of the probability and nature of attitude change, as well as of other reactions, following upon discrepant action.

Most importantly, for the present purposes, an analysis of the person's reasons for engaging in the action may reveal that there were indeed some meaningful relationships between his initial attitudes and the action he has taken. It may well be that the impression, conveyed by the literature on counterattitudinal behavior, of a disjuncture between attitude and action—of attitude as merely a post hoc adjustment to action—derives from the design of studies in this genre, which deliberately separates the induced action from the motives behind it. Kelley's (1967) analysis of the way in which the typical dissonance experiment creates the illusion of freedom in the subject is very helpful in showing how this separation occurs. In view of their special character, these studies cannot provide a sufficient basis for concluding that attitudes do not determine action but merely follow upon it.

3. The internal logic of research on counterattitudinal behavior forces the view that attitudes, though cognitively adjustable, are indeed "real" and play a directive role in behavior. Most dissonance theorists would, I believe, concur with this conclusion. According to dissonance theory, a person changes his attitude because he has behaved in a way that did not "follow from" his original attitude. There is thus the presumption that the person normally expects his behavior to follow from his attitudes and perhaps even feels that it ought to follow from his attitudes (although dissonance theory does not spell out this part of the phenomenology), and that much of the time a person's behavior in fact does follow from his attitudes. Furthermore, attitude change reduces dissonance because it provides the person with an attitude from which his earlier behavior follows retroactively. The new attitude can fulfill that function only insofar as it is "real," and, if it is real, it should have a directive effect on subsequent behavior. That is, unless the counterattitudinal action was of the one-shot variety with no implications for the future (which, of course, it often is in dissonance experiments), the new attitude should, at least for some time to come, predispose the person to behavior that follows from it. If attitudes appear ephemeral in many dissonance experiments, it may be because the objects to which they refer and the behavior to which they are linked are ephemeral. However, when applied to real-life situations, dissonance theory does assume that attitudes are real and have implications for behavior.

This reasoning does not apply as clearly to a Bemian type of attribution analysis because, in Bem's view, attitudes in the sense of internal states are by definition not "real." Insofar as any attitudelike phenomenon can be real for a radical behaviorist, however, the attitude statements following counterattitudinal action are real in Bem's scheme; they certainly must be viewed as directive with respect to subsequent behavior. In Bem's analysis, these statements reflect self-perceptions, which he regards as functionally similar to the perceptions of others. Just as our perceptions of others clearly influence our behavior, so should our self-perceptions, according to the internal logic of Bem's scheme. If I see myself as liking brown bread, to use Bem's favorite example, I am more likely to buy brown bread, to serve it to my friends, and so on. In other words, when a person makes an attitude statement following upon counterattitudinal action, he is not merely labeling his behavior for the benefit of others or for his own amusement; his self-descriptions constitute relatively stable determinants of his subsequent behavior.

In short, I have argued that the logic of the research on counterattitudinal action requires a view of attitude as something more than an epiphenomenon and a cognitive adjustment, even though it does follow upon behavior. If one accepts the dissonance or attribution interpretations of the findings, one cannot at the same time cite these findings as evidence of the bankruptcy of the attitude concept.

Toward a Conception of Attitude as an Integral Part of Action

I have come to the defense of the attitude concept, not only because I have a vested interest in it, but because I consider some such concept as attitude to be essential for social-psychological analysis.

It is very difficult to imagine a social psychology without a construct designed to capture the conceptions of social objects and events that people bring to their interactions with each other and with their social institutions and that they share, to varying degrees, with other members of their diverse groups, organizations, and communities. To reject the construct on the basis of the current evidence would be to abandon prematurely a major conceptual tool. Nor does it seem fruitful to argue whether or not attitudes exist; the question is whether they can be conceptualized in ways that yield useful analyses and insightful conclusions about social behavior.

The priority need at this point is for more refined conceptualizations of attitudes, particularly—in view of the challenges presented by the two lines of research discussed above—of their linkage to action. In the remainder of this article I offer some notes toward such a conceptualization. Much of what I have to say has been stimulated by the research on attitude-discrepant behavior—both the work of others, carried out mostly in a dissonance or attribution framework, and the work that my colleagues and I have done within a functional framework—and attempts to put that work into a broader context in which the dynamic connections between attitude and action become more apparent.

Attitude, in my view, and in the view of most attitude theorists, is not an index of action, but a determinant, component, and consequent of it. Furthermore, it is not an entity that can be separated—functionally or temporally—from the flow of action, but is an integral part of action. Attitude and action are linked in a continuing reciprocal process, each generating the other in an endless chain. Action is the ground on which attitudes are formed, tested, modified, and abandoned.

From its very origin, an attitude is embedded in an action situation. A person's attitudes toward a particular object are formed in the course of his interaction with that object, or his interaction with other persons or with communication media transmitting information about the object. These interactions may yield information (*a*) about the object itself, obtained through observation, hearsay, or some combination of these; (*b*) about the attitudes of others and the prevailing group norms vis-à-vis the object; or (*c*) about the person's own relationship to the object, particularly in the form of feedback from his own actions toward it. The way in which the information is processed is a function of the motivational and cognitive contexts in which the interaction occurs. That is, the attitude the person forms is grounded in the particular functional significance that the situation has for him—the goals he is pursuing, the values he is hoping to maximize, the coping processes in which he is engaged (cf. Katz, 1960; Smith, Bruner, & White, 1956). Similarly, the attitude he forms is linked to the particular cognitive framework within which the interaction occurs—the values defining the situation, the issues under consideration, the other objects involved. The motivational and cognitive contexts in which the attitude is formed determine the nature of the resulting attitude—that is, the motivational basis of the attitude and the attitude system of which it becomes a part—and hence the conditions under which the person is likely to act on this attitude in one or another way, as well as the conditions under which the attitude is likely to change.

This dynamic view of the functioning of attitudes implies that attitude formation and change is a continually ongoing process. Attitudes develop out of the person's interaction with an object in a particular motivational and cognitive context. As he continues to interact with the object (directly or indirectly), the attitudes are tested, exposed to new information, sometimes filled out and shored up, and sometimes changed. In principle, attitudes should be developing and changing whenever a person is exposed to new experiences and information. In practice, changes are usually quite slow and gradual, because attitudes, once established, help to shape the experiences the person has with the attitude object. They affect the kind of information to which the person will be exposed, the way in which he will organize that information, and often (as in interpersonal attitudes) the way in which the attitude object itself will behave. Thus, an attitude by its very functioning (quite apart from any special motivations to maintain that attitude) tends to create the conditions for its own confirmation and to minimize the opportunities for its disconfirmation.

As a person interacts with an object toward which he has established an attitude, he is subject, then, to two competing sets of forces. On the one hand, the new information to which he is exposed produces forces toward change. On the other hand, the existing attitude creates forces toward stability—not only in the form of motivated resistances to change (stemming from the functional significance the attitude has for the person), but also and primarily in the form of confirmatory

experiences inherent in the day-to-day functioning of attitudes. Which of these two sets of forces will prevail on any given occasion—or what balance between them is achieved—depends on the nature of the existing attitude, of the new information to which the person is exposed, and of the situation in which the interaction occurs. In general, however, it can be said that both stability and change are parts of the essential nature of attitudes.

Attitude change processes are most likely to be set into motion if the person is sharply confronted with a discrepancy between his attitude and some item of new information. The discrepant information must be sufficiently strong and challenging to overcome the competing forces toward stability, and even then, as we well know, there are many ways of neutralizing the discrepancy short of attitude change. Three major types of discrepancy conducive to attitude change can be identified, corresponding to the three types of information about an attitude object mentioned above: (*a*) discrepancy between the attitude and information about reality (e.g., about the characteristics of the object or the implications of the policy to which the attitude refers); (*b*) discrepancy between the attitude and the attitudes of significant others; and (*c*) discrepancy between the attitude and one's own actions—which is the focus of the present analysis.

Discrepant Action and Attitude Change

Perhaps the most significant contribution of dissonance theory is that it has focused our attention on *discrepant action*, which constitutes a major arena for attitude change. Discrepant action, as used here, refers to any action toward an object that is out of keeping, from the actor's own point of view, with his attitude toward that object. We usually apply the term to actions that in some way "fall short" of the attitude—that is, fail to live up to the level of commitment that the attitude represents. Such failure may occur because actions in line with the attitude appear too costly and difficult, because they are inhibited by situational pressures, because their anticipated consequences are too negative, or because competing motives impel the person to follow a different course. Discrepant actions, however, may also take the form of actions that "exceed" the person's attitude—that are at a higher level of commitment than that implied by his attitude. Situational pressures or social facilitation may induce him to act in ways that are more generous, more courageous, or more tolerant than his attitude requires.

TABLE 1

The Conduciveness of Action to Attitude Change

Components of action	Processes generated by action	
	Motivational	Informational
Context of the action	Meeting task and situational demands	Coding information about object and social expectations
Contemplation of the action	Justification of action	Definition of object and of self vis-à-vis object
Consequences of the action	Anticipation and fulfillment of new role requirements	Integration of new experiences

Commitment to a discrepant action may create the conditions for reassessment and revision of related attitudes and thus set a process of attitude change into motion. Attitude change, in the present view, is not a reaction to the discrepancy as such, aimed at removing the inconsistency between action and attitude, but rather an outcome of various motivational and informational processes that are generated by the action. Table 1 presents a classification of six such processes, all of which are likely to come into play, at some level of intensity, in any action situation. Attitude change may be mediated —under the appropriate circumstances—by any one of these processes or any combination of them.

The columns of the table refer to motivational and informational processes, respectively. The rows refer to three components of the action, each of which generates both motivational and informational processes that are potentially conducive to attitude change. The first row refers to the processes generated by the context of the action—the situation in which the person decides to act and carries out the action. The second row refers to processes generated by the contemplation of the action—the person's backward look at the action he has taken and its implications. The third row refers to processes generated by the consequences of the action—the actual and anticipated events that the action brings in its wake. For any given unit of action, the three rows correspond to a focus on the present, on the past, and on the future, respectively. Proceeding row by row, I shall describe the motivational and informational processes that may come into play.

1. *Context of the action.* Let us assume, for illustrative purposes, that a student is urged by his friends to participate in preparing and signing a statement to the university administration protest-

ing against classified research—an action that does not flow spontaneously from his current attitudes. To the extent that he is motivated to meet the demands of this particular situation, this request sets a decision-making process into motion. He assesses the options available to him and their respective consequences; he decides whether or not to engage in the action; and, if his decision is positive, he determines the nature and extent of his participation and previews the content of what he will be doing. In the course of the decision making, he is likely to think through the issues raised by the action, to consider appropriate information, and to review relevant arguments. These efforts, in turn, may involve him in an active process of reevaluating his position—of reconsidering the implications of his original attitudes for his own values and for his relationship to important reference groups. The process of reexamination may provide attitudinal support for the action he has been asked to take and may thus lead to attitude change, in the form of internalization or identification or some combination of the two (Kelman, 1961). In other words, attitude change may emerge out of the process of reexamining his attitudes motivated by the situational context of the action.[8]

A high degree of choice about the induced action is particularly conducive to attitude change (Kelman, 1962). If the person is undecided as to whether to carry out the action, then the higher his degree of choice, the more likely he is—in the process of arriving at the necessary decision and firming it up—to reexamine his attitudes and to marshal forces in support of the action that he finally selects. He may, of course, decide against taking the induced action, and this negative decision would now have considerable attitudinal support. But if he does take the action, it is likely to be accompanied by attitude change. A high degree of choice may facilitate attitude change even in the absence of indecision, that is, even if the situational forces are so strong that the person is automatically inclined to take the induced action. The fact that he is given the choice may force him to engage in a process of active self-persuasion to find attitudinal support for the action he has already decided to take.

Once the decision to act has been made, the person's motivation to meet the demands of the task in which he has agreed to participate may bring a further process of reexamination of attitudes into play—depending, of course, on the specific nature of the action. In the example cited before, preparation of the protest statement requires review of the issues and development of appropriate arguments. If the task is to be carried out effectively, the person has to think of supporting arguments and to present them in a convincing way. In the course of engaging in this process, he may become aware of nuances and implications of the issues that he had not considered before and may thus succeed in persuading himself. This is one reason for the potential effectiveness of role playing—even though it may be a mere exercise demanding no commitment from the actor—in producing attitude change, particularly if it requires improvisation (cf. King & Janis, 1956). In reverse role play among conflicting parties, for example, each player, if he is to perform his task successfully, must consider and present the other party's position from the other's point of view. He may thus gain insights into the other's position which were not previously available to him, and modify his own view accordingly.

The motivational processes generated in the action situation are accompanied by informational processes that are similarly conducive to attitude change. In the course of deciding to act and carrying out the action, the person processes various items of information that have attitudinal relevance. Some of this information is conveyed by the context of the action; some is deliberately sought out by the person to enable him to decide and to act; some is generated by the action itself. He may thus acquire data about the characteristics of the object, about the value implications of various policies, about the distribution of opinions on the issue, and about the expectations held by relevant reference groups. These are the kinds of information that typically enter into the formation of attitudes. Exposure to new information of this variety, under the appropriate motivational circumstances, provides the raw material for attitude change.

2. *Contemplation of the action.* Once a person has taken action, the action itself becomes a datum with which he has to contend. Contemplation of

[8] In the case described, attitude change actually occurs *before* the action, that is, in the course of deciding to act. The final action is thus consistent with the person's newly developed attitude, and strictly speaking, therefore, it is not a discrepant action. The total situation, however, can fairly be described as one of discrepant action because the decision to act cannot be sharply separated from the action itself.

what he has done may raise questions about the meaning of the action, about its implications for his self-image, and about the nature of his ongoing relationship to the object of the action. These musings may generate motivational and informational processes conducive to attitude change. This component of action, essentially, is the one on which both dissonance and attribution interpretations focus.

On the motivational side, the fact that a person has taken a particular discrepant action may have negative implications for his self-evaluation. Contemplation of the action may arouse guilt, shame, or some other negative affect, which in turn generates cognitive efforts to justify the action. The precise nature of the reaction depends on the form the action took, the norm that it violated, and the specific motivational system it has thrown out of balance. For example, my colleagues and I (Kelman, Baron, Sheposh, & Lubalin, in press) have distinguished between moral and hedonic dilemmas and proposed that discrepant actions arousing these different dilemmas will generate different resolution processes, often showing different patterns of relationship to the standard dissonance parameters. Efforts to justify the action, for whatever reason, may bring about attitude change, more or less along the lines proposed by dissonance theory. A morally dissonant act, for example, can be justified if the person can convince himself that the object he harmed or the cause he betrayed was not worthy of his loyalty; a hedonically dissonant act can be justified if he can convince himself that the discrepant behavior was really enjoyable and profitable.

Discrepant action is more likely to generate justification processes conducive to attitude change, the greater the individual's personal involvement in the action. Knowledge that he has acted in a certain way toward an object becomes an important datum in the person's self-evaluation and in his evaluation of the object if he regards the action as internally motivated and representative of the self. The sense of personal involvement—and hence the probability of attitude change—should be enhanced if the action is freely chosen, if it requires effort and initiative, and if "it represents a complex of interrelated role behaviors within some social system rather than a specific, isolated act [Kelman, 1962, p. 107]."

Contemplation of the action also provides new information—relevant to the definition of the object and to the person's self-definition—on which attitude change may be based. The title of the classic collection of children's definitions, *A Hole Is to Dig*, elegantly describes our tendency to define objects in terms of the way in which we characteristically act toward them. The action one takes toward an object tends to become a salient characteristic of that object. It influences subsequent interactions with the object and receptivity to further information about it and thus, eventually, evaluation of the object. For example, if a person has acted in support of a particular policy, he will be inclined to define it, in part, as "a policy that I have supported." This new definition, in turn, may contribute to a new evaluation of it as "a policy that is worthy of my support." Such an outcome can be derived from an analysis of the usual ways in which people process information, without invoking a special need for consistency.

The information provided by contemplation of one's action also has relevance for his self-definition. In line with a Bemian analysis, if a person acts toward an object in a certain way, he will (given the proper stimulus conditions) attribute to himself the corresponding attitude. Beyond that, if the action touches on central concerns, it will contribute to the person's more enduring definition of his self. Thus, repeated friendly association with another person will lead him to define himself as the other's friend; repeated participation in protests and demonstrations will lead him to define himself as an activist and radical. The person's conception of the kind of person he is, in turn, plays a major role in determining his future actions and interactions. Thus, by being integrated into the person's self-definition, the action-generated attitude gains stability and generality.

3. *Consequences of the action.* Most actions, at least outside the laboratory, have consequences beyond the immediate situation. When taking action, therefore, the person prepares himself psychologically for its anticipated consequences. For example, in preparation for the anticipated necessity of explaining and defending his action to others, he may review the issues involved in the action, rehearse the opposing arguments, and reassess his own attitudes toward the object of the action. Out of this process, attitude change may emerge, particularly since the person is motivated to find arguments supportive of the action he has taken. If he not only knows these arguments but actually believes them, his ability to offer a comfortable and convincing defense of his action is further enhanced.

The motivation to bring one's attitude into line with his action is especially strong if the action commits the person to continuing association or public identification with the object and hence, at least implicitly, to future action in support of it. Many simple acts, such as buying a product, making a pledge, or allowing one's name to be placed on a mailing list, have this consequence. Of special interest, however, are those actions that involve commitment to a new role, such as the act of joining an organization, moving into a new neighborhood, starting a new job, or entering a training program. These actions represent long-term commitments, which would be costly to break; they involve the person in an extensive set of role relationships; and they often become salient features of his public identity. Under these circumstances, the development and strengthening of appropriate attitudes in preparation for the new role becomes particularly crucial. The person is open to and actively searches for new information that lends attitudinal support to his action and thus makes his anticipated role performance more effective, more comfortable, and more rewarding. These preparatory processes are likely to facilitate attitude change in the direction of the action taken and of the future actions anticipated (cf. Kelman, 1962).

Not only the anticipated but also the actual consequences of action generate motivations for attitude change, particularly when the action takes the form of commitment to a new role. The requirements of the new role produce strong forces toward reexamining one's attitudes and making them congruent with the expected role performance. Thus, for example, if the worker in Lieberman's (1956) study who becomes a foreman is to be effective in making the choices and taking the actions required by his new status, he must develop the appropriate attitudes. If he is to be able to defend the position of management, he must know it and is likely to adopt it. Moreover, the tendency of others to cast him into the role of spokesman for management and expect him to take management-oriented positions has the self-fulfilling effect of binding him into the role so that he becomes what others expect him to be. To take another example, the white housewife in the Deutsch and Collins (1951) study who has moved into an interracial housing project is motivated to reexamine her racial attitudes because she is now involved in regular interaction with black neighbors, because she is called on to defend her decision to move into the project, and because she is identified by others as a resident in interracial housing. In short, enactment of the new role in which the person finds himself as a consequence of action sets into motion various forces conducive to attitude change, not the least of which are the expectations conveyed by others and their tendency to attribute certain attitudes to him and to treat him accordingly.

On the informational side, a frequent consequence of action is to provide the person with new experiences, which may expose him to new information. The experiences may be in the form of more extensive contact with the attitude object itself. Thus, the worker-turned-foreman in Lieberman's study has increased contact with management, which, given his openness to new information supportive of his new role, is quite likely to provide him with raw material for attitude change in the direction of management. The white housewife in the Deutsch and Collins study has new opportunities to interact with her black counterparts in daily activities and around common concerns, which, depending on what happens in the course of the interactions and on her motivation to receive favorable information, may produce favorable attitudes. The literature on intergroup contact suggests that contact at least provides the potential for new experiences conducive to attitude change. Favorable change is most likely if the contact involves equal-status interactions and if it is sanctioned by legitimate authorities—conditions that were met in the Deutsch and Collins study.

Action may also provide the person with new social experiences that indirectly yield new information about the object. His action in support of a particular group or policy, for example, may elicit praise from others, or at least fail to elicit the disapproval he had anticipated; or he may discover unexpected agreement, at least within his relevant reference groups, with the stand he has taken. These new items of information about group consensus and about the social acceptability of his action may contribute to attitude change via the process of identification. In short, as the person integrates new experiences consequent to the action, whether these involve direct contact with the object or contact with social norms about it, forces toward attitude change may well be set into motion.

To sum up, Table 1 presents a preliminary mapping of the processes generated by discrepant action. I have tried to analyze attitude change following upon action in terms of the variety of conditions, motivational and informational, that

govern the formation and functioning of attitudes. The analysis is intended to show that the action–attitude sequence can be understood if we conceive of attitude as an integral part of action. The need now is for more precise propositions about the determinants of attitude change within each of the six cells of Table 1. Such propositions can be derived from a variety of theoretical sources and integrated into a functional analysis. Each cell of the table, in fact, suggests a social-psychological theory that is particularly relevant as a source of propositions. I have in mind the theories of decisional conflict and information processing for the cells in the first row, dissonance and attribution theories for the cells in the second row, and role theory and contact theory for those in the third row.

Action as a Step in the Attitude Change Process

The discussion in the preceding section proceeded from the assumption that a discrepant action has occurred, and then explored the various ways in which this event may lead to attitude change. It is often true, however, that action—even discrepant action—does not merely precipitate attitude change, but is itself an integral part of an ongoing attitude change process.

When we examine a person's reasons for taking an apparently discrepant action, we may find that the action is not completely out of keeping with his attitudes. Though he may be responding to situational demands, to interpersonal pressures, to social facilitation, or to other extraneous influences, his response need not be entirely unwilling. The action may reflect an incipient attitude change, a movement toward a new attitude that has not yet been crystallized and to which he has not fully committed himself. Extraneous forces may thus precipitate an action for which the person was already partly prepared. The action in turn contributes to attitude change, in the sense that it provides an occasion for the person to sharpen the new attitude and commit himself to it. In short, attitude change in relation to discrepant action is not always an entirely *reactive* process, but may well be an *active* process in which action plays a catalytic role.

This process can be understood quite readily if we assume, as a number of attitude theorists do, that an attitude indicates a range, rather than just a point on a scale. Specifically, I propose that any given attitude represents a *range of commitment* to the attitude object. This notion clearly overlaps with the concept of latitude of acceptance (Sherif & Hovland, 1961), except that it refers to action potential—to the kinds of action the person is prepared to take—rather than to judgment. Within a person's range of commitment, one can identify a point representing his modal level of commitment (which is presumably the point to which a person's position on an attitude scale would correspond, if it were possible to devise situation-free attitude measures). A person's behavior vis-à-vis the attitude object fluctuates around that modal point. In some situations he may display a level of commitment closer to the upper end of his range; in others, a level closer to the lower end.

The notion of a range of commitment suggests not only fluctuations around a modal level, but also a readiness to shift one's modal level to another position within the range, given the proper circumstances. The degree of readiness for such a shift, the direction it would take, and the circumstances that might precipitate it depend on a variety of qualitative factors. The possibility of such a shift to a higher or lower level within the person's range may be entirely latent, or it may be actively entertained or even desired by the person. For example, a person may be willing or even eager to become more actively involved in an organization or a profession—to give more of his time to it or to take a leadership role—but the opportunities to do so have not presented themselves or other competing forces have kept him at his present level. Conversely, a person may feel ambivalent toward an organization or a social group to which he belongs, and be willing or even eager to withdaw from it, but the opportunity to do so gracefully in the face of competing considerations has not arisen.

Within this framework, one can see how an action can simultaneously flow from an attitude and mediate changes in that attitude. Table 2 sketches out some classes of situations in which this kind of process may occur. The first row describes the general characteristics of these situations: (*a*) A person (P) finds himself in a situation calling for action at a level higher or lower than his modal level of commitment, but still within his range. (*b*) For one or another reason, he decides to take this action, which thus involves him at least temporarily at a level of commitment higher or lower than his usual level. (*c*) Having taken the action, he becomes subject to the various action-generated forces conducive to attitude

TABLE 2

Action as a Step in an Ongoing Process of Attitude Change

Case	Preaction attitude	Nature of action	Impact of action
Generic case	Action called for is higher (lower) than P's modal level of commitment, but within his range of commitment.	P responds to opportunities, challenges, or internal forces to take action corresponding to a level of commitment higher (lower) than his modal level.	Action generates processes conducive to attitude change in form of higher (lower) modal level, and heightened (lowered) and/or narrowed (widened) range of commitment.
Case 1: Opportunity	P is prepared (e.g., through anticipatory socialization) for level of commitment higher (lower) than current modal level.	P utilizes opportunity to adopt previously anticipated role corresponding to a higher (lower) level of action commitment.	Role enactment generates experiences, task requirements, and social expectations that lead to higher (lower) level of commitment.
Case 2a: Social challenge	Level of commitment higher (lower) than current modal level is latent in P's attitude, but unexpressed because of cross-pressures or other competing forces.	P yields to social pressure to take action in line with expectations of one reference group.	Action generates motivational and informational processes that reinforce and facilitate movement to higher (lower) level of commitment.
Case 2b: Cognitive challenge	Level of commitment higher (lower) than current modal level is latent in P's attitude, but unexpressed because of failure to draw implications of own values.	P confronts implications of own values to take action in line with his revised self-expectations.	
Case 3: Self-mobilization	P wants to move to level of commitment higher (lower) than current level, but is kept back by cross-pressures or other competing forces.	P takes action as part of deliberate effort to mobilize internal and external support for movement to a new level.	Action places P in new psychological and social situation that requires and supports shift to higher (lower) level of commitment, and prevents backsliding.

Note. P = person.

change, which were discussed earlier; as a result he may manifest change by raising or lowering his modal level and his entire range, as well as by narrowing or widening the range. This generic case merely describes what happens, but does not explain why the person decided to take action discrepant from his modal level in the first place. For this, let me turn to the more specific cases.

Case 1 can be exemplified by the workers in Lieberman's study. The fact that a given worker was asked to be a foreman or a steward was probably not a mere coincidence. The chances are that he was available for this level of commitment to management or the union, that he had been moving in that direction for some time, that he had been building relevant attitudes in preparation for it—in short, that he had been undergoing a process of anticipatory socialization. The invitation to become a foreman or a steward thus represented an opportunity for him to adopt a role for which he was already prepared. It must be noted that although he was clearly ready for attitude change, his attitudes were not yet at the level of a functioning foreman or steward. It took the action commitment and all the forces that it generated to bring a new attitude—at a higher level of commitment—into sharp focus, to reorganize it, and to stabilize it.

Another example of Case 1 would be the person who joins a social movement. The point of joining represents a clearly identifiable, highly committing action step. The decision to take the leap and to define himself as a member is a critical one, and major changes in attitude are likely to follow from it. Yet, the action was obviously not unrelated to these new attitudes. The changes were no doubt already taking place, slowly, perhaps imperceptibly, prior to the action. The act of joining the movement helped to speed up the rate of change and to sharpen and crystallize the new attitudes. Similar considerations apply when a person decides to leave a movement. Typically, his commitment to the

movement has been becoming weaker for some time, but he has not quite reached the point of changing his modal position. When a turning-point in his own life or in the life of the movement finally precipitates his decision to quit—for which he had been gradually preparing himself—his attitudes change sharply and rapidly.

Case 2a might be exemplified by the student from a fairly conservative background whose political views and commitments have been moving in a new direction. He has settled into a generally liberal position, marked by support for various causes but not a high level of personal involvement in them. The possibility of a deeper commitment to some of these causes is within his attitudinal range, but he is not quite ready for it because he is not willing to break entirely with his family and home community, or because he is not prepared to pay the price of higher commitment, or because he has not fully sorted out his ideas on the matter. As often happens, this individual may find himself in a situation in which social facilitation or social pressure from his current associates induces him to participate in political action that exceeds his modal level of commitment, and this action in turn generates further attitude change. Although situational forces played a major role in inducing the action, he was at least partly ready for it. In fact, he may have had a latent interest in trying out this higher level of commitment but needed the extra push that social pressure provided. Other examples of Case 2a are the conscience-stricken anti-integrationist who welcomes external pressures, in the form of laws or *faits accomplis,* that constrain him to go along with integration (Allport, 1954); and the "latent liberal" in the South who is racially prejudiced for reasons of conformity but quite ready to change once the social norms point in the direction of greater tolerance (Pettigrew, 1961).

Case 2b is structurally similar to 2a, except that movement to a different level of commitment is inhibited, not by the existence of cross-pressures, but by the person's failure to make certain cognitive connections. For example, a young man may have serious moral compunctions about war and thus be a latent conscientious objector. He may never have considered taking this position, however, because he was unaware of the existence of this social category and because it never occurred to him that response to the draft law was within a person's domain of moral choice. Once confronted with the possibility of taking this action, he may acquire a new perspective on the problem and recognize himself as someone who has been a conscientious objector all along without ever knowing it. This example and others of this type usually represent some mixture of social facilitation and cognitive challenge.

Case 3 represents the most interesting illustration of action as a step in an ongoing process of attitude change. It refers to a person who really wants—with at least some degree of consciousness—to move to a different level of commitment, but who, for various reasons, is not quite ready to do so. Examples would be the political activist who wants to engage in more militant action but is reluctant to pay the price it would entail; the religious seeker who would like to make a complete commitment but lacks the faith to take the final leap; or the graduate student who is profoundly interested in the field of his choice but hesitates to make a lifelong commitment to it. In such a situation, the person may deliberately take an action that is beyond his current level of commitment. The political activist may decide to engage in an act of civil disobedience; the religious seeker may decide to spend a summer with the sect he is attracted to; the graduate student may accept a research assistantship in an area he knows he will find engrossing. These actions do not represent a total commitment, but they do represent identifiable steps in that direction.

To some extent, the person may take this step in order to test his level of commitment, on the assumption that true commitment implies a readiness to take actions of this kind. The action thus provides him an opportunity to demonstrate his commitment to himself (as well as to others)—or perhaps to determine, once and for all, that he really lacks the commitment. Even more intriguing is the possibility that such actions are taken to bring about a deeper level of commitment. The person is at least vaguely aware that by taking this action he places himself in a situation in which he will gradually be propelled into increasing commitment. The action creates some irreversible consequences. For example, by committing an act of civil disobedience, the political activist risks jail, which in turn may greatly limit his future opportunities. Furthermore, the person's action creates expectations in others—both those who share the action and those who witness it—who will then treat him as one who is fully committed to the cause. By setting up these expectations in others,

he makes it more difficult for himself to withdraw, for both practical and psychological reasons. In short, by taking the action, he is not only testing and mobilizing his own commitment but also mobilizing external forces to support a strengthened commitment and to prevent him from backsliding. The array of forces generated by the action thus helps him to shift to a new level of commitment and to jell an attitude that was not yet fully formed prior to the action.

In Case 3, as in the other cases summarized in Table 2, action creates commitment and generates attitude change. Yet, at the same time, the person's readiness for such change is partly responsible for his original decision to take the action. Case 3 is of particular interest since it represents a person's deliberate attempt to use a partially discrepant action as a vehicle for attitude change. It demonstrates the potential interactions, both between deliberate choice and external constraints, and between action and attitude change. It shows most clearly the engagement of attitude and action in a continuing, reciprocal, circular process. Not only is attitude an integral part of action, but action is an integral part of the development, testing, and crystallization of attitudes.

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Instrumental Conditioning of Autonomically Mediated Responses in Human Beings

H. D. KIMMEL *University of South Florida* [1]

Forty-five years ago, John Watson (1928) said, "We may earn our bread with the striped muscles but we gain our happiness or lose it with the kind of behavior our unstriped muscles or guts lead us into [p. 349]." The unfolding of events in the years following Watson's statement, however, contrived somehow to obscure its significance, at least until fairly recently. I would like to review some of the circumstances under which American psychology, in the colorful language of Gregory Razran (1972, p. 147), managed to "lose its guts" and, then, I would like to describe some of the recent empirical work on human subjects which has brought about their rediscovery. I will also try to identify some of the theoretical and practical implications of this recent work, since, as Watson implied, its significance is applied as well as basic.

Foundations of the Belief That Autonomically Mediated Responses Are Not Intrumentally Modifiable

1928–1961

In the same year that Watson was emphasizing autonomically mediated behavior, Stefan Miller and Jerzy Konorski (1928) published their landmark article which distinguished for the first time between classical and instrumental conditioning. Miller and Konorski were the first to suggest that responses mediated by the autonomic nervous system cannot be modified instrumentally. A few years later, quite independently, Skinner (1935) proposed an analogous categorization of the two types of conditioning, but he did not at first exclude autonomic responses from his instrumental category. In fact, one of his hypothetical examples of instrumental conditioning, when the to-be-conditioned response and the unconditioned response are of the same original form, was salivation to light, reinforced by food. Konorski and Miller (1937) responded to Skinner's paper and criticized it vigorously. Referring specifically to his hypothetical example of salivation to light being reinforced instrumentally by food, they said, "Being a glandular reaction, salivation cannot by any means be made a conditioned reaction of the new (i.e., instrumental) type. Skinner's case is not so much imaginary as impossible [p. 271]." In reply to their criticism, Skinner (1937) maintained an open-mindedness regarding autonomic instrumental conditioning. He wrote:

> That responses of smooth muscle or glandular tissues may or may not enter into Type R (i.e., instrumental), I am not prepared to assert. I used salivation as a convenient hypothetical instance of simultaneous fused responses of both types, but a skeletal response would have done as well. The child who has been conditioned to cry "real tears" because tears have been followed by positive reinforcement (e.g., candy) apparently makes a glandular conditioned response of Type R, but the matter needs to be checked because an intermediate step may be involved [p. 279].

Skinner's open-mindedness altered somewhat during the one year between his response to Konorski and Miller and the publication of *Behavior of Organisms* (Skinner, 1938). Perhaps because of the inconclusiveness of his experiment with Delabarre, attempting instrumental modification of vasoconstriction, or possibly because both Schlosberg (1937) and Mowrer (1938) adopted Konorski and Miller's position regarding autonomic responses, he wrote the following in *Behavior of Organisms:* "There is little reason to expect conditioning of

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Requests for reprints should be sent to H. D. Kimmel, Department of Psychology, University of South Florida, Tampa, Florida 33620.

Type R in an autonomic response, since it does not as a rule naturally act upon the environment in any way that will produce a reinforcement, but it may be made to do so through instrumental means [Skinner, 1938, p. 112]." The last phrase indicates some remaining doubt. But by the time *Science and Human Behavior* (1953) appeared, Skinner's position had become almost indistinguishable from that proposed 25 years earlier by Miller and Konorski (1928), namely:

> Glands and smooth muscles do not naturally produce the kinds of consequences involved in operant reinforcement, and when we arrange such consequences experimentally, operant conditioning does not take place. We may reinforce a man with food whenever he "turns red", but we cannot in this way condition him to blush "voluntarily". The behavior of blushing, like that of blanching, or secreting tears, saliva, sweat, and so on cannot be brought directly under the control of operant reinforcement. If some technique could be worked out to achieve this result, it would be possible to train a child to control his emotions as readily as he controls the positions of his hands [p. 114].

It would not be fair to Skinner to leave the impression that his open-mindedness was completely gone, however. I say this because of the personal encouragement he gave me just after I had begun to conduct my first experiment on instrumental autonomic conditioning using the galvanic skin response (GSR) of humans. Skinner visited the University of Florida in the early spring of 1959 to give an invited lecture, just after I had begun to run my first subjects. He graciously came to the laboratory to look at the equipment and discuss our research plans. There were no reservations discernible in his urging that we pursue the undertaking. In fact, because of his reservations regarding punishment as a possible means of attaining response suppression, Skinner suggested that we add an experimental condition in which subjects receive reinforcement contingent on nonresponding. It still was, as he had written earlier, "a question for experiment [Skinner, 1937, p. 279]."

Nevertheless, the *public* statements of most major figures in American psychology transformed Miller and Konorski's original exclusion of autonomic responses into unquestioned dogma. As late as 1961, even while at least five different investigators were collecting data and beginning to report preliminary positive findings, Gregory Kimble (1961) wrote:

> Although autonomically mediated reactions such as the GSR and vasoconstriction are readily conditioned classically, they seem to be impossible to condition by instrumental methods. The Pavlovians simply state (without documentation available in English) that glandular responses cannot be conditioned instrumentally. Mowrer (1938) was unsuccessful in an attempt at instrumental conditioning of the GSR, and Skinner (1938) reports that he and Delabarre could not condition vasoconstriction by making reinforcement contingent upon the response. *Thus, for autonomically mediated behavior, the evidence points unequivocally to the conclusion that such responses can be modified by classical, but not instrumental, training methods* [p. 100, italics added].

Evidence that Kimble was aware of at least one positive result at the time he wrote his "unequivocal" statement was given by him in April 1960 in an invited address he gave to the Southeastern Psychological Association. In his talk, entitled "Total Behavior: The Conditioned Eyelid Reflex," Kimble referred to the fact that one of the research papers on the same program (H. D. Kimmel & Hill, 1960) reported successful operant conditioning of the GSR in humans. He indicated that it would take more than a single study to overcome so well established a generalization. To his credit, it must be added that subsequent positive results *did* make Kimble change his position, and in writing: "These data seem to settle the immediate issue. Apparently an autonomic, involuntary, normally elicited (rather than emitted) response can be instrumentally conditioned as single-process theory maintains it should be [Kimble & Garmezy, 1968, p. 319]."

Initial Positive Findings

1957–1961

Toward the end of the 1950s, as mentioned previously, an apparent change in the *Zeitgeist* regarding the possibility of instrumental autonomic conditioning was occurring. At least five different groups of investigators independently were attempting to modify human autonomic responses instrumentally, and all eventually obtained results more or less capable of positive interpretation. Because of their historical priority, I would like to mention them briefly. In the Soviet Union, Lisina (1960) trained human subjects to control digital vasoconstriction and dilation by means of instrumental shock-escape and avoidance, plus visual feedback regarding vasomotor reactions. Her results were first described in English by Razran (1961). Lisina found that her subjects could learn to terminate shock by means of digital vasodilation, overcoming the unconditioned tendency for shock to elicit vasoconstriction. They also could learn to prevent shock entirely by vasodilating instead of vasoconstricting to a light–sound combination presented in advance of shock. None of the subjects showed

these instrumental conditioning effects without real-time feedback of their vasomotor behavior.

At about the same time at the University of Toronto, Mandler (Mandler, Prevan, & Kuhlman, 1962) was attempting to influence the frequency of occurrence of unelicited GSRs, by presenting response-contingent light stimuli. In this study, reinforcement periods were alternated with extinction periods over several days. Six of the nine subjects made significantly more GSRs during reinforcement than during extinction. But, because overall GSR frequency tended to decline throughout the experiment, in both reinforcement and extinction periods, its results were interpreted as negative with respect to the possibility of instrumental autonomic conditioning. Mandler also attempted to train human subjects to discriminate changes in their heart rates (Mandler & Kahn, 1960), but reported that the subjects could do this only when respiration changes occurred.

While these investigations were underway in the Soviet Union and Canada, three other independent autonomic instrumental conditioning projects were in progress concurrently in the United States. Two of these involved heart rate as the autonomic response, with both discriminative reward and Sidman avoidance techniques under study. The discriminative reward work, done by Harwood (1959, 1962), yielded negative results for both increases and decreases in heart rate, but there was some evidence suggesting that very brief decelerations (i.e., changes in the interbeat interval) might be higher in frequency during the presence of S^D than during S^Δ. Somewhat more promising results were found by Shearn (1960, 1961) with the Sidman technique, although no significant reduction in the number of shocks received was attained. In Shearn's study, an amplified auditory presentation of the subject's heart beat was delivered to him during the conditioning session, somewhat analogous to Lisina's feedback technique, but not presenting heart *rate* directly.

The fifth independent investigation of instrumental autonomic conditioning in humans began in my laboratory at the University of Florida in 1958. Our first study employed pleasant and unpleasant odors to reinforce unelicited GSRs (Kimmel & Hill, 1960). In this study the only evidence of an instrumental conditioning effect was that subjects who received response-contingent reinforcement increased sharply in response rate at the beginning of extinction, while subjects who received non-response-contingent reinforcement decreased in response rate when extinction began. This effect did not depend on which odor was used. We then shifted to a dim white light as reinforcement, with the subject seated in a totally dark room, and instrumental conditioning effects were found during both acquisition and extinction (Fowler & Kimmel, 1962). Postexperimental interviews were used to eliminate subjects who reported anything resembling awareness of a response-reinforcement contingency.

Early Extensions

1962–1966

Following publication of the results of these initial studies, several other laboratories took up the task of investigating instrumental autonomic conditioning in humans. Within the five-year period 1962–1966, the reliability of the initial positive findings with the GSR was firmly established (Greene, 1966; Greene & Nielsen, 1966; Johnson, 1963; Kimmel & Kimmel, 1963; Shapiro, Crider, & Tursky, 1964), and the GSR work was extended to include investigation of instrumental punishment and avoidance (Grings & Carlin, 1966; Kimmel & Baxter, 1964; Kimmel, Sternthal, & Strub, 1966; Senter & Hummel, 1965). All of these studies confirmed the conditionability of the human GSR by instrumental reinforcement. Four additional studies done during this period provided evidence that instrumental modification of the GSR cannot easily be attributed to mediation via an instrumentally conditioned or voluntarily emitted skeletal response (Birk, Crider, Shapiro, & Tursky, 1966; Crider, Shapiro, & Tursky, 1966; Rice, 1966; Van Twyver & Kimmel, 1966). In the Birk et al. (1966) study, a human subject volunteered to be partially curarized, greatly reducing the possibility of skeletal activity, yet showed the effect of instrumental reward training in his GSR.

Extension of the scope and confirmation of the reliability of instrumental heart rate conditioning also was accomplished during the 1962–1966 period. Two studies of heart rate conditioning using an avoidance paradigm were reported, both with positive results. In the first of these avoidance studies, done by Brener (1966), an auditory stimulus which varied directly in frequency with the subject's heart rate was continuously fed back in real time to the subject. Heart rate slowing was the avoidance response. In a second study, Brener (1966) suc-

ceeded in training subjects to avoid shock by means of heart rate speeding. In this study visual feedback was presented, but only at times of criterion responding. Another experiment on instrumental heart rate conditioning, using a punishment paradigm, was reported by Frazier (1966). The subjects in Frazier's study were correctly informed that shock might be delivered whenever a visual stimulus was present and that no shocks would be delivered when the visual stimulus was absent. During the presence of the visual stimulus, declines in heart rate from the rate of the previous minute were punished by shock. Maintenance of the same heart rate prevented the shock, so that this study could also be placed in the avoidance category. No response-correlated feedback was used in Frazier's study, and its results are contaminated by the obvious fact that the administration of shocks during the presence of the visual stimulus may, itself, be sufficient to elevate heart rate or prevent its decline, either directly or via classical conditioning. Heart rate slowing was also obtained by Engel and Hansen (1966), using monetary reward, but without informing the subjects that reward was contingent on heart rate or any other response. No feedback other than the rewards was given. In the Engel and Hansen (1966) study, respiration data did not support a mediation hypothesis. Control of *both* slowing and speeding of heart rate within the same subjects was obtained by Brener and Hothersall (1966), using auditory feedback correlated with heart rate. The subjects were instructed to try to keep the pitch of the auditory stimulus high when a green discriminative stimulus was on and to try to keep its pitch low when a red stimulus was on. They were not informed that the pitch of the sound was correlated with heart rate. Respiration records indicated that both heart rate and breathing patterns differed in the high and low conditions, but only one of the subjects guessed that respiration might be involved in controlling the sound. None of the subjects guessed that heart rate was involved.

In addition to these GSR and heart rate studies, two experiments on instrumental conditioning of unelicited digital vasoconstriction (Snyder & Noble, 1965, 1966) were also done during the 1962–1966 period. These studies replicated the procedures previously employed with the unelicited GSR and they showed substantially greater increases in response rate during acquisition than had been shown with the GSR and heart rate. Greater resistance to extinction than that found with the GSR and heart rate was also reported. In the second of these studies (Synder & Noble, 1966), it was reported that the conditioning group did not differ from a yoked control group in respiration rate, heart rate, or electromyogram (EMG), in agreement with the earlier GSR studies and with some, but not all, of the heart rate studies.

Methodological Concerns: Skeletal and Cognitive Mediation

In 1967, I published a summary of most of these findings in the *Psychological Bulletin* (H. D. Kimmel, 1967). By this time there had been 16 published studies of instrumental conditioning of the GSR, 5 of heart rate, and 3 of the vasomotor response, not including the 1938 studies of Mowrer and of Skinner and Delabarre. Evidence showing instrumental autonomic conditioning in humans was reported in all of these 24 experiments. By this time it was clear that the evidence pointed "unequivocally" if not overwhelmingly to the conclusion that autonomically mediated responses of humans *are* conditionable instrumentally, a conclusion I came to in my *Bulletin* article. But this conclusion did not necessarily mean that the mechanism of instrumental autonomic conditioning was understood or that it was the same as that underlying instrumental modification of skeletal responses, even assuming that the latter mechanism were actually known. The ink was hardly dry on the *Psychological Bulletin* article when Katkin and Murray (1968) argued that instrumental autonomic conditioning in humans could never unequivocally be demonstrated because of our inability to control effectively for skeletal and cognitive mediation. Short of massive curarization, such as now has been done with subhuman subjects (DiCara, 1970), skeletal behavior may always be a possible mediator of changes in autonomic behavior, although the evidence currently available, some of which has already been touched on and some of which will be mentioned below, indicates that "possibility" and "actuality" are not identical.

For example, Rice (1966) found operant GSR conditioning in human subjects who were reinforced for GSRs *only* when they occurred in the absence of EMG activity. Van Twyver and Kimmel (1966) reported significant differences between conditioned and yoked control groups even when all GSRs that were observed to occur close in time to

respiration irregularities or EMG activity were eliminated from their data. Gavalas (1968) reinforced subjects for making deep respirations, which elicited GSRs, and found that the skeletal respiratory response was learned while the GSR habituated. Commenting on the significance of this "decoupling" methodology, she argued, "If observable skeletal mediators of observable autonomic responses do not produce changes that mimic those seen in operant/autonomic conditioning, it becomes highly unreasonable to suppose that unobservable mediators do [Gavalas-Medici, 1972, p. 108]." And, as has been mentioned previously, Birk, Crider, Shapiro, and Tursky (1966) obtained instrumental GSR conditioning in a partially curarized human subject who was capable of only very limited movement. Several of the studies of instrumental conditioning of heart rate, and one of the studies of vasoconstriction, likewise, appear to have provided adequate evidence that EMG and respiration did not change in concomitance with the conditioned changes in the autonomic response. Even more damaging to the skeletal mediation hypothesis are recent results on instrumental salivary conditioning and on cardiac integration in humans. These studies will be discussed in detail in a subsequent section.

The matter of cognitive mediation is somewhat more complicated, in my opinion, since it is related to the perennial controversy regarding whether any human learning can occur without awareness of contingencies. As Rochelle Gavalas-Medici (1972) recently observed, those who argue for cognitive or other mediation often fail to differentiate between sufficiency and necessity. As she pointed out,

> It is necessary—but not sufficient—to show, as Belmaker, Proctor, and Feather (1972) did, that a skeletal mediator (inconspicuous muscle tension) can elicit the autonomic response in question (heart rate). In order to demonstrate the critical role of the mediator, operant reinforcement must be made contingent upon increases in heart rate which are elicited by the increased muscle tension. The careful coupling of the responses and the reinforcer are essential [p. 108].

I would add to this that demonstrations that cognitive strategies may be *sufficient* to elicit autonomic responses do not force the conclusion that such strategies are necessary for changes in autonomic responses to occur. In fact, as I have argued in recent publications (H. D. Kimmel, 1973a, 1973b), consistent eliciting stimuli foster habituation, not maintenance or strengthening of responses, whether they be autonomic or skeletal.

After we had conducted intensive interviews with about 200 subjects in my laboratory, their responses convinced us that we were wasting both our own time and theirs. Ninety-eight percent of the subjects interviewed in our laboratory report no awareness that *anything* they might have done during the conditioning session was correlated with the delivery of the reinforcement. These subjects typically respond with incredulity to our suggestion that something they were doing influenced the reinforcement. This is, of course, reminiscent of the thumb cramps reported by Hefferline's (Hefferline, Kennan, & Harford, 1959) subjects after the extinction phase of his study of instrumental conditioning of EMG activity in the thumb. Hefferline's subjects speculated that the cramps resulted from the tightness of the EMG electrodes. Observations of this type do not *prove* the absence of cognitive mediation, to be sure, but they make the hypothesis more difficult to accept. Similar comments regarding postexperimental interviews have been made by other investigators (e.g., Engel, 1971), who even reported eventually feeling foolish about conducting them.

The data relevant to the possible role played by cognitive factors in instrumental autonomic conditioning in humans are actually quite unsystematic and confusing. For example, Greene and Nielsen (1966) found that subjects with high scores on a paper-and-pencil test of autonomic awareness showed little, if any, evidence of instrumental GSR conditioning, while subjects with low autonomic awareness scores showed significant conditioning. Thysell and Huang (1968) found good GSR avoidance conditioning without augmented, response-correlated feedback, but no avoidance conditioning at all with augmented feedback. Engel and Hansen (1966) reported conditioned heart rate slowing in subjects who did not guess that heart rate was the reinforced response, but the four subjects in their study who guessed correctly that heart rate decreases kept the reinforcing light on were all nonlearners. Also of interest is Engel and Hansen's report of one subject who learned heart rate slowing and indicated that he had kept the light on by thinking of examinations in which he had done poorly and of quarrels he had had with his wife. Similarly, Levene, Engel, and Pearson (1968) found that subjects could learn to increase and decrease heart rate cyclically, in relation to exteroceptive discriminative stimuli, but that one subject who

guessed that heart rate was involved did poorly in acquiring this learned control.

On the other hand, Brener, Kleinman, and Goesling (1969) found that the greater the amount of feedback given to the subject, the better the instrumental heart rate conditioning obtained. Stern (1967), who found little or no evidence of an instrumental conditioning effect in his GSR data, nevertheless reported much evidence of cognitive and cognitive-skeletal strategies. Furthermore, Katkin and Murray (1968) argued that the Engel and Hansen (1966) and Engel and Chism (1967) studies of heart rate slowing and speeding showed merely that voluntary relaxation was the primary technique differentiating between decreases and increases in heart rate, regardless of reinforcement contingencies. Their point illustrates the imprecision that may tend to obfuscate the issue of mediation in instrumental autonomic conditioning. If the subject thinks of some fearful event which elicits an autonomic reaction, and if this is followed closely in time by reinforcement, the tendency to think of fearful events may be strengthened or maintained, assuming that the subject is actually motivated to gain reinforcement. Effective conjuring up of an emotion-producing thought obviously may elicit a variety of sympathetic reactions, such as vasoconstriction, GSR, heart rate and blood pressure increase, etc. This kind of cognitive mediation of an autonomic response is analogous to a person imagining that something hot or electrically charged has touched his hand and, consequently, moving his hand abruptly. If, on the other hand, a person simply thinks of moving his hand and then moves it, cognitive mediation is not ordinarily invoked as an explanatory mechanism. This is because we do not usually think of thinking of skeletal movements before we make them. Yet, even those rare individuals who are able to wiggle their ears must concentrate intensely to demonstrate the response. If a person thinks of relaxation and, consequently, receives reinforcement for heart rate slowing, this kind of "cognitive mediation" may be no different from what is involved in concentrating on controlling a slight hand tremor. It is not at all the same as imagining that one is lying in bed almost asleep or floating calmly in a cool swimming pool. Nor is thinking of generalized "excitement" the same as thinking of almost being struck by a car, although one's heart rate may increase in both cases. Cognitive mediation should be reserved for those instances of apparent instrumental autonomic conditioning in which the subject reports thinking of a situation or event which could conceivably elicit the autonomic response, and in which the subject also reports persistence or even increases in the occurrence of such a cognition. When nothing more is meant by cognitive mediation than what is necessary to perform an unusual or rare skeletal response, such as wiggling the ears, nothing is gained by its invocation (unless, of course, the cognition involved in ear wiggling is imagining that one is a bird or a rabbit). Making a GSR, a vasoconstriction, or dilation, slowing or speeding the heart, or increasing or reducing blood pressure or salivation, all may be assumed to be at least as difficult as moving one's ears, and cognizing in the form of scanning for some kind of proprioception is surely to be expected.

As the foregoing discussion clearly shows, the temptation to engage in polemics is almost irresistible. What data there are do not settle the basic argument because data inevitably are subject to differing interpretations. There have now been about 95 published experiments on instrumental autonomic conditioning in humans, with over 90% reporting positive results. Most of the more recent of these studies have included both control and measurement procedures to gain information regarding skeletal, cognitive, or other mediators. One of these recent studies (Lang & Melamed, 1969) involved the successful use of electric shock punishment in eliminating ruminative vomiting *in a nine-month-old infant*. The punishment was triggered by an EMG transducer which picked up indications of reverse peristalsis from the esophagus. There are now three published experiments showing instrumental conditioning of salivation in humans (Brown & Katz, 1967; Delse & Feather, 1968; Frezza & Holland, 1971). In the Frezza and Holland (1971) study, both increases and decreases in salivation were learned. There was some tendency for increases in salivation to be correlated with a tendency toward increased swallowing movements, but it was more likely that the salivation elicited the swallowing than the reverse. More significantly, there were no observable movements correlated with learned decreases in salivation. It is important to point out that humans do not succeed in influencing salivation by deliberately imagining eating succulent foods or lemons, or performing other acts (Feather & Wells, 1966). Perhaps even more cogent to the mediation issue are the data recently reported by Schwartz, Shapiro, and Tursky

(1971) on reward training of heart rate and blood pressure changes. I would like to quote one sentence from their discussion because it presents their conclusions very concisely and, also, because it identifies the problem faced by supporters of a mediation hypothesis, "Man can learn to increase both his blood pressure and his heart rate, lower both his blood pressure and his heart rate, or raise and lower his blood pressure *or* his heart rate [p. 62, italics added]." Furthermore, Schwartz (1971) has now also shown that people can learn to raise their blood pressure while lowering their heart rate *at the same time*, and, although less unequivocally, to lower their blood pressure and raise their heart rate at the same time. It seems to me that mediationists may also have to become cognitive contortionists to deal with data such as these.

I have refrained from presenting any of the evidence obtained in studies of curarized and noncurarized subhumans, although their results are obviously relevant to the question of skeletal mediation in instrumental autonomic conditioning. There are two reasons for this choice. The first is simply that this presentation is focused on instrumental autonomic conditioning in human beings and the addition of the animal work would have required time more appropriately directed to consideration of uniquely human issues (e.g., cognitive mediation, applications to clinical psychology). Second, it turns out that even dramatic effects of instrumental conditioning in deeply curarized rats may be laid to theoretical "central" mediators (Black, 1970). For some, the very idea of instrumentalization of autonomic responses is unacceptable a priori. Since I am suggesting that something resembling stubborn unwillingness may preclude acceptance of the possibility that the law of effect may embrace autonomic as well as skeletal behavior, let me remind you that methodological objections of a somewhat similar type delayed acceptance of the significance of the law of effect for skeletal behavior as well (cf. Osgood, 1953, pp. 380–382). I am alluding to what was once known as the *retroflexive paradox*. I suspect that an unrecognized dualism underlies this strong resistance to accepting the instrumental modifiability of autonomic behavior. The previously quoted remark of Skinner, in which he concluded his argument against the possibility of operant autonomic conditioning by noting that, "If some technique could be worked out to achieve this result, it would be possible to train a child to control his emotions as readily as he controls the positions of his hands [Skinner, 1953, p. 114]," illustrates my point. Skinner's statement is sensible only under the assumption of *voluntary* control of skeletal behavior, with the capriciousness and free will such an assumption implies. I believe that it is because of this unrecognized free will attributed to skeletal behavior that involuntary, autonomic behavior must be restricted to modifiability only by classical conditioning, that is, mechanically. Exclusion of autonomic responses from the elite domain of the instrumentally conditionable has the function of protecting the unrecognized dualism. Perhaps the influence of Skinner's (1971) *Beyond Freedom and Dignity* will ultimately sweep the ghost of this reverse dualism from autonomic behavior's closet. Surely, if we succeed in purging what Skinner calls "autonomous man" as an explanatory concept in dealing with skeletal behavior, we may also succeed in banishing him, in the guise of an unobservable central mediator, in dealing with the more mechanical autonomic responses.

Basic and Applied Implications

1967–1974

The realization that human autonomically mediated responses may be conditionable instrumentally has obvious implications both for basic conditioning theory and for applied problems in clinical psychology, especially in connection with the treatment of psychosomatic disorders. I will comment only briefly on what I think the basic theoretical implications are and what they are not, so that somewhat fuller treatment of psychosomatic therapy will be possible. A more complete discussion of the basic significance of the instrumental conditionability of autonomic responses would probably be premature at this time anyway, because there are still so many methodological issues to be settled and factual blanks to be filled.

Now that we have good reason to believe that autonomically mediated responses may be conditioned instrumentally, we should not assume therefore that all conditioning is instrumental conditioning, as some have suggested (e.g., DiCara, 1970). In my judgment, that would be at least as great a mistake as assuming that all conditioning is classical conditioning, that is, that the law of effect is trivial. Instrumental conditioning of autonomic responses, to the extent that we now have observed it, is not identical with instrumental conditioning of skeletal responses, to the extent that we under-

stand it. If we learn anything from the discovery that certain autonomic responses yield to instrumental reinforcement, it should be that a more critical approach to our scientific categorizations must be taken. Some responses appear to be more readily conditionable by the method we have arbitrarily devised and called classical conditioning—more readily than they are conditionable by another method arbitrarily invented and named by us. Some responses appear to be more readily conditioned by one or the other of these arbitrarily elaborated methods than are other responses. It is easier to condition a dog to flex a limb by reward training than by classical pairing of a conditioned stimulus and shock, just as it is easier to condition a dog to salivate to a tone by classical pairing of the tone with food than it is to train the animal to salivate to avoid shock instrumentally. These comparisons may be made just as meaningfully *within* either the skeletal domain or the autonomic as *between* the two. That is, it is easier to condition the human vasomotor reflex by reward training than it is to condition the GSR similarly. And it is easier to condition the GSR classically, with shock as the unconditioned stimulus, than it is to condition salivation classically with food as the unconditioned stimulus, in human subjects. Furthermore, it is easier to condition unelicited human GSRs instrumentally than to condition elicited GSRs instrumentally, even using the same reinforcement. And it is easier to condition some autonomic responses with augmented feedback than without, and the reverse is true for others.

We have focused mainly on a single difference between our two traditional conditioning methods, namely, whether the reinforcement is contingent on a response or a stimulus. There are other differences of potential significance which we have largely ignored, and there are differences within each of our arbitrary types which we have ignored as well. What is there about an autonomically mediated response that makes it easier to condition classically than instrumentally, or easier to condition when unelicited than elicited? Having reduced the arbitrary classical–instrumental distinction to a one-bit dichotomy, we are now confronted with the necessity of a complete overhaul, if we expect ever to put the pieces back together again. To me, that is the theoretical significance of instrumental autonomic conditioning.

John Watson said that we may gain or lose our happiness because of behavior our guts lead us into. Now that we know that our gut reactions are modifiable instrumentally themselves, it may be possible to redefine happiness and unhappiness directly in terms of learned autonomic patterns of reaction. I have recently suggested that the unhappy state known as chronic anxiety may be better represented in instrumental conditioning terms than by the more conventional classical conditioning model (H. D. Kimmel, 1974). Ever since Mowrer (1939) made his seminal translation of Freud's concept of anxiety into S–R language, the tendency to equate it with classically conditioned fear has pervaded our conception of it. Yet, it is obvious that the conditioned fear model is inadequately fitted to clinical anxiety, except perhaps when the anxiety is associated with phobic stimuli. Classical fear reactions are known to extinguish very rapidly when the unconditioned stimulus is removed, but anxiety is persistent. Classical fear reactions are very brief in duration, but anxiety states go on for hours, or days, without abatement. Perhaps even more importantly, classically conditioned fear is a reaction to a stimulus, while clinical anxiety is more often not stimulus bound but tends to be free floating and vague in its reference.

Greene and Sutor (1971) described a study of instrumental GSR conditioning which seems better suited to serve as a prototype of the etiology and maintenance of anxiety, in the light of these deficiencies in the classical fear model. Their subjects listened to music during the conditioning session, but unless unelicited GSRs occurred at least as often as once per 30 seconds, the music would be interrupted by an aversive pattern of one-half second of silence and one-half second of music. The GSR delayed the aversive interruptions for 30 seconds, on a Sidmanlike avoidance schedule. These conditions obtained only while a visual discriminative stimulus, three horizontal dots, were present. When the visual stimulus was changed to three vertical dots, four different control conditions were administered to four subgroups of different subjects. One subgroup simply received uninterrupted music during the control condition, equivalent to the removal of the aversive stimulus in Sidman extinction. A second group received only interrupted music, with the possibility of terminating it or delaying it removed. A third group received a mixture of interrupted and uninterrupted music, resembling the avoidance condition but not response-contingent. And the fourth subgroup re-

ceived response-contingent punishment during the control condition. All but one of the subjects responded more frequently during the avoidance than the control condition. Furthermore, this difference between avoidance and control responding increased from the first five conditioning sessions to the last five in all but one subject. Most significantly, the subjects who had received punishment for unelicited GSRs during the control condition not only showed the greatest difference between responding in avoidance and control periods, but also made the greatest increase in this differentiation from the first to the last five sessions. This increase did not result from a reduction in response frequency during punishment, but to a very large *increase* in response frequency during the avoidance condition. In other words, the subjects who showed the highest frequency of Sidman avoidance responding were the ones who had an avoidance contingency intermixed with response-contingent punishment. This tendency persisted and grew in strength during the 10-day period of this experiment. Now, a chronically high rate of unelicited GSRs is not tantamount to chronic anxiety, but to the extent that it is a microcosmic analogue of it, Greene and Sutor's study suggests a great deal about how such states may come into existence and be maintained.

Quite a range of psychosomatic ailments have come under instrumental autonomic conditioning's scrutiny in the past few years. As might be expected, most attention has been given to cardiac malfunction. Weiss and Engel (1971) employed instrumental conditioning of heart rate in an effort to control the occurrence of premature ventricular contractions in cardiac patients. All of the eight patients they studied showed some degree of heart rate control, but only five also showed decreases in the frequency of premature contractions. Four of the improved patients maintained a reduced frequency of premature ventricular contractions during several months of follow-up, the longest being 21 months at the time of publication of their report. Engel and Melmon (1968) conditioned more regular cardiac rhythms in patients with several different kinds of cardiac arrhythmias, and DiCara (1970) described a case of successful treatment of a patient with chronic tachycardia by means of instrumental heart rate conditioning. In studies of cardiac malfunction which were less therapeutic than analytical in purpose, Bleecker and Engel (1973) trained six patients with chronic atrial fibrillation and rheumatic heart disease to slow and speed their ventricular rates differentially. All of the six patients learned this control, even though they were all under digitalis medication at the time of study. Engel (1971) also reported a study of three patients with complete heart block, a condition in which the ventricle behaves as if it is functionally disconnected from the atrium and the atrial-ventricular node. None of these patients were able to learn to speed their ventricles, indicating that, while the intact heart and the innervated atrial-ventricular node ventricle preparation can learn heart rate control, the innervated ventricle alone cannot. One unexpected spinoff from this research on heart function using instrumental conditioning methods was the possible use of this method to unmask latent digitalis toxicity.

Benson, Shapiro, Tursky, and Schwartz (1971) used an operant-conditioning-feedback technique to train seven patients with essential hypertension to lower systolic blood pressure. Six of the seven were able to learn some degree of reduction in systolic pressure within the range of 8 to 34 sessions employed, with the average reduction for the entire group of seven patients being 16.5, which was statistically significant. Beyond this, five of the patients showed clinically meaningful decreases in systolic blood pressure, with one dropping from 213 to 180 and another from 162 to 133. Since these changes were observed only in the laboratory, the therapeutic value of the method remains to be determined, although its promise is considerable. Schwartz (1973) recently identified several of the more significant clinical issues and problems associated with the use of biofeedback and instrumental autonomic conditioning as therapy.

At the close of my article in the *Psychological Bulletin* (H. D. Kimmel, 1967) I expressed the hope that the next few years would provide the theoretical structure required by the growing body of results. As I have tried to show in this presentation, a great deal of additional data has been collected, in both the basic research and applied areas, but only the most preliminary theoretical conjectures have emerged. My own view is that the traditional categories of conditioning must be reexamined and new categorizations developed and evaluated. Perhaps another few years may be all we need; perhaps we will need more than a few years. In either case, the rapidly growing interest and experimentation in the field of instrumental autonomic conditioning, from the not-so-distant past, when it was "not so much imaginary as im-

possible," to the present, when biofeedback machines for home use are being recommended, must go down as a most remarkable scientific about-face.

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Aggression in Childhood

Developmental Perspectives

WILLARD W. HARTUP *Institute of Child Development*
University of Minnesota [1]

Although the literature dealing with aggressive behavior in children is extensive (Feshbach, 1970), little actually concerns ontogenetic issues, and very little derives from developmental theory. Among the more significant lacunae in research on aggression development are the following:

1. *For no species do we know from which early motor patterns the components of later aggression are shaped.* From time to time there have been suggestions that the rage reaction (tantrums) may be the wellspring of early aggressive behavior (Hamburg & Van Lawick-Goodall, in press; Munroe, 1955). Tantrums occur early in ontogenesis, they have great capacity for eliciting reaction from the environment (they are noxious), and both the occurrence of tantrums and their behavioral components are sensitive to feedback contingencies (Etzel & Gewirtz, 1967; Goodenough, 1931). Because tantrums produce both positive and negative feedback (parents capitulate to tantrum behavior as well as punish it), these behaviors probably serve as opportunities for the young child to learn the efficacy of aggressive action as well as the efficacy of aggressive inhibition. As yet, however, the contributions of such patterns to aggressive ontogenesis are unknown.

2. *The contexts which serve as origins for aggressive development have not been specified for most species.* Both field research and laboratory studies (Harlow & Harlow, 1965; Jay, 1968) suggest that the context which accounts for most of the variance in the development of nonhuman primate aggression is rough-and-tumble contact with peers. Conditions allowing for rough play to escalate into aggression and, in turn, to deescalate into playful interaction appear to facilitate the acquisition of two repertoires: (*a*) an armamentarium of aggressive behaviors and (*b*) mechanisms for coping with the affect and other outcomes of aggressive interaction. Although Patterson and Cobb (1971) have suggested that peer interaction is also the primary context for aggression development in human children, we can only conjecture that rough-and-tumble play during the preschool years serves the same functions for *homo sapiens* as it appears to serve for the common langur and the other nonhuman primates.

3. *Age changes in the aggressive activity of human children have been documented in an astonishingly small number of studies* (Feshbach, 1970).

(*a*) *The form of the behavior.* About all that can be said about age changes in the morphology of human aggression is that, from two through six years of age, there is an increase in the utilization of language in aggressive outbursts. Verbal aggression remains the preferred mode of attacking other humans among elementary-school-aged girls, but the evidence is inconsistent about changes in types of physical attack after the age of six. Again and again, social psychologists come back to Florence Goodenough's (1931) data about age differences in children's angry behavior. She found that

> With advancing age, the forms of behavior displayed during anger become more definitely directed toward a given end, while the primitive bodily responses of the infant and young child are gradually replaced by substitute reactions commonly of a somewhat less violent and more symbolic character [p. 69].

Then she added that frequency and duration of "after reactions," including sulking, whining, and

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Requests for reprints should be sent to Willard W. Hartup, Institute of Child Development, University of Minnesota, Minneapolis, Minnesota 55455.

brooding, showed a marked increase after the age of four even though the duration of the violent phases of angry outbursts changed but little during the preschool and the early elementary school years.

Surely it is time to recognize, within a developmental perspective, the functional differences existing among verbal aggressive episodes such as the following: (*a*) "Don't talk to Rachel. She is the most dumb in the world." (*b*) "I'm gonna pull that fucking Moira's hair out." (*c*) "Sucker, get out of here." Similarly, there would appear to be no reason to assume that all acts of physical aggression have common functions. Thus, it is inconceivable that, with increasingly sophisticated social-cognitive functioning, qualitative age changes would not be found in children's aggression in addition to the amount of language used.

(*b*) *The instigators of aggressive behavior.* The earlier studies (Dawe, 1934; Goodenough, 1931) also contain the only differentiated information in the literature concerning age changes in the elicitors of aggression. Goodenough found that, in infancy, angry outbursts are principally keyed to physical discomforts and needs for attention. During the second and third years, however, such episodes are triggered increasingly by "habit training." Social difficulties with peers emerge at about this time as instigators of angry outbursts, and predominate in the years that follow. Dawe's (1934) study of 200 quarrels occurring in a nursery school also tells us about age changes in the events that elicit aggression: (*a*) Although possession-instigated quarrels predominated at every age level from 18 months of age through 65 months, 78% of the youngest children's quarrels were instigated in this manner while only 38% of the oldest children's quarrels began in this way. (*b*) Physical violence increased as an instigator of quarreling from 8% to 27%, and "social adjustment" increased as an instigator of quarreling from 3% to 15% during the period covered. These data hint at two concomitant developmental changes in the functional character of young children's aggression: (*a*) a relative decrease during the preschool period in straightforward instrumental aggression; and (*b*) an increase in person-directed, retaliatory, and hostile outbursts. More about these trends later.

4. *Longitudinal studies of individual differences in aggression are few in number.* The famous studies by Kagan and Moss (1962), Bayley and Schaeffer (1964), and Emmerich (1964) stand nearly alone in elucidating sex-related differences in the stability of aggressive traits. Stabilizing of individual differences in aggressiveness appears earlier in males than in females and seems to be independent of the discontinuities in aggression development mentioned above. Evidence for developmental transformations in the area of aggression is much more tenuous, although the findings reported by Kagan and Moss (1962) are intriguing: Early aggression in girls was predictive of later competitiveness and rejection of feminine sex-typing rather than later aggressiveness. All that needs to be noted here, however, is the general paucity of data on individual differences in aggression development.

Overall, then, data concerning the developmental course of aggression are in short supply. The shortage extends to information about changes in the form of the aggressive acts themselves, the contexts in which aggression originates, the circumstances that trigger aggressive episodes, and stability in individual propensities to respond aggressively across time and circumstance.

A New Study

Three developmental hypotheses concerning children's aggression formed the basis of a new study completed at the University of Minnesota: (1) There is a greater proportion of hostile, "person-directed" aggression relative to "object-oriented," instrumental aggression in the interactions of grade school children than in the interactions of preschool children. (2) Threats to self-esteem lead more frequently to hostile attempts to injure the agent of frustration than to object-oriented aggression, particularly for older children. (3) Blocking is associated primarily with aggression which has instrumental value in gaining or preserving objects, territory, or privileges and in which injury to the other person appears to be a secondary goal. This latter relation, however, should be more consistent for younger than for older subjects.

The conceptual ancestry of these hypotheses should be apparent to anyone familiar with the literature on aggression—it lies in the distinction originating with Feshbach (1964), Buss (1966), Rule (in press), and others between "hostile" and "instrumental" aggression. Basically, this distinction has been used as a means of refining the frustration–aggression hypothesis. According to this line of theorizing, the prerequisites of hostile ag-

gression (i.e., person-oriented aggression) include (*a*) frustration-produced stimuli which have ego-threatening properties and (*b*) an inference by the subject that the agent of frustration has behaved intentionally. Other attributions may also be involved in eliciting this form of aggression, but the literature particularly emphasizes the linkage between hostile outbursts and frustrations which involve ego threats or threats to one's self-esteem. In contrast, instrumental aggression, (i.e., aggression which is aimed at the retrieval of an object, territory, or privilege) should be linked to simple goal blocking.

The distinction between hostile and instrumental aggression is far from clean. First, as Feshbach (1970) has pointed out, both instrumental and hostile elements are often involved in the same social interchange. Additionally, this distinction suggests that there is no "instrumental" value to be found in hostile aggression. Clearly, the attempt to restore one's self-esteem by making someone else feel bad is instrumental behavior. Thus, the terminology is imprecise, to say the least. Additionally, Rule and Percival (1971) have raised questions as to whether the psychological processes operating during outbursts of instrumental and hostile aggression are really very different. They found, with adult subjects, that both goal blocking and insult raised their subjects' level of aggression, but these manipulations produced perceptions and attitudes toward the agent of frustration which were virtually indistinguishable. Regardless of whether the aggressor was insulted or frustrated, he reported himself to be more annoyed with the frustrating agent, and indulged in derogation and devaluation of the agent of frustration in similar degree.

On such grounds one may question the ultimate usefulness of the instrumental–hostile dichotomy. Nevertheless, these rubrics serve reasonably well for an attempt to examine the functional properties of children's aggression. First, these concepts may be easily integrated with the existing data. Second, known facts about children's social–cognitive development also suggest that hostile aggression should not be as strongly characteristic of the aggressive activity of younger as of older children. Under the age of six, for example, children have limited capacities for role taking and the generation of inferences and attributions about other people (Flavell, 1968). To the extent that hostile aggression is dependent on attributions about the agent of frustration (especially his intentions), this type of aggression should be less evident in younger than in older children. Furthermore, self-esteem, valuations of self-competence, and notions about one's status are relatively rudimentary in young children. Just as the young child's conceptions of morality are heteronomous, his conceptions of the dimensions of self-competence and self-worth are unstable. Since reference to self-esteem is a prerequisite to hostile aggression, a lesser proportion of such aggression for younger than for older children would also be expected on these grounds.

Prediction of developmental changes in instrumental aggression is a bit more difficult. Dawe (1934) reported a declining percentage of such aggression in association with increased age. But, should there be a change in the functional quality of the aggression which is elicited by goal blocking as children grow older? Feshbach (1964), Rule (in press), and others have suggested that goal blocking should elicit object-oriented aggression (rather than person-oriented aggression), whatever the individual's age. The data of Rule and Percival (1971), however, suggest that, among adults, blocking produces attitudes and perceptions which are very similar to those produced by insult and derogation. Such an admixture of instrumental and hostile aggression in response to goal blocking should occur, however, only if the individual possesses those cognitive–inferential skills which are prerequisite to the hostile components of the activity. In other words, there is the strong possibility that children's responses to goal blocking are more purely instrumental during the preschool years than during the elementary school years because of the lesser sociocognitive maturity of the younger children. By seven and eight years of age, we would expect considerable inconsistency in the way children respond to blocking (i.e., hostile activity on some occasions and instrumental activity at other times, as well as activity which is "mixed" with respect to its components).

METHOD

The strategy chosen for studying the functional relations outlined above was naturalistic observation. The functions of children's aggression are extremely difficult to study with experimental techniques, and some aspects of children's aggressive functioning are simply not open to experimental attack (e.g., the manner in which children respond to insult).

The study was centered in six groups of children, all in one children's program in St. Paul, Minnesota. All groups, including both preschool children and first- and second-

graders, operated under a common program philosophy. The six groups were located in three different buildings, with one younger and one older group in each building; all of the subjects came from the lower socioeconomic strata of the city. It is important to note that the staff was supervised through a single administrative organization and that the structure of the groups was similar in the case of both older and younger children. All groups were "open" groups in the sense that the schedule permitted the children to have a range of choice among activities and minimal constraints on peer contact. Program activities were not identical—older children simply do not engage in the same play activities as younger children—but gross ecological differences between settings for older and younger children were minimal. One hundred and two children, 56 boys and 46 girls, were enrolled in these groups. Sixty-four were 4–6 years of age; 38 were 6–7 years of age.

Observations were conducted over a 10-week period, using a combination of time and event sampling procedures. Aggressive events were defined as *intentional physical and verbal responses that are directed toward an object or another person and that have the capacity to damage or injure.* Instructions to the observers were to record as much information about each aggressive act as he or she could reconstruct. A sample of one episode is given below:

> Marian [a seven-year-old] . . . is complaining to all that David [who is also present] had squirted her on the pants she has to wear tonight. She says "I'm gonna do it to him to see how he likes it." She fills a can with water and David runs to the teacher and tells of her threat. The teacher takes the can from Marian. Marian attacks David and pulls his hair very hard. He cries and swings at Marian as the teacher tries to restrain him; then she takes him upstairs. . . . Later, Marian and Elaine go upstairs and into the room where David is seated with a teacher. He throws a book at Marian. The teacher asks Marian to leave. Marian kicks David, then leaves. David cries and screams, "Get out of here, they're just gonna tease me."

This incident included an initial aggressive outburst followed by counteraggression and continued fighting. Other outbursts, however, were less protracted. Observers were encouraged to use clear language that would not telescope the event and to avoid inferences about the particular intentions, motivations, or feelings of the subjects (Wright, 1960).

The specimen records were then rated on three different occasions by three pairs of coders. The first rating identified all interactions which conformed to the definition of aggression, and the coders isolated the words which described the instigating (antecedent) event and the aggressive act. Agreement for this first rating was 83% for aggressive acts and 92% for antecedent events. A second pair of coders rated the nature of each aggressive act as either hostile (person oriented) or instrumental (object oriented). Their overall agreement was 92%. The third set of coders rated the particular function of the antecedent events and aggressive acts. Nine categories ranging from *bodily injury* and *destruction of property to rejection, derogation,* and *defiant noncompliance* were used to classify the aggression. Eighteen categories were used to classify antecedent events, but for this report these have been collapsed into three: (*a*) *blocking* (involving possessions, space, and activity); (*b*) *bodily contact;* and (*c*) *derogations* (negative social comparisons, tattling, ridicule, criticism). To preserve independence in assessing the function of the aggressive act and the function of the antecedent event, the coders used two sets of records with either the antecedent event or the aggressive act blacked out. Thus, if the coder was rating an aggressive act, he had no information about the antecedent event, and vice versa. The functional classification of aggressive episodes and their antecedents was accomplished with an overall percentage of agreement that reached 94%.

RESULTS

For purposes of this presentation, the results will be couched in terms of initial units of aggression, that is, those outbursts which occurred first in an aggressive sequence. Supplemental information will be provided which is based on the entire group of 758 units of aggression observed, even though the interdependence among these units prevents statistical analysis in any depth.

Age. The older children were less aggressive per unit time, overall, than the younger children ($p = .03$). This is the clearest indication in the observational literature that aggression declines in the period immediately after early childhood. The difference was comparable for both sexes. There was a significant Race × Age interaction ($p = .04$)—among younger children, the rate of aggression for black and for white children did not differ, but the older black children were significantly more aggressive than the older white subjects.

The age difference in total aggression derives primarily from an age difference in the rate of occurrence of instrumental aggression. As expected, this type of outburst occurred significantly more frequently among the younger children ($p = .004$). The significant Race × Age interaction also derives from instrumental aggression ($p = .007$). That is, there was no race difference in instrumental aggressive activity among the younger children, but the older black children showed more aggression of this type than did the older white children.

The age difference which bears most directly on the hypotheses of the study concerns the proportion of aggressive units which is hostile. Indeed, for those 84 children who initiated aggressive activity, a significantly higher percentage of the aggression was classified as hostile among the older subjects than among the younger subjects ($p = .003$).

Units of aggression were next separated according to two criteria: those elicited by blocking and those elicited by bodily contact. The percentage of each child's aggression which was instrumental was then computed, along with the percentage which was hostile. Blocking produced a significantly higher percentage of instrumental aggression among the younger children than among the older children

($p = .005$), in line with expectations and supporting the notion that there is a greater admixture of instrumental and hostile reactions to this type of frustration for older children. For aggression produced by bodily contact, there was no significant difference between younger and older children in percentage rated as hostile or instrumental.

Examining the data from another viewpoint (and utilizing all of the units of aggression recorded), a functional pattern emerges which is generally consistent with the preceding pattern of results. A clear age difference was found, for example, in the types of hostile behavior which derogation elicits. For younger children, when such antecedents elicit hostile outbursts, half (48%) take the form of bodily injury (hitting) and half consist of reciprocated derogation, threats, and tattling (52%). Among the older children, however, derogation shows a decided tendency to produce reciprocal derogation: only 22% of hostile responses to derogation involved hitting while 78% involved some type of insult or reciprocated threat to self-esteem. A parallel age difference in types of hostile aggression was not found in the aggression elicited by blocking: about 25% of blocking-produced hostility involved derogation, rejection, tattling, and threats for each age group. Thus, when elementary school children are insulted and the insult leads to aggression, the likelihood of insulting retaliation is very great; not so for preschool children. On the other hand, when either older or younger children are blocked, the proportion of insulting hostile reactions does not vary with age.

Sex. Earlier, it was noted that age was not involved in any interactions with the gender variable insofar as the sheer incidence of aggression was concerned. It will come as no surprise, however, that male subjects were more aggressive totally than the females ($p = .01$). This difference was primarily due to a sex difference in the incidence of hostile aggression ($p = .03$); no significant sex difference was obtained in the rate of occurrence of instrumental aggression. Most important of all, none of the functional analyses, that is, those relating the nature of the aggressive outburst to the nature of the antecedent, have revealed significant sex differences. Thus, there is no evidence that boys and girls are differently "wired" with respect to the operation of hostile and instrumental aggression. Such differences were not expected, and the data confirm those expectations.

Race. A similar situation prevails with respect to race. As noted, more frequent total aggression was observed among older black children than among older whites, owing primarily to the higher incidence of instrumental aggression in the former group. The validity of this finding cannot easily be questioned: There was no reference to race differences in the instructions to observers. Moreover, such differences were in no way related to the purpose of the research; the analyses were conducted only because of their obvious necessity. Indeed, the only way in which these findings could have been produced by observer bias is through more subtle racist stereotypes (the observers were white). But here, too, the more critical race differences would be those relating to the functional properties of the aggression in the interactions of the children. As with sex, no significant differences were found. The patterning of antecedents for hostile aggression and the elicitors of instrumental aggression were not different for our white and black samples.

CONCLUSION

These results lend support to two hypotheses: (1) The developmental course of human aggression may be best understood by means of a differentiated "functional analysis" of the problem. (2) The distinction between instrumental and hostile aggression is heuristically valuable for studying the functions of aggression in early childhood even though it may have more limited usefulness in studies of adolescent or adult aggression.

What is meant by the word *function* in the context of this article? Simply put, the word implies the question, How does it [aggression] work? Answers to this question may be sought from many different vantage points, using many different levels of analysis. One type of analysis might involve the generation of mathematical relational statements between levels of brain stimulation and differentiated aggressive outbursts; other analyses may concern the long-term biological consequences of aggression—that is, whether it serves to maintain an individual's genes in the population. Still other types of functional analysis might concern relations between specific instigating stimuli and the qualities of aggression in particular individuals or groups and how these vary in accordance with the maturation of those response capabilities necessary for a specific type of aggressive display. Each of these functional perspectives is based on the assumption

that understanding how aggression "works" requires knowledge of how the specific aggressive outburst fits into the complicated nexus of events that precede and follow it (Hinde, in press). To this end, the ontogenetic history of the individual and the biological–evolutionary outcomes of the activity must be taken into consideration, as well as stimuli which function as "instigators," "goals," or "reinforcers" in individual behavioral sequences. To specify aggressive functions at only one level of analysis is to risk a myopic conception of the problem like that of the elephant produced by the blind man who only touches the beast's trunk.

Research such as that reported here points the way toward a more complete functional analysis of aggression in children's social interaction. The data, however, are descriptive and do not directly elucidate the relation between social–cognitive processes and age changes in aggression. Do changes in the child's use of attributions about intent and the developmental events mirrored in the observational data emerge in parallel sequence? If so, is there a systematic functional relation between advances in social cognition and changes in aggressive morphology and elicitation? These are the questions to which this work brings us.

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A Survey of Behavior Modification Training Opportunities in APA-Approved Internship Facilities

JAMES H. JOHNSON *Division of Child Psychiatry University of Texas Medical Branch* [1]

PHILIP H. BORNSTEIN *University of Montana*

Data from several sources indicate growing interest in behavioral approaches to treatment within the profession of clinical psychology. Ernst (1971) demonstrated the reality of this behavioral "movement" by citing an increased number of publications in the area. Benassi and Lanson (1972) also reported a rapidly increasing number of colleges and universities offering formal coursework in behavior modification.

Although it appears that opportunities exist for graduate training in behavior modification, detailed information is currently unavailable regarding opportunities for such training at the internship level. The purpose of the present research was therefore twofold: (*a*) to provide prospective interns with information regarding facility-specific training oportunities in behavior modification and (*b*) to ascertain whether internship facilities, in general, can provide training experiences meaningfully related to this aspect of graduate education.

It should be emphasized that the present survey was not intended to be evaluative in any sense. The listing of the presence or absence of opportunities for training in behavior modification in this article should in no way be construed as an evaluation of the overall adequacy of individual training programs. It might also be noted that the reporting of opportunities for training in behavior modification does not provide information regarding the *quality* of training within an individual program.

Method

A questionnaire designed to assess behavior modification training opportunities and interest was sent to the clinical training directors of 110 APA-approved internship training facilities in psychology, as listed in the December 1972 issue of the *American Psychologist*. In addition, a cover letter was provided which briefly explained the purpose of the study. The questionnaire consisted of the following 10 questions:

1. How many psychologists are currently on your training staff?
2. Are there one or more psychologists on your staff whose primary area of interest involves the teaching, research, and/or practice of behavior modification? If yes, how many? Degree status?
3. Does your training program at present offer any didactic training in behavior modification (e.g., a formal course or seminar devoted to behavior modification)? If yes, is this required or optional?
4. Do interns at your facility currently have the opportunity to practice techniques of behavior modification while working directly with some patient population?
5. Do interns at your training facility currently have the opportunity to receive supervision in the practice of behavior modification by someone experienced in this area?
6. How many hours per week does your average intern spend in the following behavior modification activities: Direct patient contact? Supervision in the practice of behavior modification? Didactic training?
7. Please rate on the following scale the degree to which your present interns, as a group, seem to show interest in the area of behavior modification:

LITTLE INTEREST

1 | 2 | 3 | 4 | 5 | 6 | 7

GREAT INTEREST

8. Are your present interns, as a group, generally more or less interested in the practice of behavior modification than those interns of five years ago?

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Requests for reprints should be sent to James H. Johnson, Division of Child Psychiatry, University of Texas Medical Branch, Galveston, Texas 77550.

9. In describing the orientation of your department, would you describe it as: A. Analytic, B. Behavioral, C. Existential, D. Rogerian, or E. Other (Specify)?

10. Any additional comments regarding behavior modification experiences at your facility?

Approximately one month after the mailing of the questionnaire, a follow-up letter, an additional questionnaire, and a stamped, self-addressed envelope were sent to facilities not yet responding to the first questionnaire.

Results

A total of 103 responses were received from the 110 training facilities sampled. Three of the facilities, although responding by letter, did not fill out the questionnaire, indicating that the questionnaire did not lend itself to describing their training program. Thus, the present findings are based on the 100 completed questionnaires which were returned.[2]

Table 1 presents facility-specific information related to training opportunities in behavior modification. Column 1 lists the names of training programs completing the questionnaire. Column 2 reports the number of psychologists on the staffs of training programs. Column 3 indicates the general departmental orientation. Column 4 provides information regarding the presence or absence of a behavioral psychologist on staff. The number of behavioral psychologists is listed in parentheses if more than one. The degree status of those staff members is also indicated. Information in Column 5 indicates whether the program offers formal didactic training in behavior modification and whether this training is required or optional. Columns 6 and 7 provide information regarding facility-specific opportunities for the practice and supervision of behavior modification activities.

Table 2 presents cumulative information related to training opportunities in behavior modification. Column 1 reports the percentage of programs having one or more behavioral psychologists on staff. Column 2 indicates the percentage of programs offering didactic training in behavior modification. The percentages of training programs that reported offering opportunities for the practice and supervision of behavior modification activities are indicated in Columns 3 and 4, respectively.

With regard to degree of intern interest in behavior modification, as perceived by training directors, a mean rating of 3.9 was obtained for the 97 programs responding to the question. In comparing present interns, as a group, to interns of five years ago, 73% indicated more intern interest in behavior modification, 18% indicated the same level of interest, and 9% reported less interest ($N = 92$). Data pertaining to Question 6 are not included in the results due to the difficulty many respondents reportedly experienced in answering the question as stated.

Discussion

The overall results of this survey clearly indicate that opportunities do exist for clinical training in behavior modification at the internship level, but as might be expected, such training opportunities vary considerably from one training facility to another.

A number of comments by training directors give further information concerning the place of behavior modification in internship training programs. These statements range from those indicating enthusiastic acceptance of a behavioral approach to those indicating that behavior modification plays a negligible role in the training of interns. One seemingly widely held view, even among those agencies offering behavior modification training opportunities, appeared to reflect the belief that the psychology intern should be trained as a " 'generalist' not expert in all areas but exposed to all the mainstream of clinical psychology." Statements such as this suggest that although behavior modification training may be viewed as important, it is often perceived as only one aspect of the training necessary for a well-rounded clinician. Other training directors indicated that they viewed behavioral approaches in a somewhat restricted sense, (e.g., "we see behavior modification as a technique, not as a theory of personality"). A number of respondents indicated growing staff interest in the area, suggesting more emphasis was being placed on this approach from year to year.

In summary, it would appear that internship facilities, like graduate departments of psychology, do provide opportunities for subdoctoral training experiences in behavior modification and that intern interest in behavioral approaches, although rated as moderate, is on the upswing. The findings of this study in conjunction with those reported by Benassi and Lanson (1972) suggest that behavior

[2] Three additional completed questionnaires were received after the preparation of the article and construction of Table 1. Data pertaining to these three programs are not included in the results.

TABLE 1

Facility-Specific Information Regarding Behavior Modification Training Opportunities in APA-Approved Internship Programs

Internship training program	No. psychology staff	Orientation	Behavioral psychologist	Didactic training	Opportunity for practice	Opportunity for supervision
Alabama						
University of Alabama School of Medicine	12	Eclectic	Yes (2) PhD	Yes[a]	Yes	Yes
Arkansas						
University of Arkansas Medical Center	10	Eclectic	Yes (1) MA	Yes[a]	Yes	Yes
California						
Atascadero State Hospital	7	Behavioral	Yes (1) PhD	No	Yes	Yes
Camarillo State Hospital	12	Existential	Yes (3) PhD	Yes[b]	Yes	Yes
Kennedy Child Study Center	6	Analytic-Rogerian	No	No	Yes	No
Langley Porter Neuropsychiatric Institute	14	Eclectic	No	Yes[b]	Yes	Yes
Letterman General Hospital	3	Existential	No	Yes[a]	Yes	Yes
Los Angeles County–University of Southern California Medical Center	6	——	Yes (3) PhD	Yes[a]	Yes	Yes
Medical Department Activity (MEDDAC) Psychiatric Service	5	Eclectic	Yes (1) PhD	No	Yes	Yes
Mount Zion Hospital and Medical Center	12	Analytic	No	No	No	No
Napa State Hospital	28	Analytic-Rogerian	Yes (2) PhD	Yes[b]	Yes	Yes
Neuropsychiatric Institute, UCLA Center for Health Sciences	21 full time 6 part time	Analytic	Yes (3) PhD	Yes[b]	Yes	Yes
Patton State Hospital	10	Broad	Yes (2) PhD	No	Yes	Yes
San Fernando Valley Child Guidance Clinic	7	Eclectic	Yes (1) PhD	No	Yes	Yes
Southern California Permanente Medical Group	[illegible]	Eclectic	Yes (2) PhD	Yes[b]	Yes	Yes
University of California, Berkeley, Counseling Center	[illegible]	Eclectic-developmental	No	No	Yes	Yes
Colorado						
University of Colorado Medical Center	16	Multioriented	Yes (2) PhD	Yes[b]	Yes	Yes
Connecticut						
Connecticut Valley Hospital	8	Existential	Yes (1) PhD	No	Yes	Yes
Fairfield Hills Hospital	5.5	Eclectic	Yes (1) PhD	Yes[a]	Yes	Yes
Institute of Living	6	Analytic	No	No	No	No
Norwich Hospital	10	Eclectic-humanistic	Yes (2) MA	No	Yes	Yes
Yale University School of Medicine	12	Analytic-existential-Rogerian	No	No	Yes	Yes
District of Columbia						
D.C. Area C Community Mental Health Center	15	Eclectic	Yes (3) PhD	No	Yes	Yes
Hillcrest Children's Center	4	Analytic	No	Yes	Yes	Yes
National Institute of Mental Health–St. Elizabeths Hospital	20	Analytic	Yes (3) PhD	Yes[b]	Yes	Yes
Walter Reed General Hospital	6	——	No	Yes[a]	Yes	Yes
Florida						
J. Hillis Miller Health Center	22	Eclectic	Yes (2) PhD	No	Yes	Yes

TABLE 1—(*Continued*)

Internship training program	No. psychology staff	Orientation	Behavioral psychologist	Didactic training	Opportunity for practice	Opportunity for supervision
Georgia						
Central State Hospital	8	Behavioral	Yes (4)	No	Yes	Yes
Hawaii						
Hawaii State Hospital	4	Existential	No	No	Yes	Yes
University of Hawaii Counseling and Testing Center	5	Eclectic	No	No	Yes	Yes
Illinois						
Chicago–Read Mental Health Center	70	Eclectic-dynamic	Yes (3) PhD	Yes[a]	Yes	Yes
Children's Memorial Hospital	6	Eclectic	No	No	Yes	Yes
Galesburg State Research Hospital	7	Eclectic	Yes (5) 2 PhD–3 MA	No	Yes	Yes
Illinois State Psychiatric Institute	12	Analytic-existential	No	No	No	No
Northwestern Medical School	10	Eclectic	Yes (5) PhD	Yes[b]	Yes	Yes
Indiana						
Indiana University School of Medicine	13	Eclectic	Yes (1) PhD	Yes[b]	Yes	Yes
Larue D. Carter Memorial Hospital	8	Behavioral	Yes (2) PhD	No	Yes	Yes
Iowa						
Des Moines Child Guidance Center	6	Eclectic	Yes (3) PhD	Yes[a]	Yes	Yes
Kansas						
Topeka State Hospital	8	Analytic	Yes (1) MA	Yes[b]	Yes	Yes
University of Kansas Medical Center	8	Eclectic	Yes (2) PhD–EdD	Yes[b]	Yes	Yes
Wichita Guidance Center	11	Eclectic	Yes (1) PhD	No	Yes	Yes
Louisiana						
Central Louisiana State Hospital	6	Eclectic	Yes (1) PhD	Yes[a]	Yes	Yes
Louisiana State University Medical Center	5	Eclectic	Yes (1)	Yes[b]	Yes	Yes
Maryland						
University of Maryland School of Medicine	7	Eclectic	Yes (1) PhD	No	No	No
Springfield State Hospital	10	Analytic	Yes (2) PhD	Yes[b]	Yes	Yes
Spring Grove State Hospital	6	Eclectic	Yes (1) PhD	Yes[b]	Yes	Yes
U.S. Naval Hospital	4	Eclectic	No	No	Yes	Yes
Massachusetts						
Judge Baker Guidance Center	12	Analytic	No	No	Yes	Yes
Massachusetts Mental Health Center, Harvard Medical School	7	Analytic	No	No	Yes	Yes
Worcester State Hospital	12	—	Yes (2) PhD	Yes[b]	Yes	Yes
Worcester Youth Guidance Center	4	Varied	No	Yes[b]	Yes	Yes
Michigan						
Lafayette Clinic	8	Analytic-behavioral	Yes (2) PhD	Yes[a]	Yes	Yes

TABLE 1—(*Continued*)

Internship training program	No. psychology staff	Orientation	Behavioral psychologist	Didactic training	Opportunity for practice	Opportunity for supervision
Minnesota						
University of Minnesota Health Sciences Center	21	Eclectic	Yes (2) PhD	Yes[c]	Yes	Yes
Mississippi						
University of Mississippi Medical Center	9	Behavioral	Yes (9) PhD	Yes[b]	Yes	Yes
Missouri						
Washington University Child Guidance and Evaluation Clinic	4	Analytic	No	No	Yes	No
Jewish Hospital of St. Louis	4 full time 6 part time	Analytic	No	Yes[a]	Yes	Yes
Malcolm Bliss Mental Health Center	8	Eclectic	Yes (2) PhD	No	Yes	Yes
Western Missouri Mental Health Center	12	Social learning and eclectic	No	No	Yes	Yes
Nebraska						
Nebraska Psychiatric Institute	10	Diverse	Yes (3)	Yes[b]	Yes	Yes
Norfolk Regional Center	3	Behavioral	Yes (2) PhD	No	Yes	Yes
New Jersey						
Child Guidance Center of Mercer County	5	Analytic	No	No	Yes	No
Children's Psychiatric Center	10	Behavioral	Yes (2) PhD	No	Yes	Yes
New York						
Albert Einstein College of Medicine	14	Analytic	No	No	No	No
Astor Home for Children	7	Developmental	No	Yes[b]	Yes	Yes
Children's Village	5	Comprehensive	Yes (1) PhD	Yes[a]	Yes	Yes
Columbia Presbyterian Medical Center	6	Analytic	Yes (1) PhD	Yes[b]	Yes	Yes
Convalescent Hospital for Children	10	Eclectic	Yes (3) PhD	Yes[b]	Yes	Yes
Jewish Board of Guardians	10	Analytic	No	No	No	No
Nassau County Medical Center	6	Analytic	No	No	No	No
New York University–Bellevue Medical Center	11	Analytic	Yes (2) PhD	Yes[a]	Yes	Yes
New York University Medical Center Department of Rehabilitation Medicine	6	Eclectic	No	No	Yes	No
St. Vincent's Hospital and Medical Center	5	Analytic	No	No	Yes	Yes
State University of New York, Upstate Medical Center	7	Analytic	Yes (2)	No	Yes	Yes
University of Rochester School of of Medicine	15	Analytic-eclectic	Yes (2) PhD	Yes[b]	Yes	Yes
North Carolina						
Duke University Medical Center	20	Interpersonal	Yes (2) PhD	Yes[b]	Yes	Yes
North Carolina Memorial Hospital	29	Analytic	Yes (6) PhD	Yes[b]	Yes	Yes
Ohio						
University Hospital and Case Western Reserve University	13	Analytic	Yes (1) PhD	Yes[b]	Yes	Yes
Oklahoma						
University of Oklahoma Medical Center	25	Dynamic	Yes (3) PhD	No	Yes	Yes

TABLE 1—(*Continued*)

Internship training program	No. psychology staff	Orientation	Behavioral psychologist	Didactic training	Opportunity for practice	Opportunity for supervision
Oregon						
Community Child Guidance Center	3	Eclectic	Yes (3) PhD	No	Yes	Yes
University of Oregon Medical School	10	Behavioral-eclectic	Yes (4) PhD	No	Yes	Yes
Pennsylvania						
Devereux Foundation	14	Analytic	No	No	Yes	No
Eastern Pennsylvania Psychiatric Institute (Child)	4	Analytic	No	Yes[b]	Yes	No
Irving Schwartz Institute for Children and Youth	5	Analytic-Rogerian-behavioral	No	No	Yes	Yes
Norristown State Hospital	11	Eclectic	Yes (2) PhD	Yes[a]	Yes	Yes
Pittsburgh Child Guidance Center	9	Eclectic-developmental-ego-analytic	No	No	Yes	Yes
Rhode Island						
E. Pendleton Bradley Hospital	6	Eclectic	No	No	Yes	Yes
Tennessee						
Dede Wallace Center	13	Mixed	No	No	Yes	Yes
University of Tennessee College of Medicine	13	Existential	No	Yes[b]	Yes	Yes
Vanderbilt–Peabody Program	9	Eclectic	No	No	Yes	Yes
Texas						
Baylor University College of Medicine	6	Analytic	Yes (1) PhD	Yes	Yes	Yes
Texas Research Institute of Mental Sciences	6	Existential	Yes (1)	No	Yes	Yes
University of Texas Counseling Center at Austin	14	Eclectic	Yes (3) PhD	Yes[b]	Yes	Yes
University of Texas Medical Branch–Galveston	23	Eclectic	Yes (1) MS	Yes[b]	Yes	Yes
University of Texas Southwest Medical School	16	Eclectic	Yes (2) PhD	No	Yes	Yes
University of Texas Medical School–San Antonio	11	Dynamic	Yes (2) PhD	No	Yes	Yes
Wilford Hall US Air Force Medical Center	8	Behavioral, group-dynamic	Yes (3) PhD	Yes[a]	Yes	Yes
Washington						
University of Washington School of Medicine	—	Behavioral	Yes (8) PhD	Yes[b]	Yes	Yes
West Virginia						
West Virginia University Medical Center	3	Eclectic	Yes (1) PhD	No	Yes	Yes
Wisconsin						
Mendota State Hospital	12	Eclectic	Yes (6) PhD	No	Yes	Yes
University of Wisconsin Medical School	7	Eclectic	Yes (1) PhD	Yes[a]	Yes	Yes

[a] Required.
[b] Optional.
[c] One is required; two are optional.

TABLE 2

Information Related to Training Opportunities in Behavior Modification

Behavioral psychologist	Didactic training	Opportunity for practice	Opportunity for supervision
65%	49%	93%	87%

modification is viewed as an integral part of clinical psychology training, both within the university and within the more "applied clinical orientation" of many APA-approved internship training centers.

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Ernst, F. A. Behavior therapy and training in clinical psychology: A student's perspective. *Journal of Behavior Therapy and Experimental Psychiatry*, 1971, 2, 75–79.

Supplement to Listing of APA-Approved Doctoral and Internship Training Programs

The Committee on Accreditation announces the following changes in the list of APA-Approved Doctoral Programs in Professional (Clinical, Counseling, and School) Psychology and in the list of APA-Approved Internship Programs in Professional Psychology. These changes update the listings published in the September 1973 *American Psychologist* (pp. 844–848). Complete lists are available on request from the APA Educational Affairs Office.

FULLY APPROVED DOCTORAL TRAINING PROGRAMS

Fuller Theological Seminary (Clinical)
Georgia State University (Clinical)
Hofstra University (School)
University of Mississippi (Clinical)
University of New Mexico (Clinical)
University of Vermont (Clinical)

The approval of the following doctoral training program was terminated:

Arizona State University (Clinical)

FULLY APPROVED INTERNSHIP TRAINING PROGRAMS

American University Counseling Center
Area A Community Mental Health Center
Colorado State University Counseling Center
Cornell University Medical College & New York Hospital–Cornell Medical Center

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR REGISTRATION ONLY. THE REVERSE SIDE IS FOR HOTEL RESERVATIONS.

General Convention Information — Deadline: August 1, 1974

1. Please indicate those APA divisions to which you belong (See below for list of divisions) None
   Check your most senior status regardless of whether you belong to a division
   ..... Fellow ..... Member ..... Associate
2. Please indicate the number of persons who are accompanying you to this Convention but who are not completing a registration form and are not accounted for on another registration form Adults Children
   How many of them are planning to attend any meetings? .....
3. Please provide the address to which the convention badge is to be mailed. Be sure to fill in this space even if it is the same as the information given elsewhere on this form.

   Street Address: .....

   City, State, Zip Code: .....

Registration Fee Status (check one):

$30: ☐ Nonmember

$20: ☐ APA Member, Fellow, Associate ☐ Foreign Affiliate ☐ High School Teacher Affiliate

Exempt: ☐ APA Dues-Exempt Member ☐ Student ☐ Nonmember Invited Participant ☐ Nonmember Spouse Dependent of Registrant

Badge Information (Please Print):

Institution/Company Affiliation: .....

City and State.

PLEASE PRINT (CLEARLY) NAME BELOW FOR VISIBLE LOCATOR

Title	Last Name	First Name	Initial

This card and these fees are only applicable for advance registration prior to August 1, 1974. After that date advance registration cards received in the APA Central Office will be returned and those who [illegible] will have to do so on site in New Orleans. If possible use this card rather than photocopied forms. Use of this card for additional persons who wish to register. Additional cards can be obtained in any issue of the *American Psychologist* from April through July.

PLEASE COMPLETE BOTH SIDES OF THIS FORM.

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting but advance registration represents a saving to the registrant. At its January 18-20, 1974 meeting, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For Those Registering in Advance Prior to August 1, 1974:

$30 Nonmember — $20 APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For Those Registering On-Site in New Orleans:

$35 Nonmember — $25 APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Send completed form and check for registration fee to:

American Psychological Association
1200 17th Street, N.W., Room 601
Washington, D. C. 20036

Make checks payable to APA.

List of divisions for Question 1 on registration form:

1 - General, 2 - Teaching, 3 - Experimental, 5 - Evaluation and Measurement, 6 - Physiological and Comparative, 7 - Developmental, 8 - Social and Personality, 9 - SPSSI, 10 - Psychology and the Arts, 12 - Clinical, 13 - Consulting, 14 - Industrial, 15 - Educational, 16 - School, 17 - Counseling, 18 - Public Service, 19 - Military, 20 - Adult Development and Aging, 21 - Engineering, 22 - Rehabilitation, 23 - Consumer, 24 - Philosophical, 25 - Experimental Analysis of Behavior, 26 - History of Psychology, 27 - Community, 28 - Psychopharmacology, 29 - Psychotherapy, 30 - Hypnosis, 31 - State Psychological Association Affairs, 32 - Humanistic, 33 - Mental Retardation, 34 - Population, 35 - Psychology of Women.

Please fill out this form in COMPLETE detail, particularly ARRIVAL and DEPARTURE dates, and type of ACCOMMODATIONS.

SEND APPLICATION AS SOON AS POSSIBLE to address shown at bottom of page to insure desired accommodations at convention rates. DEADLINE August 1, 1974.

☐ Check here if NO accommodations are requested.

Choice of Hotel

First

Second:

Third:

Arrival at a.m. p.m.

Departure at a.m. p.m.

Type of Accommodation Requested

Note: Single rooms are at a premium. Please share a twin bedroom wherever possible.

... Single Room @ $.....

Double Bedroom @ $.....

Twin Bedroom @ $.....

... Parlor Suite @ $.....

☐ Check here if wheelchair-accessible room is required.

Will be sharing a room with

(If any of the above will be attending the convention, they must register also unless otherwise exempt.)

Mail confirmation to: Name.....

Address.....

City..... State..... Zip Code.....

Telephone Number.....

Mail this form and check for registration fee to:

AMERICAN PSYCHOLOGICAL ASSOCIATION
1200 Seventeenth Street, N.W., Room 601
Washington, D.C. 20036

No telephone reservations will be accepted. Advance Registration and Hotel Reservation forms will be returned if not accompanied by the check for registration fee where required.

PLEASE COMPLETE BOTH SIDES OF THIS FORM

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR HOTEL RESERVATIONS ONLY. THE REVERSE SIDE IS FOR ADVANCE REGISTRATION.

HOTEL AND RATE INFORMATION

HEADQUARTERS HOTELS

Hotel	Single	Double	Twin	3rd Person
Fairmont	$26	$32	$32	$8
International*	26	32	32	8
Marriott	26	32	32	3
Monteleone*	23	32	32	4
Royal Orleans	26	34	34	6

OTHER HOTELS

Bienville House	24 50	25 50	28	
Bourbon Orleans	24-32	28-36	36-45	4
Braniff Place	18.50-20	22 50-24.50	24.50-26.50	3
Chateau	17-22	20-24	21-26	
Dauphine Orleans	22 75	27 75	32.75	
de la Poste	26	30	32-35	
Delta Towers	18	24	24	4
Downtowner	22	30	30	
Downtown Howard Johnson	22	27	27	
Governor House	16-17	20-22	25 27	3
La Salle	13	15 50	17	2
Le Pavillon	26	32	32	5
Le Richelieu	22	26	28	
Place D'Armes	18	20 22	22-26	
Prince Conti	18	24-34	24 34	
Provincial	18-21	22-25	23 50-25	
Royal Sonesta	26	32	32	8
Saint Louis	—	—	32	
Tamanaca	15.75-17.75	17.75-19.75	21.75-23.75	
Thunderbird	16-18	18-20	19-22	
Vieux Carre	20.50-21.50	24.50-29.50	26.50-30.50	

*Wheelchair-accessible rooms available. See text for details.

Rates for suites are regular rates. Rates vary with each hotel and range from $45 to $250.

Persons requesting family plan accommodations should indicate their requirements on a separate sheet of paper and attach it to the housing form. The Housing Bureau will fulfill these requirements if such accommodations are available.

Divisions are assigned to meeting facilities as follows:

Fairmont: 1, 2, 5, 10, 15, 16, 27, 30

Marriott: 3, 6, 7, 14, 17, 20, 23, 25, 28

Monteleone: 24, 26

Royal Orleans: 19, 21

Rivergate Exhibition Center/International Hotel: 8, 9, 12, 13, 18, 22, 29, 31, 32, 33, 34, 35

Rivergate Exhibition Center: Placement, Exhibits, Convention Locator System

Eighty-Second Annual Convention of the American Psychological Association

Convention Information

THEODORE G. DRISCOLL, JR. *Convention Manager*
CARL N. ZIMET *Chair, Board of Convention Affairs*

This is New Orleans! Air conditioning . . . Al Hirt . . . Andrew Jackson . . . antebellum plantations . . . antiques . . . Antoine's . . . Arnaud's . . . Audubon Park . . . bananas Foster . . . Basin Street . . . Battle of New Orleans . . . bayous . . . Bourbon Street . . . breakfast at Brennan's . . . Café du Monde . . . café au lait and beignets . . . Canal Street . . . cemeteries . . . chicory coffee . . . Commander's Palace . . . Court of the Two Sisters . . . courtyards . . . Creole cuisine . . . dixieland . . . female impersonators . . . five-cent telephone calls . . . French Market . . . French Quarter or "Vieux Carre" . . . funeral marches . . . Galatoire's . . . Garden District . . . gumbo . . . hot and humid . . . Jackson Square . . . jazz . . . Jean Lafitte . . . Jim Garrison . . . lace balconies . . . "ladies" of the night . . . Lake Pontchartrain . . . levees . . . Longue Vue Gardens . . . the Longs of Louisiana . . . Mardi Gras . . . mausoleums . . . Old Absinthe House . . . oysters Rockefeller . . . Pat O'Brien's . . . pecan pralines . . . Pete Fountain . . . prawns . . . Preservation Hall . . . Ramos gin fizz . . . river boats . . . St. Charles streetcar . . . streetcar named Desire . . . Sugar Bowl . . . Superdome, finished or unfinished . . . topless dancers . . . voo-doo.

And that is where the 1974 Convention of the American Psychological Association will be held. The dates of the Convention are **August 30** through **September 3.** While August is not the ideal time to be in New Orleans, the attractions of the city should compensate for the heat and humidity. Meetings are scheduled in the **Fairmont Hotel**, the **International Hotel**, the **Marriott Hotel**, the **Monteleone Hotel**, the **Royal Orleans Hotel**, and the **Rivergate Exhibition Center.** The bulk of the Convention program is scheduled between 9:00 a.m. and 6:00 p.m. during the five days of the meeting. Because hotels are fairly close to one another, APA is not planning to run a shuttle bus among the hotels. Public transportation buses run up and down Canal Street every few minutes at a cost of 15 cents per person.

Canal Street, New Orleans' main thoroughfare, separates the French Quarter from the business district and uptown area of the city. There are historical sights, shops, nightclubs, and restaurants within the French Quarter. In order to sample the cuisine in local restaurants it would be wise to make reservations for all meals. A handy reference on inexpensive and good restaurants is *The New Orleans Underground Gourmet* by Richard H. Collin.

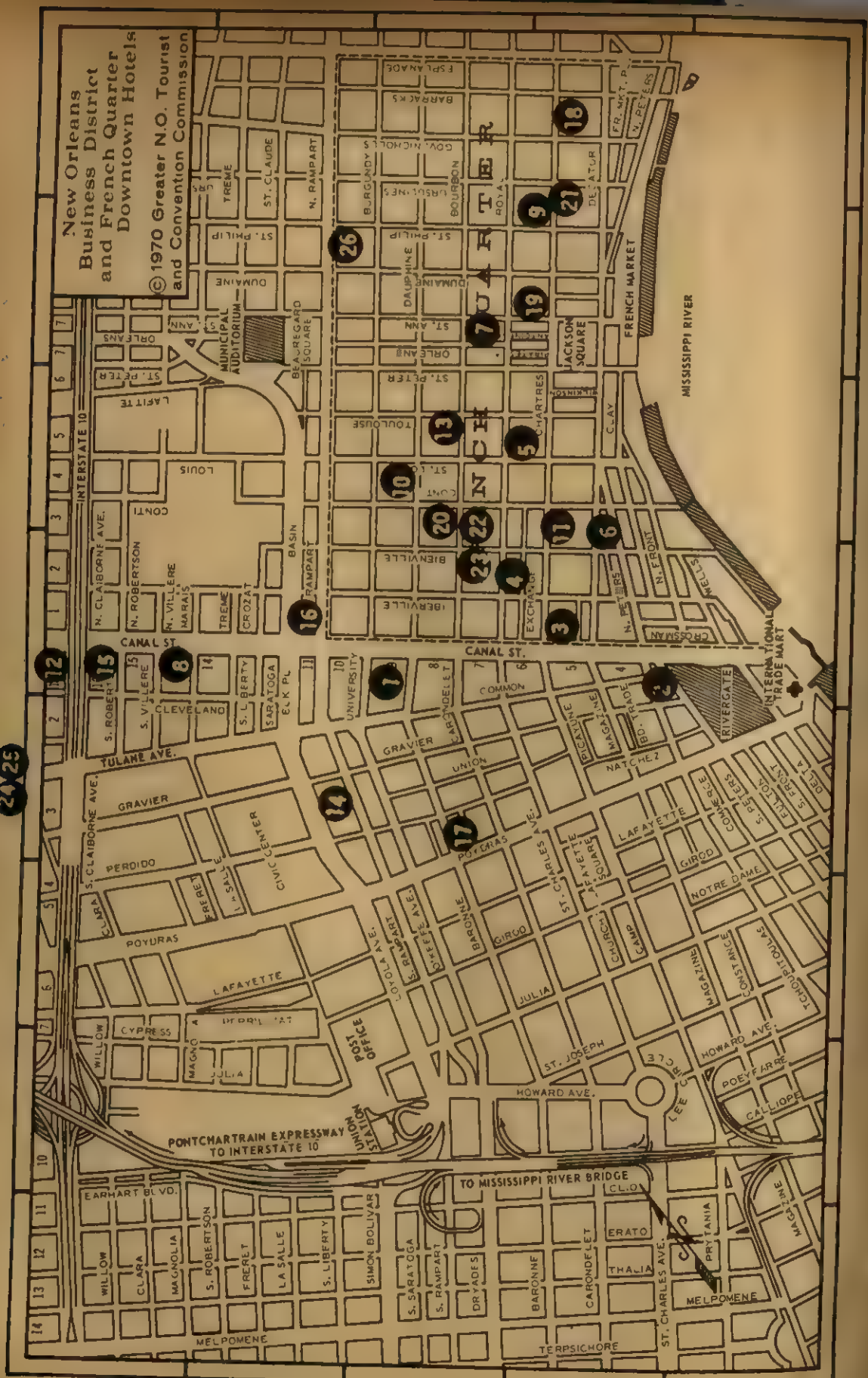
Transportation from the New Orleans International Airport to the downtown area is available by taxicab or limousine. It takes about 30 minutes to make the trip. The taxicab fare is approximately $9 plus tip for one to three persons; there is an additional charge of $3 per person for more than three. Limousine service is available directly to the hotels at $3 per person. Public transportation is also available from the airport for a fare of 35 cents, but it may be necessary to transfer to another bus within the city in order to get directly to your hotel. See you in New Orleans!

Housing

In cooperation with APA, all of the hotels have set aside, at guaranteed rates, substantial blocks of rooms. These hotels have guaranteed rates for the Convention only when registration is made through the APA Housing Bureau on the official Hotel Reservation Form. Also, these rates are guaranteed only when the Advance Registration Form and Hotel Reservation Form are returned *prior to August 1, 1974.* After August 1, every effort will be made to assign rooms at the guaranteed rate, but such assignments cannot be certain, and it is quite unlikely that late requests can be honored.

Members should also bear in mind that with the large number of rooms used, some members may not be assigned a room in the specific hotel they requested nor the type of room at the rate requested. It is advisable to return the Advance Registration Form and the Hotel Reservation Form as early as possible. These forms appear right before this section.

All housing forms are sent from the Central Office to the New Orleans Convention Bureau for room assignments. We anticipate a three- to four-week lag between receipt of request in the APA office and your receiving confirmation from the hotel. If after four weeks you have not heard from New Orleans, please contact *di-*


New Orleans
Business District
and French Quarter
Downtown Hotels
© 1970 Greater N.O. Tourist
and Convention Commission
FRENCH QUARTER
MISSISSIPPI RIVER
FRENCH MARKET
JACKSON SQUARE
BEAUREGARD SQUARE
MUNICIPAL AUDITORIUM
CIVIC CENTER
POST OFFICE
UNION STATION
INTERNATIONAL TRADE MART
RIVERGATE
CANAL ST.
INTERSTATE 10
PONTCHARTRAIN EXPRESSWAY
TO INTERSTATE 10
TO MISSISSIPPI RIVER BRIDGE
ESPLANADE
BARRACKS
GOV. NICHOLLS
BURGUNDY
BOURBON
ROYAL
DESATUR
ST. PHILIP
DAUPHINE
TOULOUSE
ST. LOUIS
CONTI
BIENVILLE
IBERVILLE
ST. ANN
ORLEANS
ST. PETER
CHARTRES
NORTHAMPTON
CLAY
N. FRONT
MELLS
CROSSMAN
N. PETERS
EXCHANGE
N. RAMPART
BASIN
ELK PL
UNIVERSITY
CARONDELET
COMMON
BD. TRADE
MAGAZINE
PICAYUNE
NATCHEZ
UNION
GRAVIER
LAFAYETTE
GIROD
NOTRE DAME
COMMERCE
FELICITY
S. FRONT
DELTA
POYDRAS
ST. CHARLES AVE.
LAFAYETTE SQUARE
CHURCH
CAMP
CONSTANCE
TCHOUPITOULAS
HOWARD AVE.
POEYFARRE
GALLIOPE
JULIA
ST. JOSEPH
LEE CIR.
S. RAMPART
O'KEEFE AVE.
BARONNE
LOYOLA AVE.
CYPRESS
MAGNOLIA
WILLOW
PERDIDO
CLARA
FRERET
LA SALLE
CIVIC CENTER
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TULANE AVE.
CLEVELAND
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N. VILLERE
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TREME
CROZAT
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ORLEANS
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DUMAINE
ST. PHILIP
TREME
ST. CLAUDE
N. RAMPART
EARHART BLVD.
SIMON BOLIVAR
DRYADES
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rectly the New Orleans Convention Bureau, 334 Royal Street, New Orleans, Louisiana 70130.

Wheelchair Accessibility

To facilitate making hotel reservations for those people concerned with wheelchair accessibility, Division 22, the Division of Rehabilitation Psychology, compiled the following information on the accessibility of some of the Convention hotels. Wherever possible, the information has been corroborated by site visits made by knowledgeable persons. Detailed information about the accessibility of various churches, restaurants, museums, and stores may be obtained by writing for a free copy of *Guide to New Orleans for the Handicapped* from Louisiana Chapter, National Society for Crippled Children and Adults, Inc., 843 Carondelet Street, New Orleans, Louisiana 70130.

Rivergate Exhibition Center

Main entrance: Accessible
Restrooms: Accessible
No sleeping rooms

International Hotel

Main entrance and elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Two meet accessibility requirements and *must be requested.*

Monteleone Hotel

Garage entrance and three elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Hotel has 56 accessible sleeping rooms which *must be specifically requested.* They are rooms on the fifth floor in the following series: 53, 56, 57, 58, 63, and 69.

Fairmont Hotel

One of the main entrances to the hotel is ramped.
Public restrooms: Women's—accessible
Men's—inaccessible
Sleeping rooms: Inaccessible

Royal Orleans Hotel

Main entrance: Assistance available to negotiate the steep ramped entrance from garage and staired main entrance.
Elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Inaccessible

Marriott Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Royal Sonesta Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Persons requesting wheelchair-accessible accommodations should check the appropriate box on the Hotel Reservation Form as well as indicate the hotel and type of accommodation requested.

Registration

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting, but advance registration represents a saving to the registrants as well as the opportunity to obtain desired hotel space and rates. At its meeting on January 18–20, 1974, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For those registering in advance (prior to August 1, 1974):

$30—Nonmember
$20—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For those registering on-site in New Orleans:

$35—Nonmember
$25—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Key to Map of New Orleans

1. Fairmont Hotel
2. International Hotel
3. Marriott Motor Hotel
4. Monteleone Hotel
5. Royal Orleans Hotel
6. Bienville House
7. Bourbon Orleans Ramada
8. Braniff Place
9. Chateau Motor Hotel
10. Dauphine Orleans Motor Hotel
11. de la Poste Motor Hotel
12. Delta Towers Hotel
13. Downtowner du Vieux Carre
14. Downtown Howard Johnson's Motor Lodge
15. Governor House
16. La Salle Hotel
17. Le Pavillon
18. Le Richelieu Motor Hotel
19. Place d'Armes Motor Hotel
20. Prince Conti Motor Hotel
21. Provincial Motor Hotel
22. Royal Sonesta Hotel
23. Saint Louis Hotel
24. Tamanaca (Best Western) Downtown Motel
25. Thunderbird (Quality Inn)
26. Vieux Carre Motor Lodge

Students and other authorized exemptions from these fees appear on the Advance Registration Form.

The convention badge, with name and institutional affiliation, will be mailed in advance of the Convention to those who preregister. Advance registrants will need only to obtain a badge holder at the APA registration area in New Orleans to complete the procedure—thus avoiding possible delays.

Complete member and nonmember registration facilities will be maintained at three locations—the Fairmont Hotel, the Marriott Hotel, and the Rivergate Exhibition Center—according to the following schedule:

Thursday, August 29, 3:00 p.m. to 9:00 p.m.
Friday, August 30, 8:30 a.m. to 5:30 p.m.
Saturday, August 31, 8:30 a.m. to 5:30 p.m.

From Sunday, September 1 through Tuesday, September 3, registration facilities will be available only at the Rivergate Exhibition Center. Hours will be 8:30 a.m. to 5:30 p.m. on Sunday and Monday; 8:30 a.m. to 12 noon on Tuesday.

Placement

The APA Convention Placement Office will be located in the Rivergate-South Hall. Hours will be from 9:00 a.m. to 5:00 p.m., August 30–September 3, except 1:00 p.m. closing day. Fees required are as follows:

Applicants—$2 for reproducing (1) availability information. $3 for use of Convention Placement facilities includes a copy of the *Position Openings Bulletin*. (Nonmembers will be charged $20 for use of Convention Placement facilities.) July 19, 1974, is the deadline for applicant listing. Appropriate fee must accompany listing.

Employers—$5 for reproducing (1) Job Description. $10 for use of Convention Placement facilities includes a copy of the *Availability Notices Placement Bulletin*. July 26, 1974, is the deadline for employer listing. Appropriate fee must accompany listing.

Employers: Please note the addition of the GENERAL OPENINGS category to be used by organizations having widespread needs for psychologists who may be recruited through a central office.

Prelisting in the more widely distributed printed booklet is encouraged, but Xerox forms will be available for on-site listing. The forms, accompanied by the appropriate listing fee, must be received by the date indicated above. Please do not include the Convention Placement facilities fee which will be collected ONLY when you register at the Placement Office. After the above date, Xerox forms will be available for listing, and requests should be directed to the address below.

A complete set of applicant availabilities will be available to employers: $15 for each category—please indicate choice. After the Convention, order from the Convention Placement Office, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Mail orders for the printed *Availability Notices Placement Bulletin* and the *Position Openings Bulletin* are $1.50 per copy (available August 7). Your check (no billing) may be sent to the above address.

Convention Personnel

You may wish to contact one or more of these people before or during the Convention:

Board of Convention Affairs: Carl N. Zimet, Division of Psychology, University of Colorado School of Medicine, Denver, Colorado 80220.

Convention Manager: Theodore G. Driscoll, Jr., Driscoll and Associates, 7109 Masters Drive, Potomac, Maryland 20854.

Assistant for Convention Management: Candy Won, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Convention Placement Office: Jim Beauter, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Please Note

Meetings for Divisions 19 and 21 will be located in the *Monteleone Hotel* rather than the Royal Orleans Hotel as noted in the convention insert.

Convention Child Care Service

Provision has been made for a professional child care service during the 1974 Convention. CON-SERV has been engaged and will offer an extensive program.

Parents desiring to use child care facilities must preregister no later than July 15, 1974. To preregister, complete the form at the bottom of this page. Please note that upon filing of the preregistration form and payment of the advance deposit as indicated on the form, CON-SERV will provide complete detailed information about the services offered and final registration materials necessary to plan for your children's comfort and safety. If for any reason your plans change, the deposit will be refunded in full if CON-SERV is notified of cancellation by August 10.

Some of the essential features of this service, as planned, are indicated below:

- The facility will be located in Braniff Place Hotel and will be open from 8:00 a.m. to 6:30 p.m., August 30–September 3, 1974. While the cost will be about $1.50 per hour per child, APA will subsidize $.75 of this cost. Hence, the cost to parents during the daytime will be $.75 per hour per child.

- If there is sufficient need, the facility will remain open after 6:30 p.m. until 12 midnight to accommodate parents who may wish to leave their children in the facility during evening hours. The parents will bear the full cost during these hours. If the need for evening child care does not justify keeping the facility open after 6:30 p.m., personnel at the facility will make every effort to help individual parents locate qualified babysitters to stay with children in their hotel rooms for those who might need this service. In any event, every effort will be made by CON-SERV to assist parents with evening child care arrangements.

- During the regular daytime hours, a variety of optional tours will also be offered at modest cost to parents. APA will subsidize part of the cost to the extent of $.75 per hour per child as though the child were in the facility.

- In order to evaluate the child care services at past conventions, the Task Force on Convention Child Care would like you to answer the following questions: (*a*) What was your general reaction to past child care facilities? (*b*) What was the least-liked aspect? (*c*) What was the most-liked aspect? (*d*) What was needed and not provided? Please attach your answers to the preregistration form.

Preregistration Form: Child Care Service—APA Convention
New Orleans, Louisiana, August 30–September 3, 1974

Preregistration form must be returned no later than July 15, 1974.

Name of Parent or Guardian: _____

Street Address: _____

City, State, Zip: _____

Name(s) of children to be registered in the child care facility:

Name	Age
_____	_____
_____	_____
_____	_____

If your child has a specific problem or handicap that we should be aware of, that is, speech impediment, retardation, epilepsy, etc., please let us know so that we may make the necessary provisions. _____

Please check this box if you are interested in tour information for your child

_____
Signature of Parent or Guardian

Please return this form to **Ms. Diane Snow, Con-serv, 1318 Arabella Street, New Orleans, Louisiana 70115,** and enclose a check in the amount of $7.50 for one child or $15.00 for two or more children as listed above. Make checks payable to "Con-serv." Indicate amount enclosed: $_____.

NOTE: This is only your preregistration form. Upon receipt of this form you will be furnished complete registration materials.

Employers of Psychologists:

If you have or anticipate openings for psychologists, you may wish to list them with the APA Convention Placement Service. The Job Description Form below is for this purpose. Additional copies supplied upon request.

A service fee of $5.00 per insertion will be required for listing in the **Convention Placement Bulletin.** FEE MUST ACCOMPANY COPY—no billing by APA—DEADLINE JULY 26. If you recruit at the Convention, an additional $10.00 fee will be required for use of the Placement Office facilities. This fee payable ONLY when you register at the Placement Office.

The FINAL FORM of your job description below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for publication. Please list your job data of the kind and in the order illustrated. Do not skip any lines. Thank you.

ILLUSTRATIVE FORMAT

Job title (caps) — Number of openings — Degree required or preferred — Date job begins

Starting salary & basis →

Duties →

Employer name & address →

```
CLINICAL PSYCHOLOGIST: 1 PhD or MA lacking only PhD thesis.  Now or Jan. 1975
$14,000-16,000/12 months.  Internship + 2 yrs experience in mental health
setting; teaching experience desirable.
Teach intro clinical psych to interns, nurses in university hospital: perform
diagnostic & therapeutic activities in univ mental health clinic; do research
in area of own interest as time allows. Joint appointment in clinic & psych
dept possible.
JA Smith, Chairman, Dept Psych, Univ of Erie, Centerville 3, NY.
See him Aug. 30, 31, Sept. 1, 2, am & pm, or write.
```

Tentative interview schedule (Aug 30-Sept. 2, am &. or pm, Sept. 3 until 1 pm are the available times).
If none, enter name to whom inquiries may be mailed.

WORKING COPY—Draft your job description here, following the above guidelines.

1 2 3 4 5 6 7 8 9

FINAL FORM—TYPE YOUR FINAL JOB DESCRIPTION WITHIN BOUNDARY ONLY.

Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research ☐ General Openings

1 2 3 4 5 6 7 8 9

Detach on dashed line and return to:
DEADLINE: JULY 26, 1974

Convention Placement Office, American Psychological Association
1200 Seventeenth Street, N.W., Washington, D.C. 20036

[illegible]

[illegible]

[illegible]

Comment

The XYY Chromosomal Abnormality: Some "Facts" and Some "Fantasies"?

In their article entitled "Human Aggression and the Extra Y Chromosome: Fact or Fantasy?" Jarvik, Klodin, and Matsuyama (August 1973) reviewed some of the literature which has accumulated on a rather intriguing and controversial subject. The behavioral characteristics of XYY males have been the topic of considerable speculation and discussion in recent years.

The article by Jarvik et al. [illegible]

[illegible]

times higher than the estimated general population rate of 1:700.[1]

Table 4 (Chromosome Surveys of Criminals) also reveals a confounding of studies which did and did not select for height. Without having tried to check carefully every single citation in this table, we were able to determine that several of the studies had screened only tall subjects (e.g., Falek, Craddick, & Collum, 1970; Marinello, Berkson, Edwards, & Bannerman, 1969; Melnyk, Derencsenyi, Vanacek, Rucci, & Thompson, 1969; Welch, Borgaonkar, & Herr, 1967). However, Jarvik et al. (1973) made no distinctions between these studies; nor did they even provide any information about the height selection factor.

There are still other problems with Table 4. We have already indicated the differing prevalence rates obtained for regular mental hospitals and penal institutions, and the distinctly higher rates rather consistently found in security hospitals (namely, institutions for mentally disordered offenders). In Table 4, Jarvik et al. included *at least* five studies which pertain to *mentally disordered offenders* (namely, Abdullah, Jarvik, Kato, Johnston, & Lanzkron, 1969; Melnyk, Derencsenyi, Vanacek, Rucci, & Thompson, 1969; Nielsen, Stürup, Tsuboi, & Romano, 1969; Nielsen, Tsuboi, Tuver, Jensen, & Sachs, 1969; Welch, Borgaonkar, & Herr, 1967). Not only is such lumping together of studies misleading, but in addition, the reader is not even informed about the types of studies and populations included in this table.

Finally, we wish to address ourselves to the repeated statements by Jarvik et al. linking the XYY chromosome complement and aggression. The title of their article ("Human Aggression and the Extra Y Chromosome") as well as numerous other statements suggest that males with 47,XYY karyotypes tend to be rather aggressive. For example, references were made to the "unusually aggressive temperament," the "dominant feature" of "excessive episodic aggressiveness," and the "powerful aggressive tendencies" of many XYY males. At one point the authors stated: "Since the XYY genotype has been associated with antisocial behavior, it would be reasonable to expect a higher frequency of this disorder among persons who have come in conflict with the law than among men in general [p. 679]." And, again, the authors asserted: "The data available so far, therefore, provide strong presumptive evidence for an association between criminal behavior and the extra Y chromosome [p. 679]."

To begin with, the data to which these authors alluded do *not* link the XYY karyotype with either "antisocial behavior" or "criminal behavior." The studies in question pertain to offenders who have been incarcerated in various institutions. This latter group represents a small and highly selected sample of all those convicted of crimes; those convicted of crimes represent a small fraction of all those who come to official attention for criminal behavior. Furthermore, not all of the criminal behavior comes to official attention, and not all behavior that may be considered "antisocial" is necessarily defined as "criminal" (e.g., see Shah, 1972). To the extent that several studies do suggest a higher prevalence rate for the XYY condition among institutionalized offenders, the increased risk which may be incurred pertains to certain types of *institutionalization*—and not for antisocial or criminal behavior as such.

Even though Jarvik et al. made numerous references to the "aggressiveness" and the "impulsive violence" of XYY males, they failed to provide any clear or convincing evidence to support such assertions. They did cite some highly publicized instances of XYY males charged with murder. It would have been interesting to know, for example, if the XYY males in various mental, penal, and security hospitals had increased rates of involvement in violent crimes, and if they had displayed more violent behavior toward other patients and staff, in reference to comparable groups of 46,XY males *in the same institutions*. Such information was not provided by the authors even though it is available in the literature.

For example, in preparing a recently completed comprehensive review of the XYY chromosome complement (Borgaonkar & Shah, in press), we were able to report on a number of studies which provide data that seriously challenge assertions about the "unusual aggressiveness" of XYY males. These data pertain to psychological test indices as well as to aspects of institutional adjustment. See, for example, Hope, Phillips, and Loughran (1967); Little (1968); Price and Whatmore (1967a, 1967b); and Street and Watson (1969).

We would not assert that the data thus far available clearly indicate a lack of aggressive and violent behavior among XYY males as compared to control subjects from the same institutions. However, these data do lead us to question the assertions made by Jarvik et al. about the unusual aggressiveness of XYY males. Based on presently available data, it would appear that, in general, XYY males have been falsely stigmatized as a group.

In closing we wish to point out that we are *not* trying to explain away biologically determined factors in human behavior; nor are we inclined to explain behavioral phenotypes entirely on the basis of environmental influences. Indeed, we have had occasion to point out that behavioral and social scientists have typically displayed an environmentalist bias (e.g., see Shah, 1972; Shah & Roth, in press). However, what we wish to emphasize is that the issues discussed by Jarvik et al. cannot and should not be posed in an either/or fashion. The "basic question," we would suggest, pertains to

[1] On the basis of more recent estimates (Borgaonkar & Shah, in press), it would appear that the prevalence rate for the general population may be even lower, namely, in the range of 1 XYY in 1,500–3,000 males.

the need for more sophisticated conceptualization of the aforementioned issues and more careful formulation and testing of relevant hypotheses.

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Saleem A. Shah
Center for Studies of Crime and Delinquency
National Institute of Mental Health
Rockville, Maryland

Digamber S. Borgaonkar
Division of Medical Genetics
Johns Hopkins University School of Medicine

Another Career Choice for BA Psychology Students

I would like to congratulate Pinkus and Korn (August 1973) for their efforts on behalf of the BA (psychology) graduate. My own dual roles in psychology and engineering have given me many occasions to reflect on the vast difference between the utilization of bachelors graduates in these two fields and, following from this, the great differences in attention paid to the undergraduate and his curriculum. I think many more professors in psychology would feel better about their undergraduate teaching and advising if they felt it really had some fairly definite and important purpose. I hope individual faculty, departments, and the APA will ponder this question further.

I would like to introduce another career direction to this discussion: the engineering psychology/human factors/ergonomics area. Although I currently believe that a master's degree is necessary for adequate entry here, I also believe that delineation of this area is in the spirit of the article's considerations. While at the University of California, Berkeley, and now at the University of Wisconsin, Madison, I have had the occasion to advise a number of BA psychology students who subsequently gained an MS in engineering in the Industrial Engineering Department. While I cannot claim to have any final curriculum suggestions, it is clear to me that an MS in appropriate engineering study *can* follow the BA in psychology and, more importantly, can *build* on it, rather than simply consider it largely "wasted time." What can be done, of course, depends largely on what the student did with his BA—a point more closely in tune with the article. Last week I had the singular joy of helping a new junior in psychology plan for a human factors career. It was clear that in his remaining two years he had a number of choices within his BA requirements that could greatly effect his entry into the field. Unfortunately most psychology students do not get advice this early.

I believe the appropriate MS product in this field is a student who

will occupy the role of a human factors specialist within a broader systems engineering framework. To implement this I advise students to pursue two components in their master's work: (*a*) sharpen their "applied experimental psychology" skills in the direction of human factors design and research needs and (*b*) acquire skills in system engineering analysis and design. Our department offers a number of graduate courses in analysis and design that a (properly prepared) BA in psychology is perfectly capable of using.

There are jobs in a great variety of places for such students. I would like to encourage psychology departments and undergraduate students to contact human factors, industrial engineering, or systems engineering specialists and departments to explore this potential.

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Gordon H. Robinson
Department of Industrial Engineering
University of Wisconsin, Madison

Certification of School Psychologists

In "Requirements for Certification of School Psychologists," Graff and Clair (August 1973) wrote, "It appears that the profession of school psychology in Canada is either quite limited, or not regulated by provincial departments of education [p. 709]."

I can assure them that school psychology is in a very healthy state at least in some parts of Canada, particularly in Alberta, Quebec, and Ontario. The Calgary Board of Education's Division of Special Educational Services, for example, has a very flourishing school psychology section. The second half of Graff and Clair's statement is, dei gratia, true.

Graff and Clair also implied that Ontario is the only province with certification. This is not the case. I believe each of the 10 provinces now has a certification act. Psychologists are, however, certified as psychologists, not as particular specialists. At least in Alberta, control of specializations is achieved by means of the Ethics Committee of the Psychologists Association of Alberta rather than by specific legislation. It may be of interest to know that of the 335 certified psychologists in the province, about 33% are engaged in school psychology in one form or another. Of another roughly 33% in the universities, several are school psychologists by origin.

At both the University of Calgary and the University of Alberta (Edmonton), there are important training programs at the master's and doctoral levels for school psychologists. In both cases these programs are in the Department of Educational Psychology.

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B. P. Frost
University of Calgary

Editor's note. Another reader (Carole W. Kinder, Follow Through Program, Fairfield County Schools, Winnsboro, South Carolina) has noted that the certification requirements for South Carolina as they appear in the Graff and Clair article are in error (p. 707). South Carolina does require a teaching credential as well as an internship for all three levels of school psychologist. There is also an educational evaluator position which requires 18 hours of special courses, a teaching credential, and an "externship."

On "A Functional Analysis of Depression"

As a practicing behavioral therapist, I found that Ferster (October 1973) failed to recognize some significant clinical subtleties in his functional analysis of depression. Nonconsideration of these subtleties by therapists is potentially dangerous. For example, Ferster reported that a depressed person "repeatedly tells how badly he feels, cries, talks about suicide, and complains of fatigue and illness [p. 858]." But what about the "smiling depressives" who deny their feelings of pervasive gloom and, after lulling their gullible therapists into a false sense of security, enter the ultimate extinction process via an overdose of some lethal "stimulus"? While Ferster faintly alluded to masked depressions (i.e., those characterized by compulsive, phobic, or ritualistic behaviors), he does not appear to recognize their fundamental clinical significance. Behavioral trainees must be warned to avoid decreasing certain compulsive rituals without recognizing and treating the basic depressive reactions of which they may be a consequence.

It seems that Ferster believes that endogenous depressions (i.e., those due mainly to biochemical, hormonal, and other physiological factors) are necessarily maintained by their functional consequences. This is highly debatable. However, if many of these cases do not receive corrective medication, they may not be available for us to determine the "relation between the [depressed] behavior and the environment that prompts, shapes, and maintains it [p. 861]."

In specifying "decreased frequency" as "the common denominator among depressed persons [p. 861]," Ferster contradicts himself by subsequently addressing the fact that many depressed persons carry on with their day-to-day activities, but nevertheless experience a loss of reinforcer effectiveness in situations that previously had proved highly rewarding. It is circular reasoning simply to ascribe the foregoing phenomenon to inherently unrewarding schedules of reinforcement. The main point about depressed persons is not only the absence of immediate

positive reinforcement but *a singular lack of hope of receiving future rewards*. The latter consideration led me to develop a therapeutic "positive time projection" technique several years ago (Lazarus, 1968).

Apart from the fact that many depressed persons may not exhibit any decreased frequency in overt activity despite deep subjective levels of misery and feelings of personal unworthiness, there is another error in Ferster's functional formulation. To paraphrase a point that had been underscored by Ferster (1965): Whether a man who becomes less active and more passive is necessarily depressed cannot be determined by observing his behavior alone.

Ferster's penchant for reducing certain complex human behaviors to those of key-pecking pigeons is curiously interspersed with psychodynamic formulations such as "primary and secondary process" (p. 863) and even "repression" (pp. 866, 867). This *paradigm clash* creates the illusion of pseudoscientific respectability for psychoanalysis while breeding a state of confusion worse confounded for behavioral clinicians. All in all, we have yet another instance of a brilliant laboratory scientist overlooking crucial components in the less structured gray world of the clinic and the natural environment "because the tight environment of the experimental testing ground makes it impossible for certain behaviors to occur or for certain observations to be made [Lazarus & Davison, 1971, p. 196]."

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Arnold A. Lazarus
University College
Rutgers—The State University

Ferster Replies:

I neither intended to make a case for psychoanalysis nor wished to ignore the knowledge of human conduct from that point of view. Apparently, my use of the terms *primary process* and *repression* was found to be distressing. I will postpone to a future paper a substantive discussion of the aspects of human conduct that prompt clinical observers to use these terms. For the present, I will comment on why, as a behaviorist, I pay attention to mentalistic concepts, such as those of Freud and others who followed his lead.

I have never thought of behavior principles as a substitute for knowledge about the content of human activity. Nor have I been able to accept the conclusion that all psychodynamic clinicians ensnared patients into years in treatment without tangible benefit. Clinicians I have known have described their patients to me in sufficiently plain English about overt behavior so that I feel assured that large therapeutic changes were accomplished. I have found that my training as a behaviorist and my attitude as a natural scientist have given me the possibility of crossing the boundary of the clinician's mentalistic language, not by translating it but by using it as an indicator of what the clinician has observed that prompts him to speak as he does. The language and the epistemology of psychodynamic concepts are not to my taste, but I am impelled to deal with them because they are used by many of the clinicians with the most experience with the widest range of behavioral problems. When I hear a clinician talking about a patient, even mentalistically, my habit is to question him about what he observed firsthand. In most cases, I find the facts and observations quite readily available, particularly when I show an interest in the details of what has gone on between him and his patient.

Although I have done enough clinical work, both individual and group, to make me familiar with the major dimensions of psychopathology and psychotherapy, it is quite correct that as a person trained primarily as an experimental psychologist, I have not met the breadth and variety of problems that would be the case if I were a full-time clinician. Perhaps it is just this combination of point of view that allowed me to break away from a formal topographic view of depression in favor of a functional analysis emphasizing the frequency of generically defined classes of behavior. It is only in this kind of simplification that the analyses I presented have any advantage over the detailed description of the practitioner. Such a theoretical organization helps the reader to notice aspects of the patient's behavior that will guide his intervention. The theory presented in my article dealt with frequency as the dependent variable and the basic behavioral processes as the independent variable. In psychoanalytic theory, the major events are the child's developmental interaction with the parents. Hopefully, the theoretical organization of a functional analysis of behavior will help to make the knowledge of psychodynamic psychology and practical knowledge of depression communicable.

It should seem clear at this point that I did not intend my article to be a guide for beginners to treat depression, like they would use a microtomist's vade mecum. I don't believe there is a quick way to learn to treat difficult patients. My hope would be that it would be a guide for acquiring clinical experience more efficiently than from a mentalistic vantage point. I believe that

a beginner has a better chance of identifying the "the smiling depressive" or having some perspective about how to think about drugs and hormones if he understands the functional approach to describing depression than without it. The real danger to effective clinical work is to shut oneself off from information that can assist in the patient's treatments.

C. B. Ferster
American University

A Return to Introspectionism?

Royce (October 1973) documented well the cycle of monism–dualism in the philosophy of man and was accurate in his denunciation of the "official doctrine" of dualism.

Skinner's (1971) straw-man account of "autonomous man" revealed him to be a dyed-in-the-wool dualist. Although he strongly denies the existence of the "fictitious" autonomous man, Skinner advocates that society express its free will by controlling contingencies of reinforcement on a large social scale. But if behavioral determinism is to reign supreme and if Skinner has really been successful in debating to death his free-willing "autonomous man," then from whence comes the human or mystical force to interfere with the determined natural and social phenomena? "Autonomous man" refuses to die, even at the hands of such an eloquent positivist.

Psychologists might find interesting a new interpretation of quantum mechanics which offers a pluralistic model for monism. This model, developed by Everett (1957) under the academic guidance of Wheeler (1957), includes a noncollapsing state vector for the whole universe. This state vector may be split up into orthogonal vectors, each of which is observed from a mutually exclusive perspective. This splitting into orthogonal vectors reflects a continual splitting of the universe into a multiplicity of mutually unobservable but equally real universes. The splitting is induced primarily by the act of human measurement, although the observer sees only the universe-as-he-sees-it and not the splitting process itself (DeWitt, 1970). For any given time period the observer can be aware of only one of a number of worlds (Cooper & Van Vechten, 1969).

A return to introspectionism? But of course! Psychologists Wundt have it any other way, it seems.

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David Ryback
West Georgia College

More on Sex and Salary

Cates (October 1973) noted that female psychologists earn less money than male psychologists. While the point is valid and needs to be made, the magnitude of the difference may be deceiving.

Specifically, would the magnitude shown be as large if the variables of age and years of professional experience were held constant? Any data on the point?

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Cates, J. N. Sex and salary. *American Psychologist*, 1973, 28, 929.

Eric C. Theiner
Southaven, Mississippi

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Southwestern Psychological Association: April 17–19, 1975; Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Rocky Mountain Psychological Association: 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque

For information write to:

Irwin Cohen
Veterans Administration Hospital
Mental Hygiene Clinic
1055 Clermont Street
Denver, Colorado 80220

New England Psychological Association: November 15–16, 1974; Boston, Massachusetts

For information write to:

Barbara Ross
University of Massachusetts
100 Arlington Street
Boston, Massachusetts 02116

Association for the Care of Children in Hospitals: May 29–June 1, 1974; Chicago, Illinois

For information write to:

Myrtha Sice
Publicity, 1974 ACCH Conference
Children's Memorial Hospital
2300 Children's Plaza
Chicago, Illinois 60614

American Association on Mental Deficiency: June 2–7, 1974; Toronto, Canada

For information write to:

George Soloyanis
American Association on Mental Deficiency
5201 Connecticut Avenue, N.W.
Washington, D.C. 20015

Association for the Psychophysiological Study of Sleep: June 6–9, 1974; Jackson Hole, Wyoming

For information write to:

David Foulkes
University of Wyoming
Box 3291
University Station, Wyoming 82071

Society for Psychotherapy Research: June 13–15, 1974; Denver, Colorado

For information write to:

Allen E. Bergin
Faculty Office Building
Brigham Young University
Provo, Utah 84602

Conference on Humanistic Education: August 21–23, 1974; Carrollton, Georgia

For information write to:

David Ryback
Education Center
West Georgia College
Carrollton, Georgia 30117

Association for Humanistic Psychology: August 25–28, 1974; New Orleans, Louisiana

For information write to:

John Levy
Association for Humanistic Psychology
325 Ninth Street
San Francisco, California 94103

Postdoctoral Institute: August 27–29, 1974; New Orleans, Louisiana

For information write to:

Arthur J. Gallese
Department of Psychiatry
Louisiana State University
Medical School
1542 Tulane Avenue
New Orleans, Louisiana 70112

Society for Ear, Nose, and Throat Advances in Children: October 18–19, 1974; San Francisco, California

For information write to:

Virgil M. Howie
2345 Whitesburg Drive, S.E.
Huntsville, Alabama 35801

Family Institute of Philadelphia: October 24–25, 1974; King of Prussia, Pennsylvania

For information write to:

Florence Kaslow
807 Buckingham Road
Rydal, Pennsylvania 19046

Association for Advancement of Behavior Therapy: November 1–3, 1974; Chicago, Illinois

For information write to:

David H. Barlow
Program Chair, AABT
305 East 45th Street
New York, New York 10017

American Society of Clinical Hypnosis: November 5–10, 1974; New Orleans, Louisiana

For information write to:

F. D. Nowlin
American Society of Clinical Hypnosis
800 Washington Avenue Southeast
Minneapolis, Minnesota 55414

Third Information and Feedback Conference: People Services: November 7–8, 1974; Toronto, Canada

For information write to:

IF Conference Committee
Counselling and Development Centre
York University
Toronto, Ontario, Canada

National Conference on the Use of On-Line Computers in Psychology: November 20, 1974; Boston, Massachusetts

For information write to:

N. John Castellan, Jr.
Department of Psychology
Indiana University
Bloomington, Indiana 47401

Psychonomic Society: November 21–23, 1974; Boston, Massachusetts

For information write to:

Frederick A. Mote
Department of Psychology
University of Wisconsin
Madison, Wisconsin 53706

International Conventions

Thirty-Fifth Annual Meeting of the Canadian Psychological Association: June 12–14, 1974; Windsor, Ontario

For information write to:

Miriam E. Bunt
Department of Psychology
University of Windsor
Windsor, Ontario, Canada

First Worldwide Conference on Medical Sexology: July 3–6, 1974; Paris, France

For information write to:

Hans Lehfeldt
Society for the Scientific Study of Sex
784 Park Avenue
New York, New York 10021

Fourth Annual Conference of the European Association for Behaviour Therapy: July 11–15, 1974; London

For information write to:

Lawrence E. Burns
Department of Psychology
Birch Hill Hospital
Rochdale, Lancaster, OL12 9QB, England

Eighteenth International Congress of Applied Psychology: July 28–August 2, 1974; Montreal

For information write to:

Secretariat of the 18th International Congress of Applied Psychology, Inc.
C. P. 242
Station Youville
Montreal, Quebec, Canada

Eighth Congress of the International Association for Child Psychiatry and Allied Professions: July 29–August 2, 1974; Philadelphia, Pennsylvania

For information write to:

Albert J. Solnit
Yale University
333 Cedar Street
New Haven, Connecticut 06510

Fifth International Conference on Humanistic Psychology and Second Indian Conference on Humanistic Psychology: August 1–4, 1974; Vishnakapatnam, India

For information write to:

Carmi Harari
285 Central Park West
New York, New York 10024

Second International Conference of the International Association for Cross-Cultural Psychology: August 6–10, 1974; Kingston, Canada

For information write to:

J. W. Berry
Psychology Department
Queen's University
Kingston, Ontario, Canada

Fourth International Conference on Social Science and Medicine: August 12–16, 1974; Elsinore, Denmark

For information write to:

P. J. M. McEwan
Centre for Social Research
University of Sussex
Falmer, Brighton
Sussex, BN1 9RF, England

Seventeenth International Convention of the Parapsychological Association: August 22–24, 1974; Jamaica, New York

For information write to:

J. G. Pratt
Division of Parapsychology
Box 152, University of Virgina Medical Center
Charlottesville, Virginia 22901

Thirty-Second Annual Conference of the International Council of Psychologists, Inc.: August 29–September 4, 1974; New Orleans, Louisiana

For information write to:

Frances A. Mullen
2901 King Drive, No. 1017
Chicago, Illinois 60616

International Conference on Dimensions of Anxiety and Stress: September 2–5, 1974; Athens, Greece

For information write to:

Irwin G. Sarason
Department of Psychology NI-25
University of Washington
Seattle, Washington 98195

First International Congress on Obesity: October 9–11, 1974; London, England

For information write to:

Kim Solly
1st International Congress on Obesity
22 Montagu Street
London, W1H 2BR, England

Sixtieth Annual Convention of the International Association of Pupil Personnel Workers: October 27–31, 1974; Atlantic City, New Jersey

For information write to:

Joan E. Gaeng
Montclair State College
Upper Montclair, New Jersey 07043

Fourth International Congress of Psychosomatic Obstetrics and Gynecology: October 27–November 2, 1974; Tel Aviv, Israel

For information write to:

Organizing Committee, Congress of Psychosomatic Obstetrics and Gynecology
P.O. Box 16271
Tel Aviv, Israel

Fifteenth Interamerican Congress of Psychology: December 14–19, 1974; Bogotá, Colombia

For information write to:

Liuz F. S. Natalicio
Secretary General
Interamerican Society of Psychology
P.O. Box 88 UTEP
El Paso, Texas 79968

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29–January 1, 1975; Nairobi, Kenya

For information write to:

S. H. Irvine
College of Education
Brock University
St. Catharines, Ontario, Canada

International Conference on Psychological Stress and Adjustment in Time of War and Peace: January 6–10, 1975; Tel Aviv, Israel

For information write to:

Norman Milgram
Department of Psychology
Tel Aviv University
P.O. Box 16271
Tel Aviv, Israel

Tenth International Congress of Gerontology (and Geriatrics): June 22–27, 1975; Jerusalem, Israel

For information write to:

The Congress of Gerontology
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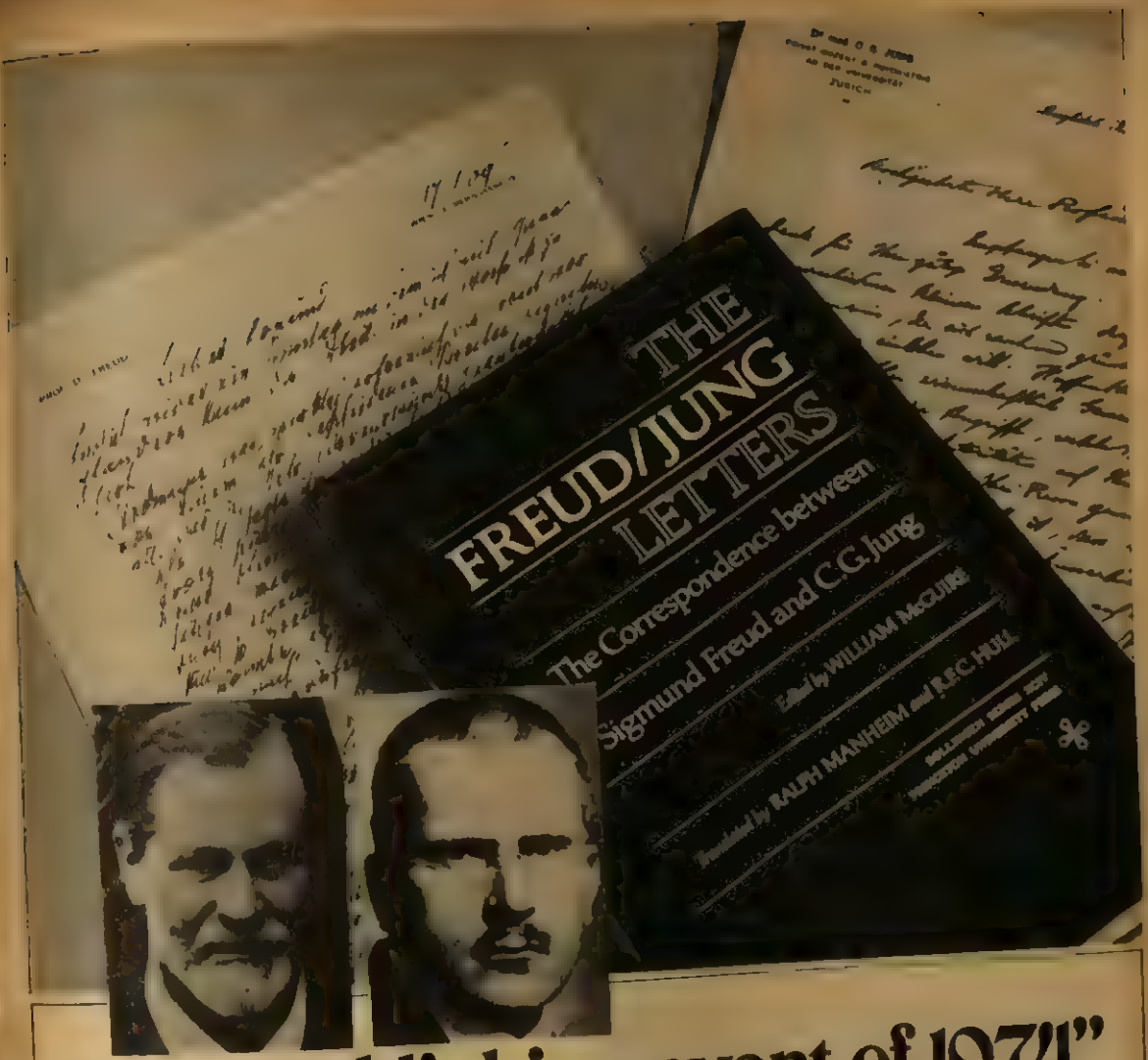
Tel Aviv, Israel



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Edited by WILLIAM McGUIRE
Translated by RALPH MANHEIM and R.F.C. HULL

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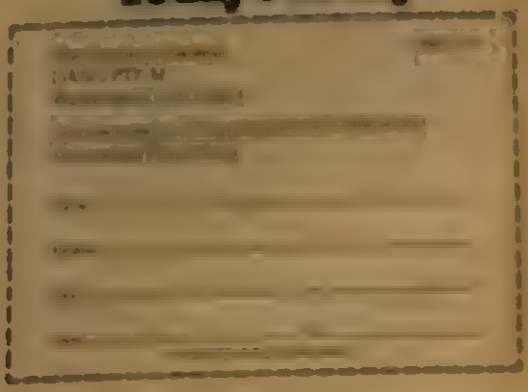

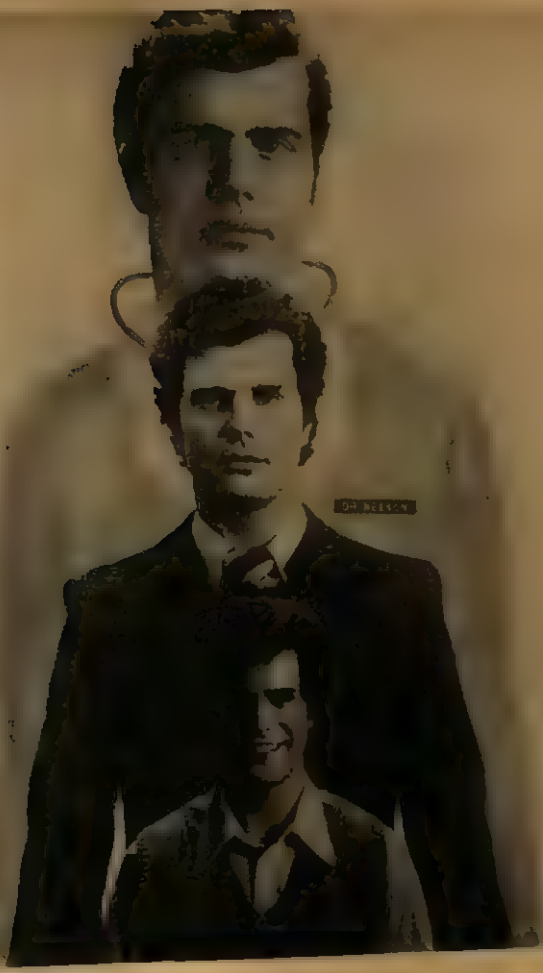

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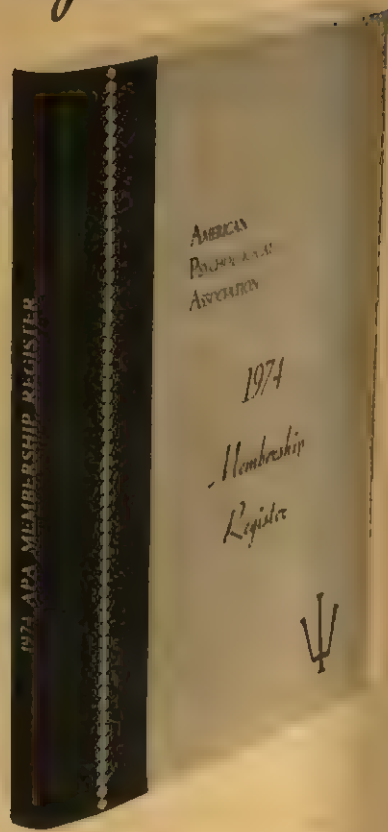

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Journal of the American Psychological Association

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AMERICAN PSYCHOLOGICAL ASSOCIATION

The American Psychological Association, founded in 1892 and incorporated in 1935, is the major psychological organization in the United States. The purpose of the APA is to advance psychology as a science and as a means of promoting human welfare. It attempts to accomplish these objectives by holding annual meetings, publishing psychological journals, and working toward improved standards for psychological training and service.

In addition to the *American Psychologist*, the Association publishes 13 other journals: *Contemporary Psychology*, *Developmental Psychology*, *Journal of Abnormal Psychology*, *Journal of Applied Psychology*, *Journal of Comparative and Physiological Psychology*, *Journal of Consulting and Clinical Psychology*, *Journal of Counseling Psychology*, *Journal of Educational Psychology*, *Journal of Experimental Psychology*, *Journal of Personality and Social Psychology*, *Professional Psychology*, *Psychological Bulletin*, and *Psychological Review*. The APA also publishes *Psychological Abstracts*, the *APA Monitor*, a monthly *Employment Bulletin*, the *Biographical Directory*, the *Convention Program*, and the *Catalog of Selected Documents in Psychology*.

Report of the Executive Officer: 1973

KENNETH B. LITTLE, *American Psychological Association*[1]

Annually, the Executive Officer is required to report briefly to the Council of Representatives and in writing to the members of the APA on the activities of the Central Office, the administrative arm of the Association. The Bylaws are remarkably vague on the nature of those activities, stating merely that there shall be a Central Office with such functions and functions as Council may assign it, that the Executive Officer shall be the Director of the Central Office, and that the Board of Directors shall supervise him in the execution of his duties. One of the original justifications for a Central Office was the operation of a placement service for returning psychologist veterans, an activity still in operation although with somewhat fewer veterans. However, functions and staff have increased in number since those early days, so much so that this report is limited to highlights.

In contrast to the single brief paragraph on the Central Office in the Bylaws, there is a wealth of detail on the governing structure of APA in that document. This consists of the Council of Representatives as the legislative body, its executive committee, the Board of Directors, and a sizable number of boards and committees reporting through the Board to Council. The system of boards and committees and elected representatives constitutes the decision-making mechanism of the Association. Its general function is to generate and debate policy and to establish the nature of the activities of the Association. It is the task of Council to consider recommendations that have filtered upward through the system for final action or to be informed of interim action that has been taken by the Board of Directors under the authority granted it.

A small number of the governance units are operational committees, that is, committees that have specifically delegated duties which are carried out during the year and reported on only annually to the Council. Two excellent examples of such committees are the Accreditation Committee and the Committee on Scientific and Professional Ethics and Conduct. Policies governing such operational committees have been hammered out over the three decades of APA's existence in its present form and are embodied as specific directives in various legal documents including the Bylaws and the Rules of Council.

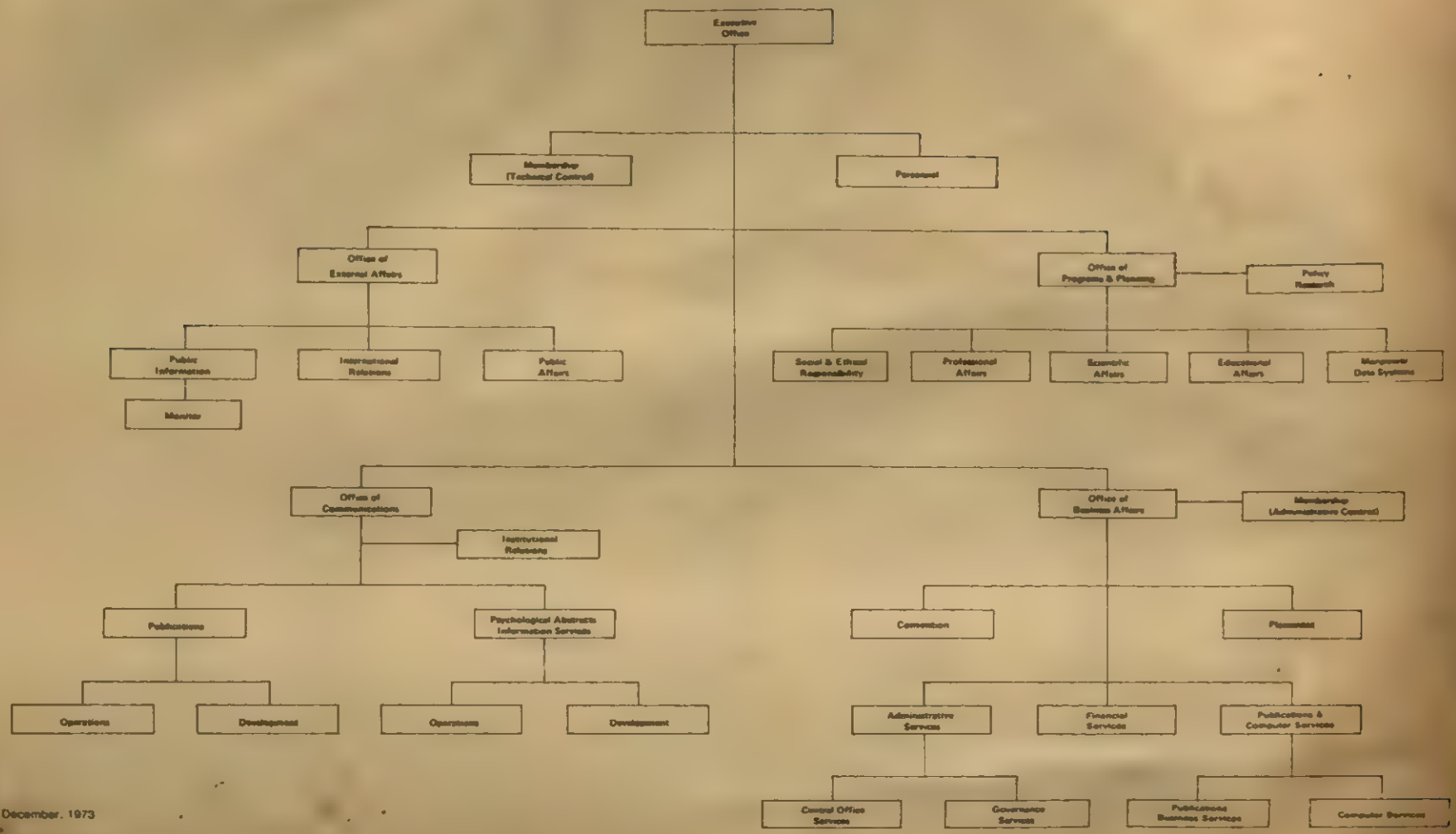
The organization of the Central Office only roughly corresponds to the extensive and complicated governing structure. The Central Office table of organization as of December 1973 appears in this article (p. 1666) but should be taken with a grain of salt. It changes as positions and personnel come and go and sometimes just for the hell of it.

The lack of exact correspondence is explicable if one accepts the fact that the APA is about 80% a business (APA in 1973). This means that about [illegible] of its revenues (and [illegible] of its expenses) are for direct services and products. The Association receives the money and contracts to deliver, for example, 12 issues of a journal, an accreditation visit, a copy of the *Graduate Study Bulletin*, a PANAR search, a convention, an advertisement in the *Employment Bulletin*, or arrangements for a Head Start consultant to a regional center for the U.S. Department of Health, Education and Welfare (DHEW). An [illegible] amount of services or products is [illegible] by [illegible] recipient. For example, at no cost to the [illegible] a member or [illegible] can secure [illegible] in APA services as presentation of an [illegible] member pay [illegible] and yearly [illegible] of various units are given gratis annually. Such business operations have no [illegible] parallel in the governing structure but are run by the Executive Officer and staff under the supervision of the Board of Directors.

This business-like character of the Association markedly limits the amount of discretionary resources available for Association use in non-revenue-producing activity. As in the nature of business activities, and APA is an example, they are tre-

[1] Report made to the Council of Representatives of the American Psychological Association, Washington, D.C., January 18, 1974.

AMERICAN PSYCHOLOGICAL ASSOCIATION
CENTRAL OFFICE ORGANIZATION



December, 1973

mendously dependent on events beyond their control. No vote of APA could stop the rise in postal rates March 1; no vote of APA will increase the amount and quality of paper available to our printers; no vote of APA can halt the inflation in our economy.

Since there is a positive and significant relationship between Association expenditures and a number of Central Office staff members, clearly a great deal of our work is devoted to business activities. For the remainder of staff time and effort, the next largest portion is in logistic support of boards and committees of the policy- and decision-making arm of the Association as well as operational committees. It is spent on such things as preparation for meetings, including accumulating agenda items from various sources, working up an agenda with background data, making physical arrangements for the meeting, participating in the discussion and taking notes, perhaps writing the minutes, acting as a resource person, and following up on actions of the committee to make sure that the several stages of the decision process are covered and the decision itself is implemented when approved. As a lower estimate, 150 meetings of APA boards and committees took place last year involving a minimum of 300 individual staff member days merely sitting in meetings for which the individual had direct responsibility. For the primary staff member, an additional several days are involved in preparation for the meeting and several more are spent in following up on its details.

The staff member days in meetings are increased substantially by the fact that most involve more than one staff member. A Board of Directors or a Council meeting may require the presence or availability of over 75% of the professional staff. In 1973 it looks as if the record will be, for one staff member, over 60 working days of meetings or 12 working weeks in board and committee meetings. This might help explain why the staff winces each time the Association proposes to establish yet another such group. To these Association meetings must be added the regular and ad hoc liaison meetings with outside groups: the Scientific Manpower Commission, the National Advisory Mental Health Council, the American Federation of Abstractors, the National Research Council, and the other 30-odd organizations with which liaison is maintained.

What does the Central Office staff do with the balance of its time? To a large extent it answers mail or otherwise serves in an information-dispensing role. Well over one million pieces of mail arrive at 1200 Seventeenth Street each year (28 tons of it in 1973), only a small portion of which are dues payments or even complaints. The vast majority are questions from teachers, from students, from the general public, from legislators and government officials, and from writers. You name it, we get it and we answer it. Of course, no one office deals with this correspondence. Central Office staff members at senior levels are selected for competence and knowledgeability about certain areas and can deal with most of the questions that arise. Most staff members soon learn considerably more than they knew when they came on board or perhaps wish to know. In addition, the Central Office maintains formal clearinghouses in precollege psychology, psychology's manpower, and research funding sources, as well as an administrative section for general correspondence.

With that general overview of Central Office functioning, let me turn to 1973. From the program staff's point of view, it was a miserable year largely because of the politicization of the Association, the instigation on false pretenses of a special meeting of Council, and a barrage of general and personal invective directed against the Board of Directors, the Executive Officer, and the Association. A certain amount of this nonsense is accepted as normal APA operating procedure, particularly when it comes from the Association's own boards and committees, but the directed and virulent attack from representatives of an outside organization during 1973 was remarkable. The impact was twofold: First, the always precarious morale of the Central Office staff was further undermined, and, second and much more important, an enormous amount of staff time was wasted in assembling materials and data and preparing evaluations and reports. I say wasted because the effort seemed to have no effect in reducing the generation of ill-advised or downright foolish proposals. The staff time spent on such things necessarily had to be diverted from other, more worthwhile activities.

Fortunately, such antics have had little effect on the 80% of Association and Central Office business activities to which I referred earlier. If they had, the Association would have really been in bad shape. The impact has been felt most among the program staff, an impact that was heightened by the work demands of a 35% increase in board and committee meetings in 1973 over 1972.

With that foreword, let me proceed to the several

Offices and their 1973 activities. The Central Office is organized into five major offices: Communications, Programs and Planning, Business Affairs, External Affairs, and the Executive Office. These are described and discussed seriatim.

Office of Communications

This office, the largest of the Central Office units (67 staff members), is divided into three major departments: Publications, Psychological Abstracts Information Services (PAIS), and Publications Development. The Director is Harold Van Cott.

PUBLICATIONS

This department, under the direction of Anita DeVivo, Executive Editor, is responsible for the production of APA's 14 primary journals. In 1973 the journals contained 15,000 text pages, each of which had been carefully redacted for accuracy and format by one of the Association's 17 technical editors. Text pages were up by 1,000 over 1972 and total subscriptions to the journals exceeded 150,000. Such statistics are probably of little interest to people other than editors or publishers; of more importance to APA members is the preparation of a major revision of the *Publication Manual* due out in 1974 and the preparation for the subsectioning of the *Journal of Experimental Psychology*. The last was described in some detail by Arthur Melton in the January 1974 issues of the *APA Monitor* (p. 3) and the *American Psychologist* (pp. 65–66).

PSYCHOLOGICAL ABSTRACTS INFORMATION SERVICES

The activities of this department, under the direction of Robert Kinkade, Executive Editor, are the publication of *Psychological Abstracts* (*PA*) and the provision of products and services derived from the *PA* tapes. In 1973 we provided 24,407 abstracts on 5,000 pages to over 8,000 subscribers. Starting in 1973 the *PA* indexing system was increased from 800 terms to 3,000, thus making location of documents in *PA* much easier. As a spin-off of the new *PA* classification and indexing systems, a *Thesaurus of Psychological Terms*, a valuable reference tool for all psychologists, will be produced. This should be available to the public in mid-1974. A new PAIS product was issued in 1973: three-year cumulative author and subject indexes to *PA*. These were previously produced by G. K. Hall under a royalty arrangement, but will now be published directly by APA at three-year intervals at a substantially increased net profit.

The year 1973 also saw continuation of the transfer to APA of formerly contracted services for the publication of *PA* magnetic tapes. By 1974 all *PA* composition will be done in-house with our own computer at considerable savings.

Three interrelated PAIS tape services—PATELL, PADAT, and PASAR—provide access to the *PA* file from 1967 to the present from remote terminals (PADAT), by mailed search requests processed at APA (PASAR), or by search of the *PA* tape file under lease of that file (PATELL). By the end of 1973, 32 organizations had access to the *PA* file through remote terminals; PASAR requests averaged 20 per week over the year, increasing each month during the year; and leases of *PA* tapes (PATELL) had been negotiated with two foreign countries and five United States universities and research organizations.

Despite the expansion of PAIS during the year, in 1973 for the first time *PA* showed a surplus after many years of deficit grant support. The new products and services derived from the *PA* computer tape file also showed a modest surplus at the year's end. The Psychological Abstract Information Services are now among the most advanced and useful abstracting and indexing systems in science and technology.

PUBLICATIONS DEVELOPMENT

This department, with Elliot Siegel, Manager, has as its major activities the production of separates (books and pamphlets published by APA); the Journal Supplement Abstract Service; and the management of all royalties, permissions to reprint, and other republishing arrangements. Five separates, including the *Convention Program* and the *Proceedings*, were published in 1973 with the *Psychology of Adult Development and Aging* being—for APA—a best-seller.

The Journal Supplement Abstract Service (JSAS) is an experimental publication service featuring on-demand dissemination of separately bound copies of original psychology materials. The manuscripts are reviewed by a 10-member editorial panel, and abstracts of accepted articles are published in the quarterly *Catalog of Selected Documents in Psychology*. Started in 1972, JSAS had 1,400 subscribers and had received 4,200 document orders at

the end of October 1973, an increase of 100% over the previous year. Two APA divisions, 19 (Military) and 21 (Engineering), have adopted JSAS as their official publication source, and for 1975 Division 21 has approved a member assessment to cover the cost of subscriptions to the JSAS abstracts. Optional subscription to JSAS was listed for the first time on the 1974 member dues bill, and a thorough evaluation of its use and finances is planned in 1974. With the advent of machines that will reproduce copies of documents from microfiche at low cost (JSAS documents are now photocopied for users) expected at any time, JSAS may well become a prototype for publications systems of the future.

During 1973 all APA activities concerned with the secondary use, distribution, and sale of previously published APA journal articles were combined into a single program. This program encompasses the potential sale and distribution of single articles and collections of articles as reprints, the granting of copyright permission authorization for the one-time use of single articles, the permission authorization for the one-time use of single articles, and the negotiation of long-term contractual agreements that assign rights to republish materials (by run, volume, year, or article). One development in this area has been the renegotiation of contracts that allow organizations to sell microform and microfiche editions of APA journals and JSAS documents. Now, for the first time, anything that APA publishes in serial form can be obtained on film, with suppliers paying APA royalties ranging from 20% to 25% of revenues. Another development is a plan for an APA reprint service. Initially, this service would make available on demand reprints of articles appearing in the *American Psychologist*. If successful, reprints from other journals may be added. A properly designed reprint service would provide single copies for individual scholarly users and multiple copies for classroom and group use, thereby reducing the temptation to copy illegally.

The Central Office had faced the 1973 communications program with trepidation. Twelve years of research and development grant funding for a National Information System for Psychology (NISP) came to an end in 1972. Deficit grant support for *Psychological Abstracts* averaging $100,000 a year for a decade also ended. New products and services, such as the Journal Supplement Abstract Service and the *Psychological Abstracts* computer-based services, were still too new for their success to be predictable. Cuts in library budgets, paper shortages, and rising costs were expected to affect even such traditional products as the journals. Controversy over NISP and its Experimental Publication System still echoed in some ears. And while much had been done to plan and complete several elements of NISP, other important tasks awaited completion. Thus, we had financial, political, and technical reasons for worry. Similarly, the effect of the decision to eliminate the dues credit for journals was an unknown factor lurking on the 1974 horizon.

As the year ended, however, it found the program in reasonably good shape, and loss of revenue for 1974 from elimination of the journal credit only slightly exceeded projection. The years 1974 and following are undoubtedly going to have their problems, two of which are almost upon us. The first is the ongoing consideration being given by Congress to revision of the copyright laws and recent court decisions permitting almost unlimited free duplication of published scientific and technical articles. The communications program produces close to $100,000 in royalties and reprint payments, an amount that may be in serious jeopardy.

As paper shortages continue and prices rise, and as printing costs mount, the exploration of alternatives to our present methods of journal production has become essential. To give you some idea of our dependence on this material, if you stacked all copies of the journals, the abstracts, the *APA Monitor*, and the separates we have produced one on top of the other, you would have a column over six miles high. As a first step in addressing this problem, cost comparison studies are being made by staff between hot and cold type methods of printing, and the journal staff has taken preparatory orientation training in photocomposition—a method widely used to publish encyclopedias, telephone books, and *Psychological Abstracts*.

Regardless of the techniques to be adopted in the future to combat rising production costs, paper shortages may ultimately force APA, as well as other publishers, to use nonpaper materials for information storage and dissemination. One benign solution would be to print less on paper and to use microform for extensive data tabulations, minor studies, and backup information.

Office of Business Affairs

The Office of Business Affairs, the second largest of the four offices, consists of six departments and a

total staff of 55. The Director of the Office is Boris E. Cherney. The several departments are discussed below.

MEMBERSHIP

This department is under the direction of Jane Hildreth, who has been with the Central Office since its inception in 1946; it has, in addition to Jane, three other staff members. The department processes all applications for membership in APA; applications for Students in Psychology, High School Teacher Affiliates, and Foreign Affiliates; changes in membership status; and requests for division membership. It also handles the thousands of pieces of correspondence each year from members and affiliates. In 1973, the department processed 3,164 applications for membership in APA, resulting in 2,907 individuals being elected for 1974, the largest number in APA history. For comparison, in 1972, 2,655 new members were elected. At the end of 1973 there were 35,000 members, 7,000 students, 750 foreign affiliates, and 550 high school teacher affiliates—in all, a grand total of about 43,000 persons.

CONVENTION

This department is headed by Candace Won, Assistant for Convention Management, and is a one-person department. She is responsible for the logistics aspects of the annual convention, coordinates the requirements of divisions for hours and meeting rooms, and works closely with the Convention Manager, Theodore Driscoll, and the staff of Driscoll and Associates. In addition, Candace Won processes advance registration, prepares the annual *Call for Papers*, and assists the APA divisions in developing the program. In 1973, the convention was held in Montreal, Quebec, Canada, and was attended by at least 19,000 persons, 12,500 of whom registered, with 6,500 persons accompanying the registrants. For contrast, the number of persons attending the 1970 (Miami Beach), 1971 (Washington, D.C.), and 1972 (Honolulu) conventions was about 13,000, 15,000, and 11,000, respectively. In each case, about two thirds of the attendees were registered. The 1974 convention is scheduled for New Orleans and is expected to attract about 15,000–17,000 persons.

PLACEMENT

This department is headed by James Beauter and has one additional staff member. Responsibilities include publication of the monthly *Employment Bulletin* and the conduct of the placement service at the annual convention. Currently there are 3,800 subscriptions to the *Bulletin*, which, for the 12 issues in 1973, listed 884 positions open for recruitment and 224 availabilities of persons for employment. At the 1973 Montreal Convention, there were 164 employers present and another 141 employers who had listings in the *Annual Convention Placement Bulletin*. Together the employers were recruiting for 574 positions while 521 applicants were seeking positions. In addition to the APA placement service, the department supports regional associations by preparing the *Regional Placement Bulletin* used by those associations in the conduct of their local placement operations.

ADMINISTRATIVE SERVICES

This department is headed by William W. Carpenter, who is assisted by 16 other staff members. Their functions include support of the APA governance system, mainly the Council of Representatives and the Board of Directors, during preparation for, the course of, and the mopping up after meetings. It also handles hotel reservations for all board and committee members attending meetings in Washington, D.C., maintains a central repository for minutes of all APA board and committee meetings, and is responsible for the regular updating of the Rules of Council. The department also provides support for the Central Office staff by making purchases, maintaining an office supplies room, handling incoming and outgoing mail, running a print shop, and in general being responsible for all housekeeping functions including property management of the APA headquarters building for both APA itself and its tenants.

A few statistics: The number of impressions produced in the print shop in 1973 was 3,800,000 (adding another quarter of a mile of paper to the stack of paper) in contrast to 3,700,000 in 1972; the number of board and committee meetings serviced was 150 in 1973 versus 111 in 1972; the number of *bags* of mail received was 1,124 in 1973 versus 1,108 in 1972; and the number of bags of mail shipped out was 2,415 in 1973 versus 2,232 in 1972. We mailed 125,000 copies of nonperiodical publications in 1973 versus 114,000 in 1972; and, finally, our postage bill for correspondence and materials shipped out went up to $60,000 in 1973 compared to $52,000 in 1972.

Such statistics as I have just cited may seem

trivial and indeed they are—taken in isolation. The staff uses them as trend indicators of Association activity, particularly that of members of the governing structure. When the percentage increases in such measures and starts diverging excessively from the percentage of increase of revenues over expenses, we are in trouble. Unfortunately, this is what has been occurring during the past three years.

FINANCIAL SERVICES

This department is under the direction of the Controller, Charles L. McKay, assisted by seven other staff members. It maintains and develops the financial systems and records of the Association as needed to protect the assets of the organization and provides timely financial reports, budgets, forecasts, projections, and other financial statements and data required by the management. During 1973, the department cashed 100,000 checks and wrote 11,000. By contrast, in 1972 the department cashed 87,000 checks and prepared 10,000 disbursement checks. During 1974, there are plans to further computerize operations in a continuing effort to mechanize the accounting system.

PUBLICATIONS BUSINESS AND COMPUTER SERVICES

This department is headed by Forrest Mullins and consists of the two divisions described below.

The Publication Business Services Division consists of 10 employees and is primarily responsible for processing publication orders received from members and nonmembers. The department also promotes and handles all advertising carried in APA journals. Several of the more significant factors contributing to the department's 1973 workload were the receipt of approximately 5,800 letters of inquiry and other member correspondence during 1973 versus 5,000 received and answered in 1972; 2,710 advertising orders processed in 1973 as compared with 2,380 in 1972; and total invoices prepared in 1973 of 9,450 versus 8,900 in 1972.

The Computer Services Division, managed by William Rodgers with a staff of nine other people, underwent a transition during 1973 which has greatly improved and increased our internal computer capabilities. A Burroughs B-2500 System with four tape drives and a large capacity for disk storage and retrieval was installed to replace a less sophisticated card-processing computer. As mentioned earlier, we are now, therefore, performing certain work for Psychological Abstracts and Manpower Resources formerly done by outside contractors. The result is a net savings to APA. Additional computer upgrading features will be installed in early 1974 which we expect will result in even greater economies. For information, this division keypunched and verified 604,000 cards in 1973 compared to 506,000 in 1972; produced 1,990,000 mailing labels for journal subscribers in 1973 versus 1,810,000 in 1972; and produced 3,533,000 labels for sale versus 3,182,000 in 1972. The last item produced $80,000 in revenues in 1973 against $65,000 in 1972.

Office of Programs and Planning

Within the Office of Programs and Planning are the five departments that represent the greatest proportion of the nonpublishing, nonsupport activity of the Association. Although I say nonpublication activity, the staff of Programs and Planning was responsible in varying degrees in 1973 for putting together the *Biographical Directory;* the *Consolidated Roster;* the *Graduate Study* booklet; the *Ethics of Human Research* booklet; several publications primarily for high school and two-year college teachers; at least five newsletters; and a sizable number of reports in the *APA Monitor*, the *American Psychologist*, and the Journal Supplement Abstracts Service. Work is currently proceeding on a revision of the *Career* booklet, a booklet on test standards, and several other information pieces. The Office consists of the five departments described below and has as its Director C. Alan Boneau, who is assisted by 32 additional staff members.

EDUCATIONAL AFFAIRS

The focal areas of activity in Educational Affairs are precollege psychology, the teaching of psychology in two-year colleges, graduate programs, professional training, continuing education, and accreditation of training programs. It is headed by J. R. Nazzaro with Ronald Kurz as Associate Administrative Officer.

In precollege psychology the staff has concentrated in 1973 on projects to provide materials for teachers, and two major outcomes of this effort can be reported to you at this time. The first is the publication of the *Psychology Teacher's Resource Book: First Course*, an updated and revised version of the previous publication, *Program on the Teaching of Psychology in the Secondary Schools*. A sec-

ond activity concerned with materials for teachers involves the preparation and submission of a grant proposal to the National Science Foundation for the development of curricular materials. A good deal of staff time this year was spent in negotiation with NSF on the details of the project, the grant, $150,000 for the first year, was received on January 18, 1974. In precollege psychology, the staff has also been concerned with standards for training in certification of behavioral science teachers and has been engaged in data-gathering efforts involving surveys of teachers and colleges. The Educational Affairs staff established a system of liaison with state psychological associations which to date has over 30 individuals coordinating precollege psychology activities at the state level. A newsletter had been initiated to exchange information among them.

In the area of the two-year colleges, the staff has surveyed nearly 4,000 two-year college psychology instructors in an attempt to determine needs of these individuals that could be met by the APA. Again it appears that the Association's role will be to coordinate state activities because the primary felt need of two-year college teachers was for locally sponsored workshops.

In the area of psychology in the graduate schools, the booklet *Graduate Study* has been published under a new format that provides considerably more standardized information to students about programs, program requirements, and program expectations. This information also proved highly useful to the participants at the Vail Conference. The department also conducted its annual enrollment, degree, and graduate employment surveys and is now handling the annual faculty salary survey formerly conducted by the National Council of Chairmen of Psychology Graduate Departments. The staff is currently working on standardization of application and recommendation forms as a possible step toward systematizing the application and selection procedure for graduate schools.

A significant portion of the department staff activities was the administrative arrangements for meetings and visits. Viewing the accreditation program in this fashion, 44 visits were arranged, 14 applications processed; 236 reports received, analyzed, and prepared for review by the Committee on Accreditation. The department also administers the Visiting Scientist Program, now APA funded, and arranged 73 visits to small colleges, including 13 visits by minority consultants to predominantly minority institutions. For the Visiting Psychologist Program providing consultation to training agencies, the staff solicited applications from more than 5,000 agencies, received 116 completed applications, and arranged for visits to 39 of them. The Educational Affairs staff also carried out the administrative arrangements of the Vail Conference with its 140 participants and also participated in structuring the conference, in staffing the meeting, and in following up on the correspondence and the conference report activities after the conference itself.

SCIENTIFIC AFFAIRS

This department, with Miriam Kelty as Administrative Officer, has been, during 1973, concerned primarily with science policy, the ethics of research, population, psychological testing as applied to employment and educational selection, and research funding. In the area of science policy, the staff monitors developments within the federal government and reports these to the Association membership. The reports, in 1973, included anticipated changes in the peer review system for grants, the implications of New Federalism for psychology, and the reorganization of DHEW.

In the ethics of research, the Scientific Affairs Department has provided the administrative staff for the activities of the Committee on the Conduct of Psychological Research with Human Participants and has arranged the preparation and dissemination of the report of that committee. It will be available this year as an APA separate. The staff also has disseminated proposed regulations and legislation dealing with human experimentation to knowledgeable segments of the membership, collated their responses, and submitted to Congress and DHEW compendia on views of psychologists with regard to proposed restrictions of research with human subjects.

In psychological testing, the Department of Scientific Affairs' staff has suffered, along with the Committee on Psychological Tests, through the development of the revised manual *Standards for Educational and Psychological Tests*. It conducted three public hearings on the document to solicit opinions of the draft and collected and collated the disparate views for the Committee. Also in the general area of employee selection procedures, the staff of the department provided technical consultation to the Equal Employment Opportunity Coordinating Council in its drafting a document on employee selection and arranged and promoted a

public hearing for psychologists to comment on the draft.

With respect to research funds, the staff has maintained communication with several of the federal agencies, reporting anticipated policy changes, and arranged three staff seminars with the research grants staff of the National Institute of Mental Health, the National Science Foundation, and the Department of Defense. They also conducted a survey for the Board of Scientific Affairs of a small number of graduate departments to determine the effects on those departments of the reductions in research support. The results, however, were inconclusive. In addition, more than 400 foundations were surveyed to explore their actual or potential sources of support for psychological research and/or training. These activities in conjunction with similar activities of the Educational Affairs Office provide within Central Office a major clearinghouse function dealing with information about funding for psychological research and training.

PROFESSIONAL AFFAIRS

Professional Affairs has been headed since last April by Arthur Centor with Gottlieb Simon as Associate Administrative Officer. The major areas of concentration in 1973 have been state association affairs, health profession matters, legislative and governmental affairs, standards for service, and several special projects. For the state associations, the effort has been to provide information as a basis for activity in newly emerging and critical areas of psychology. Working with Public Affairs, the staff started early in 1973 to develop an awareness within state associations of the implications of the New Federalism and has maintained a continuous liaison through a Central Office newsletter aimed primarily at those associations. A series of information packets were prepared describing the New Federalism philosophy and ways in which state associations could mobilize themselves to find different sources of support for training, research and service. In health affairs, Professional Affairs staff support for the Committee on Health Insurance included production of a new and enlarged edition of a health insurance information packet and participation in negotiation meetings with the insurance industry and Blue Cross/Blue Shield. The department has also been involved in drafting guidelines for professional standard review committees and a national network of state-sponsored professional standard review committees. In addition, they have been directly involved in helping to define and organize the National Registry of Health Service Providers in Psychology, coordinating the concerns of APA with those of the other interested participating groups.

A considerable amount of staff effort was taken up by APA's project to develop standards for service facilities. This NIMH-funded project has Arthur Centor as its principal investigator. He also serves in the same role for the Office of Child Development-supported project to provide psychologist consultants to each DHEW region on the delivery of mental health services in local Head Start programs.

One rather unusual project of Professional Affairs this past summer was an attempt to assess professional psychological activity in the public interest and the factors that promote such activity. In this project, college students interviewed professional psychologists in the Washington, D.C., area. A report and set of recommendations are currently being written.

SOCIAL AND ETHICAL RESPONSIBILITY

In the Department of Social and Ethical Responsibility, headed by Fred Strassburger, there are two sections. The first is concerned with the ethics program of the Association and involves voluminous correspondence and intricate procedural arrangements. Brenda Gurel, Ethics Officer, and the staff working with the ethics program have been helping the committee revise its rules and procedures. This job, completed this year with the bylaw changes sent to the members, will greatly simplify and streamline the processing of ethics cases. Staff has also conducted ethics workshops and seminars with state groups and has served as consultant to several other associations for which a liaison in matters of ethical conduct is appropriate.

In the area of social responsibility itself, the foci of staff activities have been in the area of women's rights, minority rights, and with a number of special concerns of the Board of Social and Ethical Responsibility for Psychology. The department completed a survey of department chairpersons of psychology and is currently tabulating the results and preparing a report on the status of women and minorities on psychology faculties. It also completed a file of women psychologists, by specialty, for use by the Committee on the Status of Women in Psychology in suggesting journal reviewers and nominations for boards and committees. An addi-

tional activity in the area is an informal clearinghouse handling requests for information on qualified women job applicants and on equal employment opportunity problems and programs for women. The staff also organized a symposium of interest to women in psychology for the Montreal Convention.

The Social and Ethical Responsibility Department in cooperation with Educational Affairs developed and administered the program of consultants for minority affairs to colleges. It was a special part of the Visiting Scientist Program. The year 1973 also saw a continuation of the vendors project and the development of an APA affirmative action plan. Finally, there has been considerable participation by the Central Office staff in the activities of the Board of Social and Ethical Responsibility, particularly with respect to the concern about prisoners of war.

MANPOWER RESOURCES

The Manpower Resources Department, headed by Thomas Willette, has, as its name implies, responsibility for the design and development of the APA Manpower Data System. In 1937 the 46,000 questionnaire records received from the 1972 Survey were processed and converted into a computerized data file. Using this file, the Manpower Resources staff devised specifications and supervised the efforts of commercial firms in producing the 1973 *Biographical Directory* and the *Consolidated Roster for Psychology*, both of which were published during the past six months.

A major task for the staff has been the design and testing of software, for maintaining and updating the file, for editing records, for statistical tabulations, and for search and retrieval work. As a result of this, the data base is now a major source of information about psychologists and their activities. Several general tabulations of the data have been already made, and a number of analyses and studies have been planned and are now in various stages of completion. The questionnaire is now being redesigned for mailing in 1974 to produce a revised, updated, and corrected 1975 *Biographical Directory*.

Office of External Affairs

This office, the next to the smallest of the Central Office units (10 staff members), is divided into three departments: Public Information, International Relations, and Public Affairs. Departmental distinctions tend to become blurred, however, because of the small size of the staff. The Director of the Office is Harley O. Preston.

PUBLIC INFORMATION

The director of this department is APA's Public Information Officer, James F. Warren, who also serves as the editor of the *APA Monitor*. The *APA Monitor* has continued to grow in size and circulation faster than predicted. In 1973 its paid circulation reached 43,700 subscribers, exclusive of a sizable free distribution to public officials, representatives of the various news media, and give-away copies. There were four 16-page issues, two 20-page issues, three 24-page issues, and one 32-page issue. The Convention issue consisted of 40 pages, with approximately 20 pages of paid advertisements. Innovations in 1973 included the publishing of pro–con articles on certain selected psychological controversies, extended and in-depth feature articles on governmental programs or proposals, and the use of separate supplements to disseminate drafts of major APA reports for member comments and criticisms.

The Public Information Department received and processed 162 entries for the 1973 National Media Awards Program of the American Psychological Foundation. It also conducted seminars for members of the working press at two regional meetings and at the Annual Convention in Montreal. The purpose of these seminars is not to generate "spot" news—this is done through the APA Press Room also conducted by this department—but to promote more accurate reporting of psychological information by science writers for the daily press, magazines, and television.

The department continued to send special press releases monthly to collegiate and weekly newspapers as well as issuing regular press releases on major happenings involving the APA and individual psychologists. It also continued its practice of hiring a journalism intern for the summer months. The 1973 intern was a student from the Kansas State University School of Journalism.

INTERNATIONAL RELATIONS

The Director of External Affairs also acts as the head of this department. Much of the activity during the first part of 1973 dealt with continuing activities from the 1972 International Congress of Psychology in Tokyo, and commendation was received from the National Science Foundation on

the APA's formal report of this important international meeting. Other activities have included the opening of contacts with psychologists and educators in the People's Republic of China, increasing interest and participation of American psychologists in the social science programs of UNESCO, recruitment of faculty members for universities in developing countries, and providing staff support to APA official representatives to the International Union of Psychological Sciences and to the U.S. National Commission for UNESCO.

The past year also began APA's organizational affiliation with the American Council of Learned Societies. It is too soon to report any results from this affiliation, but it should mean increased support for international activities for psychologists working in the history of psychology, philosophical psychology, and psychology in the arts.

PUBLIC AFFAIRS

This department is directed by Harry R. Seymour, who also serves as principal adviser to the Executive Officer and Central Office staff on APA's legislative activities.

During 1973 the department prepared (or had prepared) and distributed to the APA membership and/or units of APA (divisions, state associations, or APA governing units) the following 14 special reports:

1. An analysis of the Nixon Administration's 1974 Budget and Program Plans, with emphasis on implications for psychology.
2. A summary analysis (similar to No. 1) for distribution to all members of APA governance, boards, committees, and heads of academic departments.
3. A Federal Budget and Program article for the April 1973 issue of the *APA Monitor*.
4. A summary report on actions taken by APA to offset the effects of the 1974 Federal Budget and Program cuts.
5. An analysis of 20 National Health Insurance legislative proposals.
6. An analysis of proposed legislation on aging.
7. An analysis of proposed legislation on vocational rehabilitation.
8. An analysis of legislation extending the Public Health Services Act (S. 1136).
9. An analysis of legislation on student assistance.
10. An analysis of revenue sharing under the New Federalism.
11. Facts on Medicare and Medicaid.
12. Major health legislation in the 93rd Congress.
13. Facts and projecttions on professional standards review organizations.
14. An analysis of state associations' and divisions' legislative activities and interests—for the Committee on Legislative Affairs (COLA).

These reports either appeared as feature articles in the *APA Monitor* or were mailed directly to appropriate members of the Association's governing units.

In addition, the department prepared the regular monthly "Legislative Notebook" for the *APA Monitor* and provided staff liaison and logistic support to the transitory ad hoc Committee on Legislative Affairs and the ad hoc Committee on APA–CAPPS Relations, preparing the appropriate background or summary reports as required.

The department also organized and conducted the Fourth Annual APA Conference on Public Policy for Psychologists just prior to the Montreal Convention and either singly or with a Program Administrative Officer represented APA at numerous meetings of numerous other associations or coalitions thereof, including the National Association of Mental Health, the American Psychiatric Association, the Coalition for Health Funding, the Coalition for Mental Health Funding, the National Council of Community Mental Health Centers, and the Child Development Consortium. A leadership role was taken, with NAMH, in a state coalition effort which was instrumental in the extension of 12 health service acts in fiscal year 1974.

In a similar fashion, either directly or in a supporting role, the department provided APA representation in meetings with various Executive Branch officials, including the Deputy Secretary and Assistant Secretaries of DHEW, and officials of the Social and Rehabilitative Service, the Social Security Administration, and the National Institute of Mental Health. Discussions of APA interests were held with staff of individual legislators and committees and with members of Congress including Senator Hartke, Senator McClelland, Congressman Waldie, Congressman Ullman, Congressman Staggers, Congressman Burke, etc.

Presentations on psychology and government were also made in various forums, for example, Indiana Psychological Association, West Virginia Psy-

chological Association, Brookings Institution Seminars, and APA Convention programs.

A final note on External Affairs: Through its three departments and common administrative section, it handles most of the written and oral inquiries received by the APA from the general public. The number of letters rose substantially in 1973 and the sheer bulk of mail was somewhat overwhelming. Revised procedures for answering such a volume of mail now include greater reliance on referrals to divisions and to offices and departments within the Central Office, and on greater use of informational brochures. Personal replies, however, remained the most frequent chosen response to concerns and questions of the general public about psychology and the APA. In addition to its correspondence responsibilities, in 1973 External Affairs arranged public exhibits in the Central Office on the life and works of both Titchener and Wundt and a large APA exhibit at the Montreal Convention and arranged for APA representation at inaugural ceremonies for 12 college or university presidents.

Executive Office

The last and smallest office is that of the Executive Officer. Its staff consists of myself as Executive Officer, a secretary, the Personnel Officer, Robert Campbell, and a personnel assistant. This staffing description is slightly misleading in that two individuals located in other offices for administrative convenience report directly to and advise the Executive Officer on matters in their areas. These two are Jane Hildreth, Administrative Associate for Membership, and Harry Seymour, Public Affairs Officer. If you read this as reflecting my priorities, you will be more or less accurate. The first priority is the 36,000 members who constitute the real American Psychological Association as contrasted to the pseudo-Association of the Council of Representatives, the Board of Directors, and other governing units and the Central Office. The second priority is the Association's interactions with the world of American citizenry and the faces and positions we present to it, and the third priority is a feeling of responsibility for the approximately 165 employees of the Association.

PERSONNEL

The 1973 Proceedings (this issue, pp. 381–413) reports changes in the senior staff of Central Office. For graded staff there was, during this past year, a 23% turnover—well below par for the Washington, D.C., area. Our percentage of minority members employed now stands at 23%, up only slightly from last year but contrasting well with the 15% of three years ago. Thirty-six percent of the new hires during 1973 have been minority members, but a major problem remains in securing minority and women staff members at the upper management levels. Of 50 inquiries in response to an advertisement of professional staff positions in Central Office, only 1 was from a member of a minority group despite specific appeals to the Association of Black Psychologists and Psychologists for La Raza.

During 1973 the Central Office affirmative action plan was completed by the Personnel Office, with the assistance of the rest of the Central Office staff. It was praised with faint damns by the Board of Social and Ethical Responsibility for Psychology and with enthusiasm by the Board of Directors. A systematic review of all employee benefits was also made, resulting in a change in life insurance carriers (for a savings of $5,000), the addition of a local health maintenance organization as a provider under the employee health plan, the addition of a cashable option on the retirement plan, the upgrading of the educational assistance program, and the standardization of policies on jury duty and maternity leave.

As for the Executive Officer activity during 1973, it is difficult to describe. The position description mentioned at the beginning of this report includes a variety of functions ranging from making contributions in the Association's name to worthy charities to preparing an annual budget, and also a handy phrase: "Will, directly or by delegation." This is then followed by everything I have reported on up to this point. Within the Central Office, aside from board and committee meetings, my major activity is carrying out the managerial functions involved in a $6,000,000 operation with 167 staff members.

Externally, the major duty is serving as representative and spokesman for the Association in legislative and administrative, public policy, and interdisciplinary matters. Behaviorally, it involves a great deal of correspondence, telephone conversations, and scores of meetings or conferences. With a knowledgeable and competent staff, the legislative and administrative aspects are fairly straightforward: the staff prepares letters, documents, or testimony based on APA policies or positions and

the Executive Officer signs or orally presents the results. Presentations may be before Senate or House Committees or in conferences with legislative staff or administration officials. In almost every case the Executive Officer is accompanied by either an appropriate administrative officer from the staff or the Public Affairs Officer.

Public policy matters are somewhat more difficult because they frequently involve balancing the goals and objectives of the Association (see Article 1 of the Bylaws) and an assessment of the state of knowledge on the topic among psychologists with an apparent social good. It requires heavy reliance on the expertise of the membership as well as stated policies of the Association. It also necessitates holding one's disciplinary chauvinism in check.

The interdisciplinary interactions are the simplest and most enjoyable, involving as they do interactions with colleagues from other scientific and professional associations and agreements on common courses of action for mutual good.

Final Comment

That was the year that was for Central Office. With all its frustrations and turmoil it is now behind us and the staff is now gazing with cautious optimism at 1974. In my discussion of the several programs there was frequent reference to projects underway and the problems foreseen. I hope in my report to this body in 1975 I can announce all those projects completed and the problems resolved, but I doubt it.

Report of the Treasurer

ROBERT PERLOFF *Graduate School of Business, University of Pittsburgh*

This year's report can only begin with an expression of deepest gratitude to William McGehee for his outstanding performance as APA Treasurer for five years. In December 1973, due to health considerations, he found it necessary to resign as our Treasurer. Fortunately, however, and with characteristic commitment to APA, Bill has agreed to serve on the Finance Committee this year, enabling us to achieve an orderly transition.

This report will highlight the critical aspects of our financial state, while future reports will seek prospectively to share with you ideas and philosophies and to solicit your ideas concerning the financial problems of and hopes for the Association.

For the second straight year the Association absorbed a deficit in 1972. The preceding Treasurer's report (McGehee, 1973) predicted that "1972 will probably end the year with a deficit somewhere near $125,000 [p. 293]." The year 1972 actually ended with revenues of $5,387,891, expenses of $5,508,229, and a deficit of $120,338. Table 1 shows the main classifications of revenues and expenses for 1971 and 1972. Table 2 provides 1972 data by major program. A glimpse at APA's financial history for the last 22 years is furnished in Table 3, revealing that since 1968 APA's financial picture has been, at best, marginal and, realistically, deteriorating. Table 3 also demonstrates that the Association's expenses tend to double every five years. Of course, this growth is due partially to inflation, but it reflects, more importantly, expansion of APA itself as its membership widens and their demands increase.

Financial Condition

As stated in some prior Treasurers' reports, APA's financial condition (Table 4) appears very "healthy" as of December 31 of each year. For the most part, this state of affairs exists for two reasons: (*a*) A financial condition is representative of a point in time, and (*b*) the major part of the cash and marketable securities as of December 31 is temporarily held in custody as a result of advance payments of dues and subscriptions. These advance

TABLE 1

Revenues, Expenses, and Net Surplus (Deficit) for 1971 and 1972

	Year ending December 31	
	1972	1971
Revenues		
Member dues and fees (net of journal credit)	$1,033,794	$ 831,720
Journal subscriptions	2,131,036	2,026,155
Advertising	200,816	199,534
Other journal revenue	223,304	186,085
Sales of other publications and services	415,257	374,586
Registration fees	64,975	73,578
Exhibit space rental	33,718	53,600
Accreditation fees	69,300	71,600
Investments	89,441	82,884
Building revenue (non-APA tenants)	299,552	299,150
Grants and contracts	717,743	992,995
Other revenue	108,955	94,071
Total revenues	$5,387,891	$5,286,018
Expenses		
Personnel	$2,068,591	$2,021,674
Printing and mailing of publications	1,454,925	1,419,932
Editor's stipends and office expenses	216,044	182,948
Professional, consulting, and contractual services	334,125	316,924
Travel (other than boards and committees)	142,998	93,370
Boards and committees	192,779	185,726
Convention and placement local site expenses	43,246	57,070
Supplies, postage, telephone, other common office expenses	340,307	357,933
Building expenses	552,779	530,400
Other expenses	162,435	142,028
Total expenses	$5,508,229	$5,308,005
Net surplus (deficit)	$(120,338)	$(21,987)

TABLE 2

Revenues, Expenses, and Net Surplus (Deficit) by Major Program for January 1–December 31, 1972

Major program	Revenues	Expenses	Net surplus (deficit)
Services and activities (nonpublishing)	$1,392,792	$1,556,762	($163,970)
Communications (publications and biographical services)	2,903,486	2,925,671	(22,185)
Building operations (non-APA tenants only)	299,552	299,552	-0-
Grants and contracts	619,757	619,757	-0-
Other (investments, mailing labels, other)	172,304	106,487	65,817
Total	$5,387,891	$5,508,229	($120,338)

TABLE 3

Revenues, Expenses, and Surplus (Deficits) for 1951–1972

Year	Revenues	Expenses	Surplus (deficit)	Surplus (deficit) as % of expenses
1951[a]	$ 287	$ 300	(13)	(4.3)
1952[a]	318	289	29	10.0
1953[a]	377	373	4	1.1
1954[a]	400	409	(9)	(2.2)
1955[a]	437	467	(30)	(6.3)
1956	524	489	35	7.2
1957	736	710	26	3.7
1958	865	792	73	9.2
1959	854	824	30	3.7
1960	977	914	63	6.9
1961	1,275	1,165	110	9.4
1962	1,603	1,582	21	1.3
1963[b]	1,615	1,668	(53)	(3.2)
1964	1,642	1,602	40	2.5
1965	2,406	2,271	135	6.0
1966	2,850	2,661	189	7.1
1967	2,983	2,836	147	5.2
1968	3,528	3,485	43	1.2
1969	4,235	4,197	38	.9
1970	5,053	5,041	12	.2
1971	5,286	5,308	(22)	(.4)
1972	5,388	5,508	(120)	(2.2)
Total	$43,639	$42,891	748	1.7

Note. Numbers are given in thousands of dollars.
[a] Does not reflect grant activity.
[b] Does not reflect capital gain of $148,858 on sale of property located at Sixteenth and O Streets, Washington, D.C.

TABLE 4

Comparative Statements of Financial Condition as of December 31, 1971 and 1972

	As of December 31	
	1972	1971
Assets		
Cash and marketable securities	$2,767,521	$2,425,781
Accounts receivable and prepaid expenses	481,087	335,056
Other	99,236	97,274
Land, building, equipment (net)	3,502,842	3,550,811
Total assets	$6,850,686	$6,408,922
Liabilities		
Accounts payable	$ 684,638	$ 536,238
Building loan	2,253,296	2,329,799
Deferred income	2,782,917	2,290,764
Other	13,163	15,112
Total liabilities	$5,734,014	$5,171,912
Net worth	$1,116,672	$1,237,010
Total liabilities and net worth	$6,850,686	$6,408,922

payments, held as of December 31 of any given year, are shown in Table 4 as deferred income, but are consumed during the subsequent year's operations as the goods and services (for which advance payments were made) are delivered.

To illustrate the effect of the $120,338 deficit in 1972 on APA's net worth, one has merely to glance at the net worth (Table 4) at the end of 1971 and 1972. As shown in Table 4, the Association's net worth declined by $120,338 (the 1972 deficit), from $1,237,010 at the end of 1971 to $1,116,672 at the end of 1972. If this declination rate were to persist each year for the next 10 years, our net worth would be zero. The Association's net worth is essentially invested in nonliquid assets, namely, the headquarters building, therefore, it is not available to pay current bills. However, as stated above, the annual cycle of advance payment of dues and subscriptions provides the liquidity needed to pay current bills even in deficit years, provided these deficits are not excessive and not continuing year after year. Deficits cannot be absorbed indefinitely

[illegible]

A Word about [illegible]

[illegible]

- [illegible]

- [illegible]

[illegible] Budget

[illegible]

A Word about the Future

[illegible] **but you can be assured that it will be [illegible] than necessary**

REFERENCE

[illegible]

Proceedings of the American Psychological Association, Incorporated, for the Year 1973

Minutes of the Annual Meeting and Special Meeting of the Council of Representatives

Special Meeting, March 30–April 1, 1973, Washington, D.C.
Annual Meeting, August 27 and 30, 1973, Montreal, Quebec, Canada
and January 18–20, 1974, Washington, D.C.

WILBERT J. MCKEACHIE, *Secretary, Michigan*

These proceedings constitute the official record of actions of the Association taken during the year both by the [illegible] and by the Board of Directors. The Annual Meeting of Council was held in accordance with Article III, Section 2, of the APA Bylaws. The council reports in the notes for the minutes the state of research on the psychology and education of children exposed to lead, existing research and proposed steps to meet this crisis, and the general nature of the APA Council report [illegible] Issues considered by vote of Council on the minutes of this meeting are reported as part of these minutes [illegible]

The Council of Representatives was called at each meeting [illegible] Nine [illegible] were introduced in these sessions. Reports were made on various reports, some of which will be considered elsewhere. Copies of these reports were [illegible] and are on file in the Council Office. As long as the reports were [illegible] copies of the reports are available from the Council Office.

The Board of Directors met seven times during the Association year. All substantive actions of the Board were [illegible] and are recorded in these minutes. Minutes of the Board's actions are the responsibility of the secretary of each meeting.

As in previous years, these minutes are organized in several sections than chronological order. The major headings used are:

I. Minutes of Meetings in [illegible]
II. Elections, Awards, Memorials, and Motions
III. Action and Information Requirements
IV. Reports of Officers
V. Resolutions and Legislative Recommendations
VI. Organization of the APA
VII. [illegible]
VIII. [illegible]
IX. [illegible]
X. [illegible]
XI. [illegible]
XII. [illegible]
XIII. [illegible]
XIV. [illegible]
XV. [illegible]

I. MINUTES OF MEETINGS IN [illegible]

A [illegible] approved the minutes of the Annual Business and Meeting minutes of the [illegible] as published in the American Psychologist [illegible] pp. [illegible]

A [illegible] approved the minutes of the meeting of the Board of [illegible] Meeting, March [illegible] [illegible] Meeting, August [illegible] [illegible] Meeting, January [illegible] [illegible]

The Board approved the [illegible] minutes of the [illegible] and not on the [illegible] and [illegible]

II. ELECTIONS, AWARDS, MEMORIALS, AND MOTIONS

A [illegible] approved the results of the [illegible] elections as follows:

[illegible] and work in [illegible] and [illegible]

[illegible] Council [illegible] William McCracken [illegible] work done during his tenure as Recording Secretary.

[illegible] Council voted the following statement of appreciation [illegible] William McCracken who had to resign his office as Treasurer because of illness.

The Council of Representatives voted unanimously to express the appreciation to William McCracken for his outstanding service as Treasurer from 1968 to 1971. Having been granted three years of sabbatical [illegible] the Council [illegible] and persons to dealing with the problems and he [illegible] efforts which involved personal risk to his health. He [illegible] the future good health [illegible]

[illegible] Council voted the following [illegible] of the Board of Directors [illegible]

[illegible] Council [illegible] the consideration and election of members of Council to [illegible] with both carry out all the activities previously performed by the Recording Secretary.

[illegible] adopted the following change to Rule of Council [illegible]

[illegible] The results of the election to the Board of Directors shall be reported to the Council of Representatives within [illegible] the regular [illegible]

[illegible] Rule [illegible] would become new Rule [illegible]

[illegible] voting each member of Council to vote for as many candidates as there are offices to be filled.

Old Rules could be remembered accordingly.

[illegible] Under New Business at the meeting on January [illegible] voted to elections by Council for [illegible] the boards and committees. The motions were [illegible] Meeting in New Orleans.

[illegible] the following amendment to Rule [illegible] The APA Bylaws be submitted to the membership.*

[illegible]

* [illegible] copy of the resolution was presented to the [illegible]

* Parliament reported to added.

[illegible]

[illegible] Council [illegible] the following

[illegible]

By mail ballot of October 10, 1973, Council modified the above action to read in part:[3] "and for officers other than the President-elect *and the Executive Officer* . . . ." The Committee on Structure and Function of Council was charged with preparing the necessary changes in the Bylaws and Rules of Council for consideration by Council at its January 1974 meeting. At its January 1974 meeting, Council voted that the following amendments to the APA Bylaws be submitted to the membership and the following revisions be made in Rules of Council:[1]

1. Article VII, Section 8.

The Recording Secretary shall be a Member of the Association, elected by the Council of Representatives, following nomination by the [Board of Directors] *Nominations and Elections Committee of Council* . . . .

2. Article VII, Section 9.

The Treasurer of the Association shall be a Member of the Association, elected by the Council of Representatives, following nomination by the [Board of Directors] *Nominations and Elections Committee of Council* . . . .

3. Article X, Section 1, third sentence (the section concerns election to boards and committees).

The [Board of Directors] *Nominations and Elections Committee of Council* [may nominate a single person for not more than one-third of the positions on any standing board or committee and the Board] shall nominate at least two persons for [at least two-thirds of the positions] *each position to be filled.*

4. New Rule of Council.

10.9.1. There shall be a Nominations and Elections Committee of Council that shall make nominations for (1) officers of the Association other than the President-elect and the Executive Officer, and (2) vacancies on standing boards and committees of the Association reporting directly to the Board of Directors and the Council of Representatives but excluding the Nominations and Elections Committee. The Committee shall consist of eight members, six of whom are members of Council at the time of election and elected by Council. The six Council members shall not be members of the Board of Directors at the time of their election. The President-elect with vote and the Executive Officer without vote shall also serve on the Committee. The six members elected by Council shall serve terms of three years, so staggered that two will be elected each year.

5. Revised Rule of Council.

30-6.1. Nominations for the various offices [to be filled by election of the Council] *that are elected by Council, other than the Board of Directors and the Nominations and Elections Committee,* shall be the final responsibility of the [Board of Directors] *Nominations and Elections Committee.* However, the nominations for members of committees reporting through any [other] Board *other than the Board of Directors and Council of Representatives* shall in the first instance be the responsibility of that Board. The Executive Officer shall call for suggestions of persons to serve on boards and committees from officers of divisions and state associations, and from chairmen of boards and committees. The resulting list of names shall be available to *the Nominations and Elections Committee and to* the various boards when they are preparing nominations.

6. New Rule of Council.

30-6.9. Nominations to the Nominations and Elections Committee shall be by mail ballot distributed to members of Council. Each Council member will be allowed to nominate two candidates for each vacancy. Three persons will be nominated for each vacancy chosen in order of total nominations received.

Present Rules of Council would be renumbered accordingly.

J. On August 28, during the Convention, the Committee on Scientific Awards announced the APA Awards for Distinguished Scientific Contribution to Lee J. Cronbach, Brenda Milner, and Benton J. Underwood. The American Psychological Foundation's Gold Medal Award for 1973 was given to Harry F. Harlow. The Foundation's awards for Distinguished Contributions to Education in Psychology were presented to James E. Maas and Frank Joseph McGuigan. The Foundation's National Media Award was given to Gerald Jonas, for his two-part series entitled "Visceral Learning" which appeared in the *New Yorker* magazine. David Wechsler received APA's Distinguished Professional Contribution Award. Conrad L. Kraft was named the first winner of the Distinguished Contribution for Applications in Psychology Award.

K. The following report of membership changes during the year is presented as part of the report of the Membership Committee.

1. The Board of Directors elected 1,929 new Members and 977 new Associates, as of January 1, 1974, whose names were reported in the *American Psychologist,* 1974, **29**, 135–145. In addition, 346 Associates were transferred to Member status as of January 1, 1974 (*American Psychologist,* 1974, **29**, 146–147).

2. The Association was informed of the deaths of the following 158 members during the period November 10, 1972, through November 30, 1973:

Albert J. Aaronson
Henry F. Adams
Herbert M. Alexander
Roy N. Anderson
David Angell
Clairette P. Armstrong
Dennis J. Arp
Adeline E. Babbitt
Phyllis Bartelme
Peter A. Bates
Romney O. Beckman
Helen M. Beeler
William W. Biddle
Herbert G. Birch
Matilda A. Birnbaum
Carolyn A. Blackmer
Lillian Blake
Wesley P. Booman
Dean R. Brimhall
Kenneth H. Brookshire
Mary C. Burch
Harold W. Burk
Dallas E. Buzby
L. Wayne Campbell
Sam Lewis Campbell
Leonard Carmichael
Dwight W. Chapman, Jr.
Augusta S. Clay
Joseph A. Cobb
Rena D. Collier
Edgar E. Daniels
Charlotte del Solar
Roy A. Doty
Charles I. Doyle
G. Carey Drakeford
Linda Kay Dreisbach
Ralph E. Dunford
Shirley Elbert

Erwin A. Esper
Alfred Farau
Stanley L. Fong
Lula B. Ford
Benjamin S. Gantz
Iva C. Gardner
Henry E. Garrett
John G. Gavazzi
Leon Geisler
Billie D. Gibbons
Haim G. Ginott
Clarence M. Gittens
Wallace Gobetz
Irving B. Goodman
Warren C. Gott
Earl E. Graham
Searles A. Grossman
Lenore S. Gruen
Ray C. Hackman
C. Larry Hagen
Dorothy Kern Hallowell
Robin H. Harris
B. P. Gill Helring
Robert L. Hobson
Clementina K. Hollenberg
Jules D. Holzberg
Max Horkheimer
Earl J. Janda
Bill L. Kell
Mabel C. Kemmerer
Thomas W. Kennelly
Grace H. Kent
Newell C. Kephart
Rafi Z. Khan
Forrest A. Kingsbury
John E. Klingensmith
William H. Knapp
Herbert J. Larson
Darrell E. Latham
James N. Lauber
David A. Leach
Daniel S. Lehrman
Gloria Ladieu Leviton
Louis W. Lewellyn
Don Lewis
Bernard H. Light
Edward A. Lincoln
Thayne M. Livesay
Herbert E. Lucy
Anita F. Lyons
Philip D. Maimon
Thomas P. Makres
Elizabeth M. Maloney
John H. Mariano
Donald G. Marquis
Helen Matthews
Peter R. Mattis
Robert A. McCleary
Mary McGrath
John T. Metcalf
Kenneth L. Minor
Elizabeth F. Moller
George L. Morris
Lowell J. A. Nelson
Edna R. Oswalt
Llewelyn A. Owen
Emile B. Painton
Helen K. Pancerz
Edward T. Parsons
Richard F. Paulson
A. J. Pellettieri
Dorothy C. Perkins
Lois J. Porter
Stanley D. Porteus
Steve Pratt
Samuel J. Prensky
John W. Purcell
Harvey Queen
Gregory Razran
Homer B. Reed
Richard A. Regan
Melvin G. Rigg
John C. Ringwald
Arthur Harold Roberts
Esther Katz Rosen
Roy W. Ross
Mark Rudnick
Robert J. Russell
Mildred W. Saupe
Peter E. Schellenberg
George E. Schlesser
Diana Brody Schoen
Emanuel K. Schwartz
Harry Sherman
Samuel G. Simpson
Janet I. Smith
John Frederick Souza
S. Smith Stevens
Barbara S. Stewart
George P. Stone
Vincent Tempone
Elbert C. Thoroman
J. Edward Todd
Donald T. Tomblen
George C. Turner
Edward S. Underwood
Warren P. Van Pelt
C. Nicholas Vardack
Clara N. Voos
S. Rains Wallace
Melvin H. Weinberg
Sheila B. Weinberg
Livingston Welch
Alexander G. Wesman
Marion L. Williams
Don Winfield
Robert C. Wingfield
Ellen C. Wolf
Jesse Zizmor
Theodore E. Zurett

3. The Board of Directors accepted the resignations of the following 820 members, between November 10, 1972, and November 30, 1973, of which 547 were on the basis of nonpayment of dues:

Robert P. Abelson
Samuel M. Abramson
Martin H. Acker
Madeleine Adamczyk
Diane S. Adams
Joseph J. Adelman
Joel W. Adkins
Freida B. Alderman
Bruce K. Alexander
Florence M. Alexander
Ruth A. Allee
Wayne L. Allee
D. R. Alley
Ann S. Alpern
Frank P. Amedeo
David C. Anderson
John D. Andrews
Rawley R. Angerstein
William A. Anthony
Shifra Arazi
John C. Armstrong
Ross O. Armstrong
Albert J. Aschenbrenner
Sarah H. Ashman
Robert J. Ashmore
William R. Atkinson
Elizabeth S. Backman
Marjorie B. Bahnson
K. Dale Bailey
Robert L. Baird
Thomas J. Banta
Mario Barillas
David J. Barker
Louis B. Barnes
Mary A. Baron
Valerie T. Baron
Malcolm E. Bass
William H. Batchelder
Walcott H. Beatty
Michael F. Bechely
Leland P. Bechtel
Anne G. Beck
Thomas N. Beckish
John R. Beery
John R. Belton
Terry A. Benline
Carson M. Bennett
Werner Berg
Barry D. Berger
Else-Lill Berglund
Jorge E. Berrios
Leander W. Binna
Florence W. Birkemeyer
Helen M. Bishop
Perry Black
Kenneth H. Blood
Richard H. Bloomer
Gerald R. Bodelson
Marc H. Boesen
Ellen A. Bohrer
Jerry N. Boone
John M. Booth
James Carl Borg
Robert Bornstein
Whitfield S. Bourisseau
Max Edward Bowen
Libbie B. Bower
Richard W. Boyd
Richard J. Boylan
William A. Bradley
Dana H. Bramel
Frederick J. Bremner
Bernard J. Breslaw
Lawrence J. Brick
Charles M. Briley
Arthur K. Brintnall
Elizabeth G. Brody
Reginald B. Bromiley
Jack William Bronfeld
Lee R. Brooks
Ronald R. Brotherton
Carl R. Brown
Ellen Brown
Ruth A. Brown
Wade L. Brown
Patrick B. Bruder
Helen N. Brush
Robert N. Bryan
Grace E. Budahn
Ruth M. Buescher
Louise C. Bullock
Henderson G. Burns
Pearl P. Burns
Gary S. Burstein
Blanche O. Bush
Lynn R. Caldwell
Richard F. Canavan
Eda M. Cano
Lillian C. Canter
Robert J. Carlton
Arthur Courtney Caspary
Gerald J. Cecere
Jae-Ho Cha
Jean F. Chambers
Ridgely W. Chambers
Nicholas H. Charney
Mei F. Cheng
Kao-Liang Chow
Paul R. Christensen
Conrad Chyatte
Genevieve T. Clapp
Elmer J. Clark
William C. Clegg
Theodore Clevenger
Dean J. Clyde
Beatrix Cobb
Raymond Cochrane
Kevin A. Cody
Harry B. Cohen
Spurgeon N. Cole
Charles F. Combs
Eleanor S. Connelly
Donald W. Conover
Marcia Cooper
John D. Copp
John D. Corbit
James W. Cozy, Jr.
Morris L. Crawford
Thomas H. Cremer
Barry D. Crites
James G. Crosby
Geza A. Csapo
James V. Cuff
Stephen J. Cummings
Robert P. Cunningham
Marion E. Cupp
Robert H. Curnutt
Jeanette Cutts
John I. Dalland
Joseph L. Daly
Terry C. Daniel
John C. David
Ida F. Davidoff
John R. Davis
Chauncey M. Dayton
John Henry Deagle
Walter L. Deemer
James W. Degan
Lloyd A. Delude
Florence Demont

Anthony S. Desimone
Katherine E. D'Evelyn
Charles M. Devonshire
John E. Dews
James J. Diamond
Fred M. Dickerson
Rose M. Dickson
Kenneth Diven
Gernot S. Doetsch
Gertrude M. Donat
Mary C. Donohue
Robert N. Dorsey
Anna G. Douglas
Elizabeth M. Douvan
Patrick J. Dowd
John Driscott
James A. Duke
John Dan Duke
Dorothy L. Dyer
Kenneth R Earp
Ronald D. Ebinger
John Escodi
Elizabeth M. Eddy
Norma R. Ellington
Betty W. Ellis
Robert Stuart Emmer
Larry G. Engelsen
George W. Ennis
Shirley J. Epir
Werner H. Epp
Virginia B. Erdman
Robert V. Erikson
Betsy Worth Estes
Jonathan F. Esty
Alan L Evans
Maria D Evensen
Avner Falk
Edward J. Fallon
Anita E. Fassler
Gerald W. Faust
Mirlen D. Federer
Charles A. Fenwick
Walter D. Fenz
Richard J. Ferber
William E. Ferinden
Joseph W. Ferrara
Beverly Beryl Ferris
Frank H. Finch
Stephanie F. Fine
Paul Finkel
Nancy Jo Finley
Edward T. Fitzgerald
Warren D. Fitzgerald
John J. Flanagan
Patrick J. Flanigan
John T. Flynn
Mayette L. Ford
Margaret A. Forrest
Peggy D. Forsyth
William Louis Fowler
Joseph J. Franchina
Joseph D. Frankel
Judith P. Frankmann
Wilbur K. Frederickson
Leslie R. Fricke
Ruth Z. Friedkin
Dov Friedlander
Ferdinand J. Fritzky
Abraham Froehlich
Byron E. Fulk
Donald E. Fuller
Ruthanne J. Funk
Mercedes Gaffron
Eligio S. Gallegos
Francis C. Gamelin
Harriet Gans
Lewis Gantwerk
Leo Ganz
John Gardner
Merl J. Gaskill
Jean Gaudreau
William O. Gay
Dorothy F. Gehrke
Peter J. Geiwitz
Luther C. Gilbert
Ray W. Gilbert
Lloyd Gilden
Eugene Giles
Henry B. Gillen
Ury M. Gluskinos
Bernardine Goebel
Alfred M. Goetschius
Alan D. Goldberg
Mary Goldworth
Byron Neil Golie
Daniel M. Goodacre
Helen D. Gordon
Felix C. Gotschalk
Barbara Steffens Graham
Daniel W. Graham
Grace Cone Grantham
Judith A. Gray
Karen M. Green
Leonard R. Green
Mary J. Green
Susan Greenberg
Ronald R. Greene
Eugene T. Grembowicz
Loren Grey
Sheila K. Grgin
Robert E. Griffin
Jesse W. Grimes
John O. Grimmett
David Earl Groom
James W. Guidmond
Elizabeth C. Guy
Kenneth U. Hackney
Nancy K. Hagerty
John Edward Halvotson
John J. Hanitchak
Harley M. Hanson
Maury M. Haraway
Gherry E. Harding
Curtis D. Hardyck
Pat G. Harkins
Grady E. Harlan
Irene E. Harms
John J. Harold
Esther M. Harrison
Gary L. Hart
Susan Harter
Ethel N. Hartzler
Roscoe W. Harward
David G. Hays
Grace M. Heider
Martin I. Heinstein
Raymond H. Henjum
Emily W. Herbert
Audell Herndon
Austin C. Herschberger
Leonard Hersher
Alice B. Hiat
John David Higgins
Donald C Hildum
Arthur H. Hill
Eirene R. Hill
Helen L. Hill
Wayne H. Hill
Carole B. Hoadley
Gerald J. Hodson
Norma Hoffman
Thomas P. Hogan
David K. Hogberg
Wayne B. Holder
Regis Holland
Frank J. Holmes
Fred K. Honigman
Cornelius M. Horgan
Bernard Horowitz
William H. Hossick
Herbert W. Houghton
Dorothy Houseman
Donald E. Hovey
Kay Howard
John F. Howell
Dorothy K. Howerton
Donald P. Hoyt
James A. Huddy
Archie E. Hudson
George P. Huff
John F. Hurst
John A. Hylton
Bernard Hyman
George A. Hyry
Marion W. Irwin
Adelaide Jablonsky
Lawrence W. Jaffe
Harry M. Jagoda
Roland G. Jarka
Charlene D. Jarvis
Joseph Robert Jenkins
Lucille Jenks
Mary Anne L. Jerome
William M. Job
Autrey B. Johnson
Jacqueline R. Johnson
Lucille I. Johnson
Yvonne E. Johnson
Bill Frank Jones
Donal S. Jones
Marvin C. Jones
Robert L. Jones
Clay Jorgensen
Dorothy S. Jorstad
Stanley Joseph
Walter C. Joseph
Purushottam Joshi
Dennis M. Joy
Francine D. Juhasz
Dale W. Kaess
Hugh R. Kailan
Peter K. Kaiser
Gillray L. Kandel
Lawrence E. Kanous
Henry S. Kao
Lia Kapelis
Bert Kaplan
Val E. Karan
Arnold E. Kaufman
Thomas Kavazanjian
Karl E. Keefer
Mary S. Kelbley
Mary J. Kelley
Patrick J. Kelly
William B. Kenworthy
John P. Kernan
Virginia N. Kerr
Joseph E. Kilgore
Daniel P. Kimble
Frank S. King
John A. King
John D. King
Phillip Kingsley
Donald F. Kirby
Franklin D. Kirby
Forrest H. Kirkpatrick
Mary V. Klavon
Bernard Klein
Joseph Kleinman
James Aaron Kline
Linda V. Kline
Alan E. Knoll
Gerald L. Knutson
Jane Q. Koenig
Richard A. Komm
William E. Konrad
Leonard B. Kornreich
Peter L. Kosof
Margaret L. Kramer
Paul E. Kreider
Mary A. Krider
Sunnan Kubose
Solomon Kugelmass
Rondeau G. Laffitte, Jr.
James T. Laird
Richard A. Lake
Donald Wayne Lamb
T. K. Landauer
Edward Landy
Robert D. Lane
William J. Lanier
Ruth E. Latin
Jeanne A. La Torre
Louis Lauro
Robert J. Lavallee
Viola W. Leahy
Robert E. Lecher
Bruce T. Leckart
Fred T. Lee
William M. Lee
F. F. Lefever
Shirley L. Lemeshnik
Eugene A. Levitt
Joel J. Levitz
David M. Levy
Dorothy G. Lewis
James N. Lewis
J. C. R. Licklider
Howard W. Light
Daniel R. Lilie
James B. Lingwall
Ruth A. Lipsky
James D. Lisle
Martin S. Livingston
Thomas R. Long
Charles L. Longueil
Leo A. Lorenzo
William M. Lothrop
Doman Lum
Robert C. Lumpkin
Elizabeth L. Lundborg
Dean R. Lusienski
Barbara Lynn
Grayson C. MacAndrew

Harriet K. MacConnell
James M. MacDougall
Howard L. Madden
George Magakis, Jr.
S. Adam Maher
Michael J. Maley
John B. Mallonee
Marguerite Malm
Helge H. Mansson
Marilyn M. Mantay
Robert L. Marchbanks
Lillian A. Marcus
Charles J. Marino
Norman N. Markel
Robert L. Marrone
Nancy K. Mason
Leonard Matin
August J. Mauser
David M. McArthur
Frederick J. McDonald
Ruth L. McDonough
Thomas C. McFadden
Kathryn G. McGrath
John M. McGrew
Catherine Gorshe McGuinnes
Boyd J. McKelvain
Clarence L. McKelvie
Hugh S. McKenzie
Cameron K. McKinley
Kenneth R. McKinnon
Gerald W. McLaughlin
John P. McLaughlin
Samuel C. McLaughlin
Helen B. McMurray
O. Joy McNamara
Theodore T. McNelly
Flora Marie Meredith
Barton P. Meyers
Frank Meyers
Perry C. Meyers
Robert E. Meyers
Herbert J. Michelson
David T. Miles
Frank E. Milhollan
Carroll H. Miller
Helen Elizabeth Miller
James O. Miller
La Rue H. Miller
Paula E. Miller
Gary D. Mitchell
John J. Mitchell
Wilfred M. Mitchell
Audrey U. Mivelaz
Howard Moltz
Gary T. Montgomery
Anita L. Mooney
Thomas J. Moore
Donald F. Moores
William F. Moorhouse
Rose B. Moran
R. E. Morgenstern
Charles J. Morris
Eugene Morris
John B. Morris
Olive Mortison
John R. Morton
William T. Moss
Carol S. Motanky
Guy U. Motanky
Kenneth S. Moxey
Antony J. Mullaney
Sandra O. Mullenaner
Kay C. Murray
Joan Barbara Muser
C. Leonard Muskin
Dennis R. Musselman
Marilyn Mutchnik
Mary A. Nagel
Sylvain Nagler
Thomas J. Nagy
M. Mike Nawas
Daniel C. Neale
Ted Nelson
Allan Netick
Myrne B. Nevison
Parley W. Newman
Donald G. Newton
John R. Nichols
Stanley W. Niehaus
Willa Norris
Robert B. O'Brien
William J. O'Brien
William Leo O'Connell, Jr.
John Richard O'Connor
Virgil J. O'Connor
Clifford R. O'Donnell
James E. Oliver
Edwin H. Olson
David George O'Neil
Jacob S. Orleans
Michael E. Orme
John R. Ostanski
Wesley H. Osterberg
Robert F. Ostermann
Leon S. Otis
Cynthia S. Otten
Wayne R. Otto
Marion C. Outhit
Robert L. Overing
William B. Oxford
Elliott H. Palefsky
Francisco Palomo
Elpis D. Patrinakou
Mary A. Pecoraro
James D. Pecsok
Duilio T. Pedrini
Fred E. Peifley
Bruce H. Peppin
Charles T. Perkins
Sanford H. Pesner
Cameron R. Peterson
Helen S. Peterson
Lewis F. Petrinovich
Luella E. Pettigrew
Jack C. Pflugrath
Jane Phillips
Keith D. Pierce
James D. Pietrangeli
Gene G. Pittman
Rod Plotnik
Lawrence P. Podgorski
Dorothy M. Pollock
Charles D. Polson
Dorothy L. Postman
Juanita T. Powell
Robert F. Powloski
Micha S. Prather
Bruno Preilowski
Joan L. Prentice
Janet R. Pressman
Paul Prevetta
Arnold Projansky
Thomas D. Prutsman
Lawrence A. Pugh
Richard T. Putney
Peter Rabe
Sam Rabinovitch
Lee Rainwater
Robert L. Raisler
James G. Ramsay
Bernard D. Rapaport
Rowena T. Rathbone
Rollo R. Rathfelder
Arthur S. Reber
Percy C. Reed
Gerald M. Reicher
Gary W. Reighard
Ismar Reiter
Michael Reiter
Leeta T. Renwick
Robert H. Ressler
Brenda D. Reynolds
J. B. Reynolds
Berna F. Richards
Orville R. Richardson
Ellen Ann Ripish
Robert J. Ripke
Sheilia Ripke
Benbow F. Ritchie
Carl J. Roach
Sally A. Robinson
Alec Rodger
Bert A. Roens
Jack B Rollins
Ruta M. Rose
Ronald Rosen
Kenneth M. Rosenberg
Milton J. Rosenberg
Richard B. Ross
Bernard Roth
Martin M. Rothman
Rona L. Roydes
Carl I. Rubinroit
Billy F. Russell
Frank C. Russell
Herbert E. Ryan
Maurice Rybowski
Sam Saltzer
Rex Samuel
David Sandler
Frank A. Saunders
Robert M. Savidge
Vernon H. Schaefer
Richard S Scheideman
Harold Schiffman
Frederick M Schiro
Dale R. Schissler
Geraldine A. Schmidt
Phyllis W. Schmidt
Jerome M. Schneck
Terrence E. Schomburg
Martin B. Schrank
Robert F. Schuckert
Carol A. Schulman
Ruthann M. Schwartz
Don A. Schweitzer
Julius Segal
Elaine L. Selby
Lucien R. Sellet
Cord B. Sengstake
Ronald G. Setterington
Ronald L. Sexton
Amnon Shai
Sydelle S. Shapiro
Barry Sharon
Bert L. Sharp
Charles A. Sharp
Marion F. Shaycoft
Richard W. Shearman
Muriel I. Sheldon
Marian B. Sherman
Charles P. Shimp
Miriam L. Shrifte
Paul L. Shriver
Stephen A. Shry
Denis J. Shumate
Albert W. Silver
Hyman Silver
Anthony W. Silvestro
Roger I. Simon
O. Suthern Sims
James R. Skeen
Theodore M. Skolnik
Lionel Slater
Ralph J. Slattery
Alec J. Slivinske
Anne Mary Smith
Beverly V. Smith
Catherine W. Smith
Edgar A. Smith
Virginia J. Smith
Peer Soelberg
Francis L. Sonday
Kenneth C. Spatz
Saul M. Sprung
Roger W. Squier
L. Alan Sroufe
Rudolf H. Stahlberg
Joel Stark
Sidney Stark
James E. Stary
Glenn F. Stauffer
Russell G. Stauffer
Barry Edward Stein
Jacob J. Stein
Leonard Steinberg
Mary E. Stephenson
Clara B. Stewart
Glen F. Stice
Ruth T. Storey
Peter H. Strudwick
Everett W. Stude
Ruth B. Suggs
Roger Raymond Sundling
Emeliza Swain
Don L. Swickard
Murphy J. Sylvest
Emmi Szorenyi
Hasan Tan
Charles T. Tart
Sandra T. Tatem
Frank W. Taylor
H. Augustus Taylor
Leslie R. Taylor
Marianne Teich
Peter H. Ten Eyck
James S. Terwilliger
Karl E. Thaller
Donald B. Thomas
Dorothy M. Thompson
Joseph B. Thompson
Kenneth F. Thomson
Gerald B. Thornton

John B. Thurmond
Richard V. Thysell
Henry Tjahjono
Sheldon S. Tobin
Karen P. Todd
Jerome J. Tognoli
James T. Townsend
Frederick W. Trabold
Leroy E. Train
Wonderful A. Trembly
Thomas J. Triggs
Roland G. Troemel
Paul F. Trudeau
Margaret P. Trujillo
James B. Trump
James J. Tschudy
Donald A. Tucker
James A. Turman
Frederick T. Tyler
George E. Uhlig
Nancy Ann Unwin
Muriel Uprichard
William O. Uraneck
Richard Harold Usher
Ruth W. Vanmeter
Charles J. Versacci
Frank J. Vilardo
Jack Per Vognsen
Anton C. Voorhoeve
Thomas Vris
David Wachstock
Muriel W. Wagner
Carole C. Wahler
Hardy L. Wahlgren
Merlin W. Wahlstrom
E. Lynn Waldrip
Susan R. Walen
Gene A. Wallar
Gary C. Walters
Louis L. Walters
Kenneth L. Wapnick
L. Scott Ward
David V. Warren
William V. Watkins
Donivan J. Watley
Donald T. Watson
John S. Watson
Jean C. Watts
W. Dwight Webb
Frances R. Webster
Paul L. Weene
Gerald M. Wehmer
George D. Weigel
Claire G. Weinrebe
Allan R. Weinstock
Morton W. Weir
Norman T. Welford
Robert A. Wendorf
Regina H. Westcott
Robert J. Wherry
Charles M. Whipple
Albert J. Whitehurst
James R. Whitman
Charles H. Wickersham
David A. Wicklund
Randall J. Widener
Marvel J. A. Widmayer
William M. Wiest
Charles F. Williams
Charles H. D. Williams
Charlotte L. Williams
David L. Wilson
Grace O. Wilson
Erik K. Winslow
Harvey D. Winston
Robert S. Witte
Jerry C. Wofford
Morton K. Wolf
Maxine Wolfe
Annelary G. Wolff
Michael Wolff
John E. Wood
Lila M. Wood
James R. Woodward
Richard L. Woolston
Murray S. Work
Richard E. Worthington
Carroll J. Wright
John H. Wright
Keith C. Wright
Donald H. Wykoff
Louise Wylan
George A. Xydis
Roy S. Yamahiro
Martin Yanis
Nicholas Yarmoshuk
Francis A. Yeandel
Shu-jen Huang Yen
Mary B. Yorton
George C. Zahl
Allen C. Zechowy
Glenn D. Zohner
Laszlo L. Zukmann

4. The following 39 members have been reinstated:

Sylvia Altman
Marcella V. Baldwin
Albert B. Blankenship
Angelo Bolea
Cecil W. Burns
John M. Butler
Eugene Cash
Augusta S. Clay
Evelyn B. Darrell
William R. Dodson
Cornelia A. Doty
Veronika Engelhardt-Grimm
Margaret F. Estep
John H. Feamster
Adam Fikso
Homer L. Gammill
Edith S. Gasser
William E. Gaymon
Mymon Goldstein
Frank J. Gorman
Edward J. Green
Ruth B. Haber
Walter R. Hill
Ralph R. Hoepfner
Rochelle K. Kainer
Lorne M. Kendall
Robert D. Kerns
Charles A. Kluge
Robert F. Kronemeyer
Leonard A. Licht
Norman M. Locke
Marti Meyer
Henry Curtis Patey
Stanton D. Plattor
Herbert W. Sammons
Florence Seaman
Lee B. Sechrest
Bernard D. Starr
Mary E. Wickman

5. The following 174 Members and Fellows have been transferred to dues-exempt status:

Leon P. Addis
Frances S. Alexander
Doris T. Allen
Robert M. Allen
Thelma S. Alper
Charlotte H. Altman
Louise Bates Ames
Anne Anastasi
Alice F. Angyal
Albert D. Annis
Rowena R. Ansbacher
Helen Appeldoorn
Dorothy M. Barrett
Hubert P. Beck
Garret L. Bergen
Constance C. Berry
Sidney W. Bijou
Reign H. Bittner
Emerson F. Blodgett
Merl E. Bonney
H. D. Bouman
Robert L. Brigden
Richard B. Brooks
Thelma E. Brown
C. G. Browne
Robert H. Bruce
Wendell R. Carlson
Wilton P. Chase
William K. C. Chen
Weston R. Clark
Walter V. Clarke
Beulah C. Compton
Quin F. Curtis
Neil M. Daniels
Robert N. Davenport
Paul C. Davis
Bonnye Deal
Frank A. DePhillips
Florence Diamond
David H. Dingilian
Joseph F. Downing
Charles I. Doyle
Wendell S. Dysinger
Louise Omwake Eckerson
George L. Fahey
Leona Mae Failor
Miriam Faries
Joseph Feuerburgh
Glen Finch
Warren G. Findley
Charles P. Fonda
George Forlano
M. J. Freeman
Homer L. Gammill
John V. Gilmore
Henriette T. Glatzer
Tom J. Gorham
Florence E. Gray
Paul C. Greene
Dorothy Park Griffin
Elizabeth P. Hagman
Calvin S. Hall
Sidney L. Halperin
Forrest S. Hamilton
Roy M. Hamlin
Kathryn L. Hattell
LaBerta Hattwick
Melvin S. Hattwick
Richard H. Henneman
Harry W. Hepner
Doris N. Hess
Joseph C. Heston
Vivian H. Hewer
Conwell D. Higgins
Edgar J. Hinkel
Jacob L. Hirning
Marshal S. Hiskey
Francis W. Irwin
Nathan Israeli
Louise Snyder Johnson
Donald E. Kempton
Ruth C. Kimball
Franklin H. Knower
Morris Kransdorf
Philip E. Kraus
Charles R. Langmuir
George Lapidus
C. H. Lawshe
Nettie H. Ledwith
Mary M. Lee-Street
Lawrence J. Lennon
Rensis Likert
Donah B. Lithauer
Norman M. Locke
G. Townsend Lodge
Edith Lord
John C. G. Loring
James L. MacKay
Harry C. Mahan
I. Leon Maizlish
Glenn C. Martin
Edmund P. Marx
Frieda H. McCollom
Mary T. McDade
Ross McFarland
William McGehee
William B. McKean
Fred McKinney
Walter J. McNamara
Arthur W. Melton
Ramona Messerschmidt
Jack E. Meyers
Gregory A. Miller
Edward A. Monaghan
Paul G. Murphy
Lester N. Myer
Helen Nahm
Helen M. Nelson
Theodore M. Newcomb
Ralph W. Ogan
Jay L. Otis
H. Robert Otness

Helen Pallister
Leighton R. Palmerton
L. A. Pennington
Frances C. Perce
Keith J. Perkins
Nicholas H. Pronko
Edward T. Raney
Dorothy Ransom
Lester N. Recktenwald
Harold J. Reed
T. W. Reese
M. Gertrude Reiman
Jesse B. Rhinehart
Glenn W. Rieman
Harry N. Rivlin
Elsa E. Robinson
Peggy Rodgers
Priscilla Ayres Rodgers
H. Rogosin
Verity M. Ross
Howard L. Roy
Audrey S. Schumacher
William C. Schwarzbek
Pauline S. Sears
Robert R. Sears
Louise B. Seronsy
Ernest L. V. Shelley
Bennett Shimp
Mary Louise Shire
Elsa M. Siipola
B. F. Skinner
Harry W. Smallenburg
George Horsley Smith
Richard T. Sollenberger
George S. Speer
Ralph B. Spence
Chalmers L. Stacey
Irving R. Stone
Herman N. Stuart
George R. Thornton
Adrian Tieleman
William M. Treverton
Alfred F. Trout
Hans Wallach
Frederick A. Whitehouse
Walter H. Wilke
Edith Wladkowsky
Ralph R. Wolf
Beatrice N. Wolfson
Paul Woodring
Asahel D. Woodruff
J. Lewis Yager

6. Council elected the following 94 Members to the status of Fellow, effective January 1, 1974, on nomination of the divisions listed:

NAME	DIVISION
Bernard W. Agranoff	Physiological and Comparative
J. Stanley Ahmann	Educational
Murray Alpert	Clinical
William E. Amos	Public Service
Norman H. Anderson	Experimental
F. Elton Ash	Public Service
Paul B. Baltes	Developmental, Adult Development and Aging
Gerald V. Barrett	Industrial and Organizational
Henry B. Biller	Developmental
Sidney J. Blatt	Clinical
Gordon H. Bower	Experimental
William E. Broen, Jr.	Clinical
James H. Bryan	Personality and Social
Leonard S. Cahen	Evaluation and Measurement
Bartell W. Cardon	School
Paul W. Caro	Military
Loretta K. Cass	Clinical
Calvin D. Catterall	School
June E. Chance	Clinical
Jack M. Chinsky	Community
Ira S. Cohen	Teaching
Burns B. Crookston	Counseling
James Henry Davis	Personality and Social
Sanford J. Dean	Clinical
Jan D. Duker	School
Norman S. Endler	Clinical
Alan N. Epstein	Physiological and Comparative
Frank H. Farley	Educational
Myron A. Fischl	Military
Peter G. Fotiu	School
Alan Frankel	Rehabilitation
Morton P. Friedman	Experimental
Graham V. Goddard	Physiological and Comparative
Steven G. Goldstein	Clinical
J. Richard Hackman	Industrial and Organizational
Marian D. Hall	School
J. Thomas Hastings	Evaluation and Measurement
Douglas H. Heath	Clinical
Robert V. Heckel	Clinical
Verda T. Heisler	Consulting
Harrie F. Hess	State Association Affairs
Bartley G. Hoebel	Physiological and Comparative
Durand F. Jacobs	Rehabilitation
Marceline E. Jaques	Rehabilitation
Homer H. Johnson	Personality and Social
Norman Kaplan	Consulting
Bertram P. Karon	Psychotherapy
Edward S. Katkin	Personality and Social
Lloyd Kaufman	Experimental
Benjamin Kleinmuntz	Evaluation and Measurement, Personality and Social, Clinical
Abraham K. Korman	Industrial and Organizational
Merton S. Krause	Evaluation and Measurement
Nadine M. Lambert	School
Eric Levita	Clinical
Robert Lee Linn	Evaluation and Measurement
Howard B. Lyman	State Association Affairs
Pavel Machotka	Psychology of the Arts
Albert P. Maslow	Evaluation and Measurement
Sylvia R. Mayer	Military
Albert Mehrabian	Personality and Social
Jack C. Merwin	Evaluation and Measurement
Lawrence E. Murphy	Teaching
Raymond S. Nickerson	Engineering
Leonard A. Ostlund	Counseling
Nathaniel J. Pallone	Counseling
Harry J. Parker	Rehabilitation
Howard L. Parris	Military
Gerald R. Patterson	Clinical
Donald R. Peterson	Clinical
E. Lakin Phillips	Psychotherapy
Sherman Raffel	State Association Affairs
Julian Rappaport	Community
Martin Reiser	Consulting
Ralph M. Reitan	Clinical
Frank Risch	Public Service
A. Michael Rossi	General
Herman C. Salzberg	Clinical
Gilbert Sax	Evaluation and Measurement, Educational
Joseph M. Scandura	Educational
Max S. Schoeffler	Engineering
William Seeman	Clinical
Jerry Sepinwall	Psychopharmacology
Raymond R. Shrader	State Association Affairs
Gerald H. Shure	Personality and Social
Noel W. Smith	History
George Soloyanis	Public Service
Martin G. Staiman	Clinical
Donald J. Stedman	Developmental
Bonnie R. Strickland	Personality and Social, Clinical
Sigmund Tobias	Educational
Ina C. Uzgiris	Developmental
Allen V. Williams, Jr.	State Association Affairs
George N. Wright	Rehabilitation
Herbert H. Zaretsky	Rehabilitation

7. The following 169 persons, already Fellows of the APA, were elected to Fellow status in the divisions indicated:

NAME	DIVISION
Henry E. Adams	Consulting
Leonard W. Allen	Public Service

Earl A. Alluisi	Experimental, State Association Affairs
William P. Angers	School, Counseling
William H. Angoff	Educational
Philip Ash	State Association Affairs
Robert A. Baker	State Association Affairs
Allan G. Barclay	Teaching
Dorothy M. Barrett	General
Wayne H. Bartz	General
Ernst G. Beier	State Association Affairs
Ledford J. Bischof	General
Edward Blacker	Public Service
Seymour M. Blumenthal	Hypnosis
Hedda Bolgar	State Association Affairs
Stuart Boyd	General
Douglas W. Bray	State Association Affairs
Orville G. Brim	Personality and Social
Timothy C. Brock	General
Lily Brunschwig	General
F. Robert Brush	Physiological and Comparative
James F. Bugental	State Association Affairs
Aaron H. Canter	Psychotherapy
Hilding B. Carlson	General
Rae Carlson	Personality and Social
Peter L. Carlton	General
Henry Chauncey	General
Isidor Chein	State Association Affairs
Thomas Clifford	General
Eugene A. Cogan	Community
Barry E. Collins	Personality and Social
William E. Collins	Physiological and Comparative
Natalie T. Darcy	General
Robyn M. Dawes	General
Joseph G. Dawson	Hypnosis
Florence L. Denmark	State Association Affairs
Paul R. Dingman	General
Michael Dinoff	State Association Affairs
Herbert O. Dörken	State Association Affairs
Roscoe A. Dykman	State Association Affairs
J. Wilbert Edgerton	Public Service, State Association Affairs
Peter D. Eimas	Experimental
Albert Ellis	State Association Affairs
Edwin A. Fleishman	Engineering
Uriel G. Foa	General
Bertram R. Forer	State Association Affairs
Marvin R. Goldfried	Personality and Social
Richard Gonzales	General, Experimental
Leonard V. Gordon	Educational
Philip H. Gray	General
Paul C. Greene	Rehabilitation
Ralph H. Gundlach	State Association Affairs
Alice Gustav	General
Marcia Guttentag	Personality and Social
Florence C. Halpern	State Association Affairs
Frank J. Harris	Military
Frederick H. Heilizer	General
Karl F. Heiser	State Association Affairs
Mary Henle	Psychology and the Arts
Richard H. Henneman	Experimental
William E. Henry	Community
Kenneth F. Herrold	General
Albert N. Hieronymous	Evaluation and Measurement
Ernest R. Hilgard	State Association Affairs
Ronald T. Hill	General
Raymond H. Holden	Developmental
Kenneth I. Howard	Psychotherapy
Raymond G. Hunt	General
Thelma Hunt	State Association Affairs
Ira Iscoe	State Association Affairs
Carroll E. Izard	Personality and Social
Douglas N. Jackson	Clinical
Alfred Jacobs	Clinical
Harry L. Jacobs	General
Arthur R. Jensen	Experimental
Richard Jessor	Clinical
James S. Karslake	General
James G. Kelly	Public Service
Charles A. Knehr	General
Nathan Kogan	General
Joseph T. Kunce	Rehabilitation
Albert K. Kurtz	General
Samuel B. Kutash	State Association Affairs
Luciano L'Abate	General
Howard Leventhal	General
Leon H. Levy	Clinical
Helen B. Lewis	State Association Affairs
Gardner Lindzey	Experimental
Lawrence W. Littig	Personality and Social
Kenneth B. Little	General
Lloyd H. Lofquist	General
Joseph Lyons	General
Solomon Machover	State Association Affairs
Beryce W. MacLennan	Clinical, Community
Joseph M. Madden	General
Salvatore R. Maddi	General
George H. Mahl	Psychotherapy
Melvin Manis	General
Harry V. McNeill	State Association Affairs
Paul W. McReynolds	General
Hyman Meltzer	State Association Affairs
Frieda K. Merry	General
Samuel J. Messick	Developmental
Harold Michal-Smith	State Association Affairs
Kent S. Miller	State Association Affairs
Wilbur Miller	General
Clellan L. Morgan	General
Robert B. Morton	General
Ulric R. Neisser	General
John Robert Newbrough	Clinical
Edward Newbury	General
Joseph M. Notterman	General
Charles E. Osgood	SPSSI
Jay L. Otis	State Association Affairs
Leslie Phillips	Community
Thomas F. A. Plaut	Public Service
Herbert C. Quay	Developmental
Henry D. Remple	State Association Affairs
Jerome H. Resnick	State Association Affairs
Maria Rickers-Ovsiankina	State Association Affairs
Klaus F. Riegel	Developmental
Bernard F. Riess	State Association Affairs
Bernard Rimland	General
Alan H. Roberts	Hypnosis
Robert Rosenthal	Clinical
Vin Rosenthal	State Association Affairs
Duane M. Rumbaugh	Developmental
Salvatore Russo	General
T. A. Ryan	General
Kurt Salzinger	Clinical
Bernard Saper	State Association Affairs
K. Warner Schaie	Evaluation and Measurement
C. Winfield Scott	State Association Affairs
Saleem A. Shah	Clinical
Leo Shatin	Consulting
Lewis J. Sherman	State Association Affairs
Edward Shulman	General
Max Siegel	State Association Affairs
Hirsch L. Silverman	State Association Affairs
Leonard Small	State Association Affairs
J. E. Keith Smith	General
Kendon R. Smith	General

Fred E. Spaner	Philosophical
George Speer	Consulting
Arthur W. Staats	General
Ira M. Steisel	Developmental
Siegfried Streufert	Philosophical
George Stricker	State Association Affairs
Norman D. Sundberg	Community
James T. Tedeschi	Personality and Social
John W. Thibaut	Personality and Social
Ethel Tobach	Physiological and Comparative
David K. Trites	Military
Charles A. Ullman	Consulting
William S. Verplanck	State Association Affairs
Robert Weiss	General
Walter Weiss	General
George A. Wertheim	General
Michael M. Wertheimer	Experimental
Robert G. Weyant	General
Arthur N. Wiens	State Association Affairs
Walter H. Wilke	State Association Affairs
Martha H. Wilson	General
William A. Wilson	General
Logan Wright	Clinical
Wallace H. Wulfeck	State Association Affairs
Frederick Wyatt	Psychotherapy
Ruth C. Wylie	General
Paul C. Young	State Association Affairs

8. The following 50 new Members (M) and 35 new Associates, elected as of January 1, 1973, failed to confirm their election by payment of initial dues:

Barnett Addis (M)
Rene Ahumada (M)
Josephine D. Arasteh (M)
Robert Asch
Frank Michael Bagrash (M)
Isaac Bolarinwa Balogun (M)
Naomi R. Baumgarten
Louis Charles Bernardoni (M)
James Lowe Bothwell (M)
Richard C. Boutwell (M)
Kerry William Bowen (M)
Erica Paul Bressler (M)
Stephen William Brown (M)
Michael P. Browne
Linda C. Budd
Maximo Jose Callao (M)
Ulku Camlibel
Benjamin Carl Carruthers (M)
John Stephen Conklin
Ellis Morgan Craig (M)
Lawrence E. Dettweiler (M)
Michael Patrick Emery (M)
Gail Laurene Ensher (M)
Sally Anne Felker (M)
Douglas Duane Friedrich (M)
James R. Fruge
Richard D. Grant
Frank August Green (M)
William F. Greer, Jr.
Michael O. Gross
Margarita Guzman
Lacy G. Hall (M)
Christa H. B. Hammerling (M)
Berta L. Hanley (M)
Dennis E. Hanley (M)
John H. Harper (M)
Larry Hochhaus (M)
Joyce L. Hoffspiegel
Ralph M. Hogan (M)
Elizabeth R. Hoke
Lee Holland (M)
Lawrence George Hrebiniak (M)
Lawrence Jacobowitz
Barry L. Jacobs (M)
George Willard Johnson (M)
Sharon Ellen Kozich
Thomas Patrick Lechowick
William Curtis Leonard (M)
Jerry Robert Levinson
Karlen Ruth Lyons
Barbara Z. Mallory
Robert David Marx (M)
Marilyn L. B. McConnell
Peter Donald McLean (M)
Philip George McLeod (M)
Roger A. Meisenhelder
Andrew Jackson Mills
Hossein Molavi (M)
William Herbert Moorcroft (M)
Abbas Motahari-Fard (M)
Kathleen Golden Nadeau (M)
Roger Dale Paige
Tullio David Pitassi
Leonard Hugh Reich (M)
Herbert A. Rosefield, Jr.
Dennis Runcie (M)
Joseph Alva Russo (M)
Michael C. Sabo
Marion Anthony Sanchez (M)
Joseph B. Schadeberg
Michael J. Scher
Larry W. Scherwitz
Paula Louise Schiffer
John Scott Scranton (M)
Jeffrie J. Silverberg
Lynn G. Smith
William P. Smotherman
Diana Lynn Solar
Jean Butler Stancil (M)
Robert David Stetten (M)
William F. Tracy
Ruth Eleanor Turner (M)
John Lawrence VanDyke (M)
Joseph H. Vaughn (M)
Benjamin Wallace (M)

L. Upon recommendation of the Membership Committee and the Board of Directors, Council voted that the following amendments to the APA Bylaws be submitted to the membership: [1]

1. Article II, Section 8.

Associates who meet the standards for Member status will, upon application, [automatically] be advanced to Member on the January first next following the *deadline* date of application for such advancement.

2. Article XVIII, Section 7.

Any *Fellow,* Member, *or Associate* who has reached the age of sixty-five and has been a member of the Association for at least twenty-five years, *or, regardless of age or length of membership, who has been adjudged totally and permanently disabled,* shall be exempt from further payment of dues . . . .

If the Bylaws amendment is adopted, Rules of Council would specify that the determination of disability would be by an employer or by the Social Security Administration.

M. Upon recommendation of the Membership Committee, the Board of Directors voted approval for the issuance of certificates to Fellows, showing the year of election. The Board voted, further, that current Fellows be asked whether or not they wish to receive a certificate, and authorized Central Office to establish procedures and prices, if any.

N. Under New Business, at the Council's meeting on January 18–20, 1974, a motion was submitted by the Representative from Division 18 (Public Service) concerning the establishment of a new class of membership for retired members. The motion was referred to the Membership Committee for consideration and recommendation to Council at the 1974 New Orleans meetings.

O. The APA Representative to the Scientific Manpower Commission has for some time reported to Council through the Education and Training Board. That Board felt that it would be more appropriate for the Representative to report through the Board of Directors, inasmuch as the Ad hoc Committee on Human Resources is a committee of the Board of Directors. The Board of Directors concurred, and Council voted that Rule of Council 10-5.2 be amended accordingly.

P. The Board of Directors received a request from the American Association for the Advancement of Science asking that APA consider increasing its contribution to the support of the Scientific Manpower Commission. APA's current contribution is $600. The Board postponed consideration of an increase until

more information is available on plans for the Commission.

Q. At its meeting on January 17, 1974, the Board of Directors voted to accept the recommendation of the Office of Programs and Planning and the Ad hoc Committee on Human Resources that APA publish a 1975 *Biographical Directory*, rather than a 1976 edition as voted earlier. It appeared that revenue from the initial publication of the *Consolidated Roster for Psychology* will not warrant the expense of collecting more nonmember data at this time. There will be a *Membership Register* in 1974. It was recommended further that outside sources of funds be sought to support the future continuation of nonmember data collection and statistical processing, with a study of the feasibility of publishing future consolidated rosters.

R. At its meeting in June 1973, the Board of Directors received an information report on the current status of negotiations with the Eastern Psychological Association and the Midwestern Psychological Association regarding the possibility of APA assistance in placement activities at their regional meetings. EPA found its present operation sufficiently effective, and MPA reported that it was conducting a feasibility study.

III. ETHICS, AND SOCIAL AND ETHICAL RESPONSIBILITY

A. At its meeting in February 1973, the Board of Directors received from the Committee on Scientific and Professional Ethics and Conduct (CSPEC) a series of suggestions for amendments to the Bylaws, the Rules and Procedures of CSPEC, and Rules of Council, designed to permit discretionary communication of information by CSPEC about members against whom charges have been filed. The proposed changes had been checked with Arnold and Porter (APA's counsel) and were unanimously recommended by CSPEC; they provide specifically for referral of complaints and information to state and regional psychological associations; to licensing and certification boards, without request; and for release of formal proceeding results to all concerned parties, for example, complainants, members, state and regional psychological associations, state licensing and certification boards, and the American Board of Professional Psychology. Further, the Bylaws amendments would permit APA committees and divisions to communicate among themselves in cases of alleged ethical violations. As reported under II-A-3 of these Proceedings, the membership approved the Bylaws amendments. Amendments to the Rules of Council are as follows:

1. Add the following sentence to Rule 60-2.5:

This provision does not preclude notification to others as permitted by the Association's Bylaws and CSPEC's Rules and Procedures.

2. Amend Rule 60-3.1 as follows:[1]

In general, the Committee may furnish [upon request by] *to the* agencies listed in Article X, Section 5 of the Bylaws, the following: (a) the nature of the complaint; (b) the ethical principles involved; (c) the findings and recommendation of the Committee. Additional information concerning the evidence may be provided in the event that the requesting agency is itself concerned with violations of ethical principles [and it is able and willing to furnish the Committee with timely information about cases under scrutiny by the agency involving members of the APA. The agency shall also have] *and has* clearly established and acceptable procedures for handling ethical cases.

B. On recommendation of the Committee on Scientific and Professional Ethics and Conduct, the Board of Directors accepted the resignation of an APA member under ethics investigation for violations of the *Ethical Standards of Psychologists*, specifically, Principles 1, 1c, 2, 2a, 7, and 7c, and noted that his reapplication for membership should be subject to the provisions in Article II, Section 19, of the APA Bylaws.

C. At its meeting in May 1973, in a discussion of relations between boards and committees and Central Office, the Board of Directors reviewed the initial experiences of the new Board of Social and Ethical Responsibility for Psychology (BSERP) and its problems in establishing satisfactory working relationships with staff. What is the role of Central Office staff members working with a board or committee? Past Boards of Directors and the present one have maintained the principle that every Central Office staff member works for the Association as a whole and is responsible to the Executive Officer. Yet the staff member assigned as liaison to a board must provide the support it needs as effectively as possible. Problems arise when there is conflict between desires of a board or committee and administrative practices in Central Office or overall policies of Council or the Board of Directors. At times the staff member must be an advocate for his board or committee to the larger organization; at times he must remind the particular unit he assists that his activities are limited by other aspects of the organization.

BSERP is unique with respect to its role as a monitor of internal operations of APA as well as broader social and ethical problems. Moreover, BSERP is by its nature likely to be goading APA to action in areas where BSERP believes APA has been laggard. This places the staff liaison to BSERP in an especially difficult position.

In further discussion of BSERP at its meeting in June 1973, the Board of Directors expressed the view that BSERP should accept the responsibility for the work of committees in its area, as well as consideration of problems and issues referred to it through the usual channels.

D. At its February 1973 meeting, the Board of Directors heard a report on the program to investigate discrimination policies of vendors to APA and voted to request the Executive Officer to communicate our concerns to other Washington-based scientific societies and to explore joint actions. BSERP had not yet developed criteria for what constitutes "essential services," nor substantive criteria for what would constitute compliance with APA's policies, but did report that it felt no doubts about continuing the project, urged collaboration with other associations, and had discussed the possibility of setting up a "test case" using media publicity.

At its meeting in January 1974, Council received an updated report of the APA Vendors' Program and was informed that BSERP, at its August 1973 meeting, had appointed a subcommittee to develop criteria for evaluating the data submitted by vendors.

E. At its meeting in February 1973, the Board of Directors heard a report from members of a subcommittee carrying out the Council mandate to investigate possible political misuse of medical and psychological rehabilitation procedures offered to returning Vietnam Prisoners of War (POWs). The subcommittee met with Pentagon members of the Department of Defense Task Force on Returning Prisoners of War of the Office of the Assistant Secretary of Defense for Health and Environment on February 16, 1973, and was received with friendliness but got little specific information. Plans were made to communicate with the presidents of the American Psychiatric Association, the National Association of Social Workers, and the American Orthopsychiatric Association concerning APA's experience, with the possibility of some kind of joint action if events warranted further endeavors.

At its meeting in November 1973, the Board was informed of a meeting held on November 10, 1973, between the BSERP Ad hoc Committee on the POW Issue and psychologists working for the Center of POW Studies in San Diego, California. The meeting was reported as having been fruitful and eventually cordial, after some initial questions about the implied mistrust of military psychologists resulting from the APA Council's resolution. A final report is expected by mid-1974.

F. Related to the general POW issue, the following resolution was adopted by Council at its January 1974 meeting:

RESOLUTION ON THE REHABILITATION OF YOUNG AMERICANS AFFECTED BY THE VIETNAM WAR

Recognizing that the psychological and moral burdens imposed on U.S. citizens by the war in Indochina—confronting them with profound divisions within their society, with anguish about the morality of actions taken in their names, with distrust of their national leadership, and with doubts about the justification for the sacrifices imposed upon them—weighed most heavily on the young men who were called upon to participate personally in the fighting in Indochina;

That the usual difficulty experienced by the veteran in the process of transition from military to civilian life, due to psychological traumata and other reasons, "had been markedly greater for the Vietnam veteran because of the controversial nature of the Vietnamese conflict and the rapid social-economic changes that occurred during his absence"; [4]

That "studies conducted by the military and the Veterans Administration indicate that serious and prolonged readjustment problems exist in approximately one out of five new veterans, but to a lesser degree, were experienced by all"; [4]

That Vietnam veterans as a group and their families have been receiving insufficient moral, psychological, and emotional support to enable them to come to terms with their experiences, to find employment, and to prepare themselves for the future;

And that many thousands of men who, for reasons of conscience, resisted the draft, or disobeyed military orders, or deserted, are now facing psychological problems associated with separation from their families, exclusion from their societies, and stigmatization as lawbreakers,

1. The Council of Representatives of the American Psychological Association endorses legislative and executive actions leading to:

a. Increased benefits for Vietnam veterans and improvements in the administration of such benefits, in order to assure that the educational, occupational, medical, and psychological needs of these men are adequately met, with real-dollar benefits at levels at least as high as those extended to World War II veterans;

b. A broadened definition of Service-related disabilities, which would give veterans the opportunity, on a wholly voluntary basis, to obtain treatment for psychological problems that do not require hospitalization or that manifest themselves only some time after their return home, and to obtain treatment for members of their families who play a significant role in their readjustment;

c. Freedom of choice for Vietnam veterans in contracting for psychological treatment, allowing them—whether they are still in service or out of service—the option of receiving payment for such treatment by civilian practitioners of their own choosing, if they feel that their needs cannot be adequately met by mental health personnel working within the military or the Veterans Administration; and

d. Active participation of Vietnam era veterans in developing and running the programs designed to serve their needs.

2. Council urges APA divisions and state and local psychological associations to establish registers of appropriately qualified psychologists whose skills in therapy, counseling, group leadership, or other psychological services might be useful in the rehabilitation of Vietnam veterans and war resisters, and who are prepared to devote some portion of their time to work with these men and their families, free of charge or at reduced rates. Such registers should be forwarded to Central Office so that they might be maintained centrally. Psychologists should be urged to participate in these programs, indicating both their skills and their time limitations, with the understanding that the existence of the registers will be publicized among prospective

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[4] Quotations taken from a memorandum from the Department of Medicine and Surgery of the Veterans Administration, reproduced in part in the *Congressional Record* of October 12, 1973.

clients and that inquiries by such clients would periodically be referred to them

3. Council requests that the Board of Social and Ethical Responsibility for Psychology generate recommendations for just and humane policies designed to ease the psychological problems faced by war resisters and to help them reestablish themselves within the society.

4. Council urges relevant divisions, boards, and committees within APA to develop mechanisms and provide occasions for discussing and analyzing the psychological and moral implications of the Vietnam war and its effects on the American population and particularly on the generation most directly confronted with it.

5. Council requests that the APA Central Office and relevant boards and committees take active steps to promote and support legislative and executive actions, as well as activities within the profession, designed to implement the above proposals.

G. Council adopted the following Resolution on the Use of Psychiatric Diagnosis and Hospitalization to Suppress Political Dissent:

The Council of Representatives of the American Psychological Association reaffirms its resolution of December 1972, condemning the practice, wherever it may occur, of suppressing and neutralizing political dissenters by diagnosing them as mentally ill and committing them to mental hospitals. Council considers it the responsibility of individual psychologists to oppose such practices within the organizations in which they are employed and, if they do not succeed in changing the practices, to dissociate themselves from personal complicity in them. To implement this resolution, Council requests:

1. That the APA's representatives to the Assembly of the International Union of Psychological Science (IUPS) request the Executive Committee of the IUPS to place on its own agenda and on the agenda of the next meeting of the IUPS Assembly a resolution condemning the use of psychiatric diagnosis and hospitalization to suppress dissent and a plan to undertake an international survey of the prevalence of this practice,

2. That the President of the APA urge each member organization of the IUPS to express, through its representatives to the IUPS, its own concern with this issue and its interest in placing it on the agenda of the IUPS Executive Committee and Assembly;

3. That the Executive Officer and Board of Directors of APA, through the appropriate units within the APA Central Office and governing structure, gather available information on the prevalence of this practice in U.S. civilian and military organizations and explore the necessity and feasibility of a study within the United States, to be coordinated with a prospective international study; and

4. That the President of APA make contact with the President of the American Psychiatric Association, to inquire about the results of discussions with Soviet psychiatrists on practices in that country and to explore the possibility of joint action, if such is indicated, to protest Soviet practices.

H. Council received a number of information items concerning persecution of intellectuals in Chile and voted the following:

Because of concern for the safety of fellow social scientists and intellectuals in Chile, and because it has been announced that the right of safe passage for refugees will be revoked after the beginning of February 1974, Council asks the Board of Social and Ethical Responsibility for Psychology, through staff, to take the following actions:

1. Make contact with the State Department urging the development of procedures permitting the entry of refugees from Chile into the United States;

2. Establish an ad hoc committee to ask APA members to set up a job bank, working in contact with the Job Clearinghouse of the Association of Latin American Studies, the National Council of Churches, and the American Friends Service Committee. Other professional associations should also be notified and urged to take similar action.

I. At the Council meeting in August 1973, a motion was introduced to establish an ad hoc committee on racism. Council voted to refer this matter to BSERP for recommendations to Council at the January 1974 meeting. The Board of Directors was informed that BSERP had been unable to consider the matter at its meeting in November 1973 but plans to take some action concerning this issue at its first 1974 meeting.

J. Council adopted the following Resolution on Discrimination Based on Social Class:

Recognizing past neglect of the problem of discrimination on the basis of social class, the Council of Representatives of the American Psychological Association affirms its commitment to working for the elimination of social class barriers to educational and employment opportunities in psychology; requests that the Board of Social and Ethical Responsibility for Psychology, in consultation with its Committee on Equal Opportunity in Psychology and other appropriate groups and individuals, propose mechanisms for APA study of and action on this issue to the Board of Directors for consideration at its meeting scheduled for June 28–29, 1974; and authorizes the Board of Directors to implement such proposals at the earliest opportunity.[5]

K. At its meeting in November 1973, the Board of Directors received a communication from the American Association for the Advancement of Science (addressed to Presidents of societies affiliated with AAAS), asking for support of its intent to prepare a statement of concern with respect to the treatment within Russia "of the distinguished physicist, Andrei Sakharov." The BSERP had been asked for its recommendation on the matter and strongly urged support. The Board of Directors voted to join with AAAS in its action, and our representatives to the AAAS were so informed.

L. At the meeting of the Board of Directors in August 1973, President Tyler reported on conversations she had had at the Vail Conference concerning APA responsibilities to minorities and the possibility of APA's funding of modest requests for help. The Board expressed its sympathetic understanding of the problem and its willingness to consider promptly any requests

[5] This resolution stemmed from consideration of a memorandum addressed to Council from David M. Gray, entitled "A First Step in Eliminating Social Class Oppression," which had been referred to BSERP and the Committee on Equal Opportunity earlier.

for such funds recommended by the BSERP. The latter Board was encouraged also to consider such requests in an expedient fashion.

M. Upon recommendation of the Ad hoc Committee on the Status of Women in Psychology and the Board of Directors, Council at its August 1973 meeting voted to adopt the following new Rule of Council 60-13.1, establishing a continuing Committee on Women in Psychology, with the members of the Ad hoc Committee to be retained through 1974 as members of the continuing committee:

60-13.1. There shall be a Committee on Women in Psychology which shall concern itself with furthering the major purpose of the Association—to advance psychology as a science and as a means of promoting human welfare—by ensuring that women achieve equality as members of the psychological community, in order that all human resources be fully actualized. Its mission shall be that of functioning as a catalyst, by means of interacting with and making recommendations to the various parts of the Association's governing structure, to the Association's membership, and particularly to the Division of Psychology of Women, as well as to other relevant groups, such as the Association of Women in Psychology, the Federation of Organizations for Professional Women, and the National Council of Chairmen of Graduate Departments of Psychology. Specifically, the Committee will undertake the following priority tasks:

- The collection of information and documentation concerning the status of women;
- The continued development of recommendations and implementation of guidelines;
- The development of mechanisms to increase the participation of women in roles and functions of the profession;
- The ongoing communication with other agencies and institutions regarding the status of women.

The Committee shall consist of seven members, to be elected for staggered terms of three years. It shall report to Council through the Board of Social and Ethical Responsibility for Psychology.

N. At its March 1974 meeting, the Board of Directors received a draft of the "Proposal for Project Impact," to assist graduate departments of psychology in the education of their minority students, and voted to approve it in principle, authorizing the Executive Officer to seek support.

O. The following annual reports or minutes were received:

1. Annual Report of the Committee on Scientific and Professional Ethics and Conduct, including a summary of old and new cases before the Committee, for the period July 1, 1972, to June 30, 1973.

2. Minutes of the 1973 meetings of the Board of Social and Ethical Responsibility for Psychology.

3. Annual reports from the following committees reporting to BSERP:

a. Committee on Academic Freedom and Conditions of Employment, including a supplement providing a comprehensive summary of cases reviewed during 1973.

b. Committee on Equality of Opportunity in Psychology

c. Ad hoc Committee on the Status of Women in Psychology.

IV. BOARD OF DIRECTORS

A. In accordance with Article VII, Section 3, of the APA Bylaws, the Board elected President Leona E. Tyler as its Chairman and President-elect Albert Bandura as its Vice-Chairman for 1973. For 1974, the Board elected President Albert Bandura as Chairman and Board member Richard C. Atkinson as Vice-Chairman.

B. At its meeting in November 1973, the Board voted that its provision for an Executive Committee of the Board be rescinded.

C. The Board adopted the following schedule of dates for its meetings in 1974: January 17, May 11–12, June 28–29, August 29, and December 6–7. A sixth meeting will depend on the dates of the 1974 adjourned meeting of Council, presumably in January 1975.

D. At its meeting in March 1973, the Board discussed the possible conflict felt by a Board member who may continue to represent a division on Council after election by Council to the Board of Directors. As a Board member, the orientation is toward APA as a whole; as a division representative, a special obligation is felt to represent the feelings of the division. To reduce this potential conflict, the Board will recommend to newly elected Board members that they resign their positions as division representatives to Council when they assume their positions on the Board of Directors.

E. At its meeting in November 1973, the Board received with thanks an information report from the Ad hoc Committee on Archival History announcing that a second deposition of APA materials had been made to the U.S. Library of Congress.

F. At its meeting in November 1973, the Board decided to defer until its "retreat meeting" (May 11–12, 1974) a discussion of guidelines for responding to requests from individuals and groups wishing to appear before the Board at its meetings.

G. The Board discussed frequently during the year ways in which to improve communication with and from Council. President Tyler initiated a system of writing a special letter to Council after each Board meeting, reporting the highlights and providing more of the background discussion leading to decisions than ordinarily appeared in the Minutes. The Board also invited the chairman and chairman-elect of each of the

major boards to attend one of its meetings to present plans and programs, along with budget requests, for the coming year, and asked them to indicate which issues they wished emphasized at the winter meeting of Council. This procedure proved helpful, although hopes that board chairmen would indicate priorities in a way that would be helpful in preparing the APA's overall budget were not realized.

H. The following Board members were appointed as liaisons to the indicated boards and committees for 1974: Finance Committee, Robert Perloff; Policy and Planning Board, Herbert Kelman; Publications and Communications Board, John Lacey; Board of Convention Affairs, Donald Campbell; Board of Professional Affairs, John Conger; Board of Scientific Affairs, Richard Atkinson; Board of Social and Ethical Responsibility for Psychology, M. Brewster Smith; Committee on International Relations in Psychology, Leona Tyler; Committee on Legislative Affairs, Leonard Krasner; Ad hoc Committee on Human Resources, Kenneth B. Little.

V. DIVISIONS AND STATE ASSOCIATIONS

A. Council voted to approve the establishment of a new Division of Population Psychology (Division 34), effective January 1, 1974.

B. Council voted to approve the establishment of a new Division of Psychology of Women (Division 35), effective January 1, 1974.

C. Upon recommendation of the Committee on Structure and Function of Council and the Board of Directors, Council voted at its meeting in August 1973 to add the following sentence to Rule of Council 10-1.6:

If a Council Representative fails to attend a meeting of Council and does not arrange an alternate, the Recording Secretary will inform the Division or State Association of the failure of its Representative to attend the Council meeting.

(At the Council meeting in January 1974, there were only two division representatives and one state association representative who failed to attend or arrange for an alternate.)

D. At its meeting in November 1973, the Board of Directors voted approval of the submission of a grant application to the Social Rehabilitation Service for a research study by Division 22 (Rehabilitation Psychology).

E. At its January 1974 meeting, Council received a Special Report of the Policy and Planning Board on the Matter of Incorporation of Divisions. The action, after discussion, was to establish an ad hoc committee of Council, to be appointed by the incoming President, consisting of eight members, three to be from incorporated divisions, three from the other divisions, with the Chairman of the Policy and Planning Board and a member of the Finance Committee serving as ex officio members. This committee was charged with clarifying the relationships of incorporated and unincorporated divisions to the APA, including the preparation of a list of possible division actions that might endanger the 501(c)-3 tax status of the APA. This action grew out of discussion of three specific recommendations, made by the Policy and Planning Board and supported by the Board of Directors (which had considered the report at its meeting on January 17, 1974), namely: (*a*) a division of the APA cannot commit or obligate any funds not currently available to that division or engage in any activities that might result in such obligation without prior consultation with the Board of Directors; (*b*) divisions shall account for their income and expenditures of funds to the APA on an annual basis as well as provide a statement of activities; and (*c*) divisions may not institute or participate in legal activities without prior approval of the Board of Directors.

During the discussion, the Executive Officer, in response to a question about why the concern had arisen, reported that APA's attorneys have determined that the integral nature of divisions to APA makes APA responsible for division actions, and, additionally, that APA's auditors have for some time argued that division income and expense information should be a part of APA's annual audit. Council was also reminded that at its Montreal meeting the question had arisen as to whether a division may appropriately engage in legal actions independent of APA (Division 29, Psychotherapy, had decided to join the CAPPS suit against Blue Cross/Blue Shield), that Council had voted to ask the Policy and Planning Board to study the matter, and that Council had asked that divisions contemplating legal actions delay them, pending the results of a study by Policy and Planning Board.

In summary, Council approved no specific recommendations on the matter, pending the report of the new ad hoc committee, to be available at the 1974 New Orleans meetings.

F. In a related action, upon recommendation of the Publications and Communications Board and the Board of Directors, Council voted at its January 1974 meeting that the following steps should be taken with respect to liability insurance for divisional publications:

1. The Board of Directors inform all divisions that the Association will attempt to secure liability insurance coverage on all known division publications (those publications not independently incorporated) and that each division must pay the insurance premium charged to its publication(s) by the insurance carrier (estimated at no more than $25 for a newsletter; no more than $75 for a journal).

2. The Board of Directors require divisions to submit a report to the Board of Directors as soon as prac-

ticable and annually (in January) thereafter concerning their publication operations. The report as a minimum would contain the following information for each division publication: (*a*) title and statement of editorial policy; (*b*) number of pages published; (*c*) total circulation and number of paid subscribers, if any.

Any major change in editorial policy or publication format must be reported to the Board of Directors.

3. A third recommendation, proposing a Bylaws amendment about nonjournal types of publication by divisions, was referred to the ad hoc committee established under Item V-E above.

G. In recognition of the increasing emphasis on the need for action in the public arena by organized psychology at the state and local levels, and in recognition of the increasing decentralization of important functions from the national organization to the state organization level, for example, ethics issues, Council at its Special Meeting in March 1973 voted that the Board of Directors publish a letter addressed to all APA members in the *APA Monitor* which identified (*a*) the increasing responsibilities and significance of state associations, (*b*) the relevance of state association activities to all psychologists, and (*c*) the need for support of state association activities through active membership by APA members. Further, in an effort to stimulate and support communication between state associations and APA members in a given state, Council voted that Central Office provide each state association, annually, up to three free copies of the relevant APA mailing list. (Both of these actions have been implemented.)

H. Further on the matter of state relations, Council voted at its Special Meeting that the Board of Directors explore, perhaps by the establishment of a special task force, and/or by referral to all APA boards, the interfaces between APA and the state associations. The purpose of this study would be to evolve recommendations intended to maximize the effectiveness of APA–state association interaction in the interests of the public and of American psychology.

I. Reports were received from the following divisions: 3 (Experimental Psychology); 9 (SPSSI); 12 (Clinical); 14 (Industrial and Organizational); 16 (School); 22 (Rehabilitation); 24 (Philosophical); 29 (Psychotherapy); and 33 (Mental Retardation).

J. Reports were received from the following state psychological associations: California, Delaware, District of Columbia, Montana, New Jersey, North Carolina, and Texas.

VI. ORGANIZATION OF APA

A. At its meeting in February 1973, the Board of Directors received a request from the Policy and Planning (P&P) Board for approval to seek funds for a month-long Conference on the Future of Psychology, to be held in 1973 under the P&P Board's direction (attendees to consist of members of the 1972 and 1973 P&P Boards). It was the P&P Board's judgment that their usual meeting schedule did not provide adequate time for consideration of long-range policies. The Board of Directors voted to ask the P&P Board instead to try to arrange a shorter conference that could be funded within its own resources. The Annual Report of the P&P Board for 1973 indicated that they had scheduled their second meeting for 1973 for six days with the major focus on the future of psychology.

B. At its meeting in August 1973, the Council voted to defer until its January 1974 meeting action on a motion to amend the Bylaws (Article XIX, Section 2) in such a way as no longer to permit the P&P Board to submit Bylaws amendments directly to the membership. The Board had noted that Council is free to submit its views as an accompaniment to P&P-proposed amendments, and its feeling that the P&P Board should be free (in terms of its original mission) to go directly to the membership. At its January 1974 meeting, Council defeated the motion as submitted. The P&P Board did go directly to the membership in 1973 with the amendment altering the conditions under which special meetings of Council can be called (see Item II-A-4, these Proceedings), reporting in its Annual Report that four responses were received from Council members in reply to P&P's request for comment prior to the vote.

C. By mail ballot September 28, 1973, the Board of Directors voted to appoint the P&P Board to serve as a task force to analyze issues involving psychology and its relationship to the field of health. At the Montreal meeting of the Board, there had been discussion of the need for such a task force, and it was decided that one should be appointed to report back in the fall of 1974. The P&P Board had expressed considerable interest in the project and informally had planned to undertake such an analysis. A motion submitted at the January 1974 meeting of Council that a Council task force be appointed to develop a position paper in this area was defeated.

D. At its January 1974 meeting, the Board of Directors reviewed a recommendation to be submitted to Council that would have recommended an amendment to the Bylaws to prevent the Executive Officer's voting as a Council member, voting to refer the matter to the P&P Board. The Council, however, defeated the motion without requesting referral. (As an elected officer, the Executive Officer is a nonvoting member of the Board and a voting member of Council.)

E. Under New Business at the January 1974 meeting, Council voted to refer to the Policy and Planning Board and to the Education and Training Board a motion that Council review the issue of master's-de-

gree-level training as a criterion for full membership in the APA.

F. At the August 1973 meeting of Council, a motion was introduced to provide that 30% of those members attending a Council meeting may order a roll call vote on any issue and order subsequent publication of the vote in the *APA Monitor*. This was referred to the Committee on Structure and Function of Council (CSFC), which recommended its approval, with a corresponding change in Rules of Council, as the current requirement is 50% under Robert's Rules of Order. The Board of Directors at its meeting in November 1973, however, recommended that the CSFC recommendation not be approved, feeling that a majority of Council is a reasonable percentage for a roll call vote and, further, that subsequent publication in the *APA Monitor* should not follow automatically but be voted upon in each instance. The Council, at its meeting in January 1974, discussed the matter fully and voted its approval of the original motion. A discussant suggested that written signed ballots might be an alternative to an oral roll call at times.

G. At the January 1974 meeting, a motion under New Business that Council meet three times a year for a minimum of two days at each meeting at four-month intervals was referred to the Committee on Structure and Function of Council.

H. At its December 1972 meeting, Council voted that an Ad hoc Committee on APA Organization be appointed, two members to be nominated by the Policy and Planning Board, two by the Committee on Structure and Function of Council, plus additional members they might wish to have join them. At its meeting in February 1973, the Board of Directors voted to appoint James Deese as the Chairman of the Ad hoc Committee and the following persons as members: George W. Albee, Marcia Guttentag, Edwin B. Newman, Lyman Porter, and John Stern.

At its meeting in August 1973, Council received a "Preliminary Report for Discussion" from the Ad hoc Committee, voting to discuss it in Committee of the Whole. The report suggested a decision-making structure based on participation by selected Council members as members of substantive boards concerned with four major roles of psychologists, the boards to be those of professional affairs, scientific affairs, educational affairs, and psychology in public policy (the Committee had not yet come to conclusions concerning the areas of publications and the convention). An Executive Board of Council would be composed of members of Council selected from each of the substantive boards in addition to the Association officers. Each of the boards would presumably have committees, and there would be a system of advisory and operating committees to assist the Executive Board to direct Central Office activities, plus a "watchdog" committee of Council to monitor the implementation of policy determinations made by Council.

Straw votes and comments indicated the following opinions of Council: (*a*) the plan could provide a better structure for setting priorities; (*b*) the plan should be submitted in parts for action, so that Council (and other groups and individuals considering the report) could indicate approval of some, but not all, of the recommendations; (*c*) the hope that there could be definite recommendations by the January 1974 meeting; (*d*) the Committee should consider the possibility of member dues allocations to the areas covered by the four boards, plus a fifth for "housekeeping" funds; (*e*) the relation of divisions under the proposal should be considered as more important than the Committee was reported to feel; and (*f*) a general observation that the plan appeared more viable than previous proposals for changes in APA structure.

Council was informed that the Finance Committee and the Board of Directors had referred to the Ad hoc Committee a letter from the Ad hoc Committee on Dues Structure, constituting its final report. Council received a copy of the letter for its information.

At its meeting in January 1974, the Board of Directors received the Final Report of the Ad hoc Committee on APA Organization and after extended discussion voted not to recommend approval of the recommendations of the Committee, but to recommend that the report be referred to the Policy and Planning Board for consideration as part of its continuing review of APA organization. The Board voted, further, to discharge the Ad hoc Committee with thanks for the devoted work involved in preparation of its recommendations and with sympathetic awareness of the frustrations involved in any effort to bring about major restructuring of APA.

The Board noted that in the recent history of APA the Council has consistently rejected proposals for major changes in APA structure. The fact that the issue keeps arising suggests that there are real and continuing problems. In discussion of this plan in Council, attempts should be made to identify specific, less-sweeping changes that might improve APA functioning. The Board decided to discuss the general matter at its retreat meeting in May 1974, including the development of specific changes that might be recommended under the present structure. It was also pointed out that several changes already made in the last year may make the apparent problems less forbidding—the establishment of a Nominations and Elections Committee of Council; the establishment of a Committee on Legislative Affairs—and that time should be taken for evaluation.

When Council received the Final Report of the Ad hoc Committee, along with comments from the Board of Directors, at its January 1974 meeting, it voted to concur with the Board's recommendation that the re-

port be referred to the Policy and Planning (P&P) Board, with the further request that the P&P Board report to Council at the 1974 New Orleans meetings (with adequate time provided for full discussion), considering also a motion (not adopted by Council) from a Representative from Division 3 (Experimental) that additional alternatives be presented by the P&P Board, including an earlier plan for federation. The P&P Board Chairman asked for comments from Council members, and the Chairman of the Ad hoc Committee informed Council that he would forward all relevant materials from his Committee to the P&P Board. The Ad hoc Committee was discharged with thanks.

I. At its "retreat meeting" in May 1973, the Board of Directors discussed current conflicts between some scientific and professional groups within APA, noting that the organization of one faction also forces other factions to organize. Also discussed was a possible need to call Council's attention to the more controversial issues coming up on the Council agenda and to generate more interest by divisions and state associations in APA-wide affairs.

J. The following reports were received:

1. Annual Report of the Policy and Planning Board.
2. Special Report of the Policy and Planning Board on the Matter of Incorporation of Divisions.
3. Annual Report of the Committee on Structure and Function of Council.
4. Final Report of the Ad hoc Committee on APA Reorganization.

VII. PUBLICATIONS AND COMMUNICATIONS

A. At its meeting in February 1973, the Board of Directors received with thanks and approved in principle a statement of 1973 goals for the Publications and Communications Program, with the understanding that the Publications and Communications (P&C) Board and its Communications Committee will approve goals, review specific elements, and establish priorities. The Board also discussed changes in journal operations proposed by the P&C Board, noted certain reservations expressed by the Board of Scientific Affairs and staff, and referred the proposals back to the P&C Board.

B. At its meeting in May 1973, the Board of Directors authorized an increase of $17,000 in the 1973 Budget of the P&C Board to meet expenses due to changes in editors and excessive backlogs accumulated by the outgoing editors.

C. At its June 1973 meeting, the Board of Directors was informed that the 1974 Preliminary Budget for the P&C Board included funds for page increases to facilitate holding the publication lag to 12 months for all journals and to ease the transition period for the five journals whose editorships are changing. Editors will be asked, strongly, to agree to stay within their page allotments and to keep the lag down. The Board was also informed that the 1974 Preliminary Budget for the P&C Board included an increase in funds over 1973 for office expenses for editors, reflecting the overlapping expenses of incoming and outgoing editors for four journals.

D. The Board, upon recommendation of the P&C Board, approved the concept of a program to conduct exploratory and management studies of the journals, *Psychological Abstracts*, and such other products and services as the P&C Board may determine as necessary to maintain the quality and economic integrity of the APA publications and communications services. The Board did not approve, however, increasing the P&C Board 1974 Budget by the recommended amount. In view of budgetary stringencies, the Board doubted that the P&C 1974 Budget would permit as extensive a program as envisaged by the P&C Board. After reviewing anticipated revenues and expenditures, the Board of Directors, as of June 1973, set a goal for 1974 of $85,000 or more of revenue over expense for the publications and communications activity.

E. The Board received with interest a report from the P&C Board of proposals planned for the *Journal of Experimental Psychology* (*JExP*) and the *Journal of Comparative and Physiological Psychology* (*JCPP*). The P&C Board voted that the *Journal of Experimental Psychology*, beginning with 1975, will be divided into four sections under the general title of *JExP*, with each section edited by an independent editor appointed by the P&C Board, but with all four editors comprising the Board of Editors of *JExP*. The sections, which may be subscribed to independently, will be: *JExP: General; JExP: Learning and Memory; JExP: Perception and Performance;* and *JExP: Animal Behavior Processes*. Beginning in 1975, the P&C Board voted to move experimental studies of animal behavior processes, which have as their main focus the advancement of general behavior theory, from the *JCPP* and to continue *JCPP* as a general journal of physiological and comparative psychology.

F. At its May 1973 meeting, the Board of Directors, at the request of the P&C Board, approved increases in subscription prices to the amounts listed below, beginning in 1974:

JOURNAL	MEMBER	NONMEMBER
Contemporary Psychology	—	$15
Comparative and Physiological Psychology	$25	$60
Counseling Psychology	—	$15
Psychological Review	—	$15
Developmental Psychology	—	$30

G. At its November 1972 meeting, the Board of Directors had voted to establish a Task Force on the

American Psychologist, composed of representatives of the P&C Board, the Board of Directors, and staff. At its January 1974 meeting, the Board received with thanks and appreciation the report of the Task Force, voting approval of its recommendations, as follows:

1. The *AP* continue, as it has in the past, to be upgraded editorially, but that a more "popular" type format and content not be adopted.

2. Agreed with the Task Force that many of the APA materials obligated by the Rules of Council, which are presently contained within the pages of the *AP*, should continue to be published in *AP*, but that some attempt be made to present them separately, placing a higher emphasis on the substantive articles. Staff has agreed to provide the Task Force with the necessary background information for a plan of action, to be forwarded to the Board for consideration at a later time.

The Board noted that at its November 1973 meeting it had voted to move regional association proceedings and APA's convention program proceedings to the Journal Supplement Abstract Service.

3. It is essential that the editorship remain within the Central Office, with the Executive Officer serving as Editor and the Board of Directors as an editorial advisory board.

4. The *AP* should continue to be under the aegis of the Board of Directors, getting appropriate advice from the P&C Board, treated separately from the other journals for purposes of budget analysis and page allotment deliberations. The Board of Directors is charged with taking a more active role in guiding *AP* policies, and the *AP* should be subject to annual policy review by the Board of Directors.

5. The *AP* should continue to be included as part of Association dues and the price continue to be governed by the economics of the Association.

H. At its June 1973 meeting, the Board of Directors was informed that its Executive Committee had affirmed the decision of the Executive Officer that the Journal Supplement Abstract Service (JSAS) be included on the 1974 dues statement as an available publication.

I. At its meeting in November 1973, the Board had asked staff to investigate the costs of libel insurance for all of APA's journals. (APA carries such insurance for the *American Psychologist, Psychological Abstracts*, and the *APA Monitor*.) Investigation proved that such insurance would cost around $90 per year for each additional journal. At its January 1974 meeting, the Board voted that libel insurance should be purchased for *Contemporary Psychology* but was not needed for the other journals. (See Item V-F, these Proceedings, for related actions with respect to liability insurance for division publications.)

J. Upon recommendation of the P&C Board, and of the Board of Directors, Council voted at its January 1974 meeting that the following wording be substituted for the present Rule of Council 70-10.1 concerning occasional publications (separates):

The American Psychological Association shall publish and disseminate those books, pamphlets and other materials that are necessary to inform: (*a*) the psychological community or (*b*) the public about psychological standards, scientific and professional advances, education and training opportunities, and such other information that meets the following criteria:

1. Is consistent with the objectives of the Association and furthers the purposes of APA as a not-for-profit organization;

2. Has excellence of content as attested to by independent editorial reviewers;

3. Is generated within APA, its boards or its committees, or is submitted, commissioned, or selected for publication or dissemination, and in any case is approved by a relevant board of APA;

4. Performs a communications function that would not be accomplished at all, or accomplished as well, by the commercial sector.

This program shall be planned and managed in such a manner as to recover its cost. There shall be a committee of the Publications and Communications Board to be concerned with advising the Executive Officer on overall goals and guidelines for the separates program.

Final determination of what is acceptable, including the alternative of rejection in whole or in part, in any pamphlet, book, or report offered publicly by the Association shall be made by the Executive Officer, except that the Council of Representatives may vote, on recommendation of the Board of Directors, to set up some alternative means of editorial control. In this latter case, it shall be the responsibility of the special editor to consult with the Executive Officer and the Managing Editor before giving his final approval to the document in question.

K. Upon recommendation of the P&C Board and the Committee on International Relations in Psychology, the Board of Directors at its June 1973 meeting endorsed APA's application for APA membership in the International Council of Scientific Unions–Abstracting Board.

L. The following reports were received:

1. Annual Report of the Publications and Communications Board and of the following committees and representatives reporting to the Board:

a. Communications Committee.

b. Council of Editors.

c. APA Representative to the National Federation of Abstracting and Indexing Services.

VIII. CONVENTION AFFAIRS

A. At its December 1972 meeting, the Board of Directors directed the Convention Manager to continue negotiations toward a more favorable cost arrangement with the San Francisco hotels and to report his finding to the Board. The Convention Manager reported at the February 1973 meeting of the Board that

negotiations were successful; therefore, there was no need to change the plans for San Francisco to be the site for the 1977 Convention. At its June 1973 meeting, upon recommendation of the Board of Convention Affairs, the Board of Directors approved Toronto, Ontario, Canada, as the site for the 1978 Convention. (The sites for 1974, 1975, and 1976 are New Orleans, Chicago, and Washington, D.C., respectively.)

B. At the January 1974 meeting of Council, a straw vote was taken which indicated the willingness of Council to have the Board of Convention Affairs include the city of Las Vegas, Nevada, in its consideration of western convention sites. There is a policy to hold APA conventions on the west coast every three or four years, and the Convention Caucus, informed of the difficulties in finding adequate cities, agreed that Las Vegas, with its many reasonably priced hotel rooms and good meeting facilities, should be considered.

C. Under New Business at the January 1974 meeting of Council, a resolution was submitted by the Representative from Division 22 (Rehabilitation) concerning convention facilities for the disabled. Council concurred with the action of the Board of Directors in referring the resolutions to the Board of Convention Affairs (BCA), noting that the BCA has also been concerned about the problem of architectural barriers that cause difficulty for the physically handicapped and has made it a part of the convention-planning activities.

D. At its November 1973 meeting, in its discussions of ways in which anticipated deficits in the 1974 Budget might be reduced, the Board of Directors voted that the Budget for the convention be revised to show a net gain to the Association of $50,000. The Board noted that such a change would almost certainly involve increased convention registration fees.

E. Council, at its meeting in August 1973, had voted to increase convention registration fees to $15 for members and $25 for nonmembers (the fees charged on a temporary basis for the Hawaii and Montreal meetings), with no fees for students or dues-exempt members. At its meeting in January 1974, however, Council approved the recommendation of the Board of Directors that 1974 convention registration fees be established as given in the table below, and that Council Rule 150-2.2 be revised to delete references to dollar amounts, with a new Rule 150-2.3 authorizing the Board of Directors to set fees annually after recommendation from the Board of Convention Affairs.

		1974 FEES	
	CURRENT FEES	ADVANCE	ON-SITE
Members [6]	$15	$20	$25
Nonmembers	25	30	35

There had been an earlier recommendation that a small fee would be charged for student categories, but this was disapproved by the Board of Directors at its January 1974 meeting and concurred in by Council. Helpful to the Board was a staff analysis of convention fees in 1973 and data concerning fee practices of similar associations.

F. The following reports were received:

1. Annual Report of the Board of Convention Affairs and of the following committees reporting to the Board:

a. Committee on Films and Other Media.
b. Operations Committee.
c. Committee on Program Innovations.
d. Committee on Research and Evaluation.

IX. EDUCATIONAL AFFAIRS

A. During the year, the Board of Directors took the following actions with respect to communications and recommendations submitted by the Education and Training (E&T) Board:

1. At its February 1973 meeting, was informed that the E&T Board had appointed Robert Schaefer, Professor of Education and Dean of Teachers College, Columbia University, as a "knowledgeable layman" to the Committee on Accreditation, in response to a request from the Board of Directors.

2. At its February 1973 meeting, received with thanks and commendation a near-final draft of the Proposal for a Secondary School Curriculum Development Project on Human Behavior.

3. At its May 1973 meeting, the Board discussed at length the extent to which the Association should take a direct hand in the education of future psychologists. Under this broad heading it considered—without conclusions—such issues as:

a. Whether or not APA should set up a college of its own and if so at what level, undergraduate, graduate, or postdoctoral; how and from where resources could be secured for such a venture; the relationship of such an institution to existing agencies or universities.

b. Patterns of support for graduate training most appropriate in terms of educational values, political realism, social needs, disciplinary needs, and desires of students for training; whether or not support should be on an individual or institutional basis; the appropriateness of support when jobs are beginning to diminish, the possibility that the apparent shortage of positions for psychologists is an artificial result of governmental policies; and the possibility that continued production of psychologists would force the development of new careers utilizing their skills.

c. The need for continuing assessment of education in psychology; the possibility that APA's major impact in this area might come through constant up-

[6] Dues-exempt members and students shall be exempt from convention registration fees.

grading of, for example, the Area Examinations in Psychology of the Educational Testing Service and a national assessment of achievement of psychology. Related to this last was discussion of whether such assessment should be restricted to the undergraduate and graduate levels. The Board asked that the attention of the E&T Board be called to the national-assessment-in-psychology idea and noted that several psychologists were experts in the area and could serve as consultants. It also noted that the general idea might be of interest to such pandisciplinary organizations as the American Council on Education, the Social Science Research Council, and the American Council of Learned Societies.

4. At its June 1973 meeting, the Board voted concurrence with the E&T Board's recommendation that a subscription charge for *Periodically* not be initiated.

5. At its June 1973 meeting, the Board concurred in the E&T Board's suggestion that other boards be informed of the E&T Board's practice of open advertising for membership on its committees and on that Board, via notices in the *APA Monitor*.

6. At its June 1973 meeting, the Board voted to approve a full-time staff position in the Office of Educational Affairs to develop and monitor APA activities in the area of continuing education, but contingent upon obtaining external support for the position, in view of budgetary stringencies. At its August 1973 meeting, the Board received a communication from the Chairman of the E&T Board, requesting an increase in the budget of that Board in order to secure the services of "a cognizant APA member who has a background in the area of continuing education, to be commissioned to review the APA's needs and potentials in continuing education and, in particular, to seek ways and means for supporting such activities." The Board voted to reaffirm its action of June 1973 and expressed the view that preparation of a proposal seeking funds could be done within the E&T present budget.

B. At its August 1973 meeting, the Board received a communication stemming from the Vail Conference concerning the composition of the Follow-up Commission to be established to implement the conference recommendations, voting to endorse the recommendations of the Conference for the Commission. Discussion of the Vail Conference recommendations themselves, by the Board of Directors and Council, had not yet occurred, formally, as of the January 1974 meetings.

C. At the November 1973 meeting of the Board, President Tyler reported on a meeting held with representatives of the Black Students Psychological Association (BSPA) on September 27, 1973. The Board voted to ask the Office of Educational Affairs to consult with BSPA and to arrange liaison on ways of improving information collection and dissemination from and for minority graduate students. Further, the Board voted that BSPA not be required to repay the loan made as of July 1, 1970, to assist in the maintenance of a Washington office for a three-year period.

D. The Council, upon recommendation of the E&T Board and the Board of Directors, voted approval at its January 1974 meeting of changes in Rules of Council 90-2.1 and 90-3.1 to increase the size of the Committee on Accreditation from six to *nine* members, and to update (from August 1971 to *January 1973*) the references to procedures and criteria now in force. Action was delayed on a recommendation to substitute the word "professional" for "clinical, counseling and school" in defining the programs to be accredited, pending further review by the E&T Board. The use of "professional" was deemed ambiguous.

E. On recommendation of the Board of Directors, Council adopted at its January 1974 meeting the following schedule of accreditation fees for 1974–1975:

UNIVERSITY PROGRAMS	FEE
Annual Accreditation Fee (first program)	$300
Annual Accreditation Fee (second program)	150
Visit Fee	700
Application Fee (per program)	300
Initial Visit Fee (per program)	700
INTERNSHIP PROGRAMS	**FEE**
Annual Accreditation Fee	$375
Application Fee	300
Initial Visit Fee	400

F. The following reports were received:

1. Annual Report of the Education and Training Board and reports from the following committees and other bodies reporting to the Board:

a. Committee on Accreditation.

b. Committee on Pre-College Psychology.

c. APA Visiting Psychologist Program.

d. APA Visiting Scientist Program.

e. Ad hoc Committee on the 2-Year College.

f. Task Force on Issues of Sexual Bias in Graduate Education.

g. Resolutions and Proposals of the National Conference on Levels and Patterns of Training in Professional Psychology, Vail, Colorado, July 25–30, 1973.

X. PROFESSIONAL AFFAIRS

A. The Board of Directors took the following actions with respect to recommendations and other communications from the Board of Professional Affairs (BPA) during the year:

1. At its February 1973 meeting, voted to approve publication of "Guidelines for Psychologists Conducting Growth Groups" in the *American Psychologist* at an early date. (See *American Psychologist*, 1973, **28**, 933.)

2. At its February 1973 meeting, voted to accept an invitation to join the National Consortium for Child Mental Health Services. Other members of the Consortium are: American Academy of Child Psychiatry,

American Association of Psychiatric Services for Children, American Association for Children's Residential Centers, American Association on Mental Deficiency, American Medical Association, National Congress of Parents and Teachers, National Association of State Mental Health Directors, and Society of Professors of Child Psychiatry.

3. At its February 1973 meeting, was informed that APA has been appointed *amicus curiae* in the matter of *Morales v. Therman*, a class action on behalf of juveniles committed to residential training schools in the State of Texas. As it did in *Wyatt v. Stickney*, APA is joining with the American Orthopsychiatric Association, the Center for Law and Social Policy, the American Association on Mental Deficiency, and the Civil Liberties Union as *amicus curiae*. Also participating are the Child Welfare League and the National Council on Crime and Delinquency.

4. At its February 1973 meeting, received a recommendation concerning possible opposition to a proposed revision by the Supreme Court in the rules of evidence for federal courts, voting to defer action pending additional information.

5. At its February 1973 meeting, was informed that the Accreditation Council for Facilities for the Mentally Retarded (AC/FMR) had approved the admission of the APA as a Council member, and authorized the payment of the $3,000 annual contribution, requesting also that the BPA name two Councillors to represent the APA on AC/FMR. Membership on the Council was approved by the APA Council in 1969.

6. At its February 1973 meeting, authorized increases in the 1973 Budget for BPA from $20,000 to $30,000, the additional funds requested for the Committee on Health Insurance, the Task Force on Regional Review Committees, liaison responsibilities with the American Board of Professional Psychology and the American Association of State Psychology Boards, and for a Division Presidents Liaison Meeting.

7. At its May 1973 meeting, in response to a request from the Office of Child Development (OCD), approved initial submission of a proposal to provide part-time consultation by APA members to OCD in developing a strategy for providing mental health services to Head Start children (with consultation with relevant divisions).

8. At its June 1973 meeting, informed that the Council of National Organizations for Children and Youth, to which APA belongs, had been replaced by the National Council of Organizations for Children and Youth (NCOCY), voted to approve membership in NCOCY, and authorized the Executive Officer to appoint a representative.

9. At its June 1973 meeting, received a report of a Summer Intern Project on Social Responsibility, administered under the direction of Gottlieb Simon.

10. At its June 1973 meeting, was informed that Ralph Nemir had been hired to function in the capacity of a state legislation specialist and to provide informal liaison with state associations on many matters, including revenue-sharing programs.

11. At its June 1973 meeting, the Board discussed a proposal from Lloyd Humphreys that the Association study the advisability—from the public's point of view—of the inclusion of psychotherapy in medical insurance contracts, policies, or programs. Opinions varied as to the merits of his arguments, but the matter was referred to BPA for its advice and recommendations, with consideration of the broader context of psychology's role as a health profession. Learning at its November 1973 meeting that BPA had referred the matter to the Committee on Health Insurance, the Board voted that the proposal also be referred to the Policy and Planning Board for consideration in its role as a task force to develop a position paper on psychology and health. (See Item VI-C, these Proceedings.)

12. At its November 1973 meeting, the Board voted to request the Board of Directors of the American Board of Professional Psychology (ABPP) to establish a National Registry Council as outlined in the recommendations of the Ad hoc Coordinating Committee for the National Registry of Health Service Providers in Psychology, with the provision that rules and regulations promulgated by that Council receive prior review from BPA before implementation, as well as any changes in such rules and regulations. There shall be no financial obligation nor expenditure by APA in direct support of this task. The Board was informed that ABPP was willing to undertake this activity, and that listings of individuals in the Registry would be on a voluntary basis.

13. At its November 1973 meeting, the Board was informed that the Center for Law and Social Policy, the American Psychiatric Association, and the American Orthopsychiatric Association planned to initiate litigation to establish minimum standards for alternative facilities for mental patients in St. Elizabeths Hospital in Washington, D.C. Upon recommendations from the Task Force on Standards for Service Facilities and with consultation from APA's attorneys, the Board authorized the Executive Officer to indicate APA's interest in joining the suit, with formal approval contingent upon review of the actual proposed complaint. (Secretary's note: The suit was filed on February 14, 1974, with APA and the American Public Health Association as additional parties to those listed above.)

B. John Spiegel, President-elect of the American Psychiatric Association, was present by invitation during part of the Board's meeting in November 1973, to discuss mutual problems and proposed solutions facing the two APAs in a wide variety of areas. Similarities were marked. Plans were made for Albert Bandura, President-elect of the American Psychological Associa-

tion, to attend a future meeting of the American Psychiatric Association's Board of Trustees.

C. At its December 1972 meeting, a resolution had been submitted to Council to establish a Task Force on Child Development. The resolution was referred to the Board of Professional Affairs, which considered the matter during 1973 and reported to the Board of Directors and Council in January 1974 that in view of existing policy statements and position papers a new task force is not indicated. BPA provided copies of three documents: BPA Statement on the Report of the Joint Commission on Mental Health of Children; Position Paper on Children and Youth, by the APA Ad hoc Committee on Children and Youth; and a Position Paper on the 1970 White House Conference on Children and Youth, from the Section on Clinical Child Psychology of Division 12 (Clinical).

D. Upon recommendation of BPA and the Board of Directors, Council voted at its January 1974 meeting to discharge the Committee on Relations with Other Professions and to amend Rules of Council accordingly. Council concurred with the reasons offered by BPA: policy coordination and continuity are becoming increasingly important; Central Office staff can provide interprofessional cooperation more promptly and effectively than can a committee.

E. Upon recommendation of BPA and the Board of Directors, Council voted at its January 1974 meeting to establish a continuing Committee on Professional Standards Review, with the following mission and composition incorporated as a new Rule of Council:

There shall be a Committee on Professional Standards Review which shall have the purpose of recommending policy and providing guidance for the operation of state, local, and regional professional standards review committees. The committee shall stimulate the development of state, local, or regional PSRC's and maintain up-to-date guidelines for their operation.

The committee is also charged with responsibility for developing workshop programs at the APA annual conventions and at various regional conventions in order to educate psychologists on matters relating to professional standards review. The committee will further provide a clearinghouse function and will maintain a file of case summaries which it will be provided by state, local, or regional review committees.

The committee is also charged with the task of noting problem areas which are developing and of alerting the Board of Professional Affairs to these matters.

The committee shall consist of seven persons from throughout the United States, including two public representatives, and shall be elected for staggered terms of three years.

The committee shall report to Council through the Board of Professional Affairs.

F. At its Special Meeting in March 1973, Council voted that a proposal be developed for a continuing evaluative research effort to develop social indexes to measure the effects of funding cuts on psychological services, research and education generally, and, additionally, to track the processes of government which may stimulate or inhibit the initiatives of organizations involved in these activities. The matter was referred to the Board of Professional Affairs, the Board of Scientific Affairs, and the Education and Training Board, with BPA to act as coordinator pro tem, for the purpose of ensuring the writing of a proposal, with budget, for consideration by Council at its Montreal meetings. BPA recommended to the Board of Directors in June 1973 that such a social indicator study not be undertaken by APA (too big a project for APA's inadequate funds, etc.), and that consultation with BSA and the E&T Board had not yet taken place. The Board asked that the requested consultation be pursued by BPA and a new recommendation with more details be prepared for later consideration. The Board was informed at its November 1973 meeting that the Chairman of BPA was in the process of consulting with the Chairmen of the E&T Board and BSA.

G. In lieu of an annual report, BPA submitted copies of the Minutes of its meetings held on February 15–16, 1973; May 4–5, 1973; and October 11–12, 1973. No reports were submitted by the committees and task forces reporting to BPA.

XI. SCIENTIFIC AFFAIRS

A. At its meeting in February 1973, the Board of Directors received reports and communications from the Board of Scientific Affairs (BSA), the Education and Training Board, and staff with respect to impending cuts in federal support for training, research, and service in psychology. The Board also received a communication from the Chairman of BSA about anticipated changes in the processes by which science and science-related decisions are to be made in the federal government, for example, diminution of the role of the peer review system; demise of the President's Scientific Advisory Committee. The following recommendations were made: (*a*) the Board of Directors should develop an effective expression of concern relative to the loss of funds for training, research, and service, this expression of concern to be presented as the basis of a coordinated mailing from APA members to their senators and congressmen; (*b*) the Central Office staff should develop an information package concerning the changes in funding patterns, detailing current changes in funding patterns, detailing current changes and their implications; (*c*) APA should develop a system to gather and disseminate information on current funding problems and on alternative ways of coping with funding for training, research, and service; and (*d*) the Steering Committee for the Conference on Levels and Patterns of Professional Training should be asked to

include the problem of reduced funding on the agenda for the forthcoming conference.

The Board was informed that Central Office had already developed an information package for department chairmen and individuals in the APA governing structure, and was actively involved in facilitating communication among those primarily concerned with coping with the changes. An article for the *APA Monitor* had been prepared, and APA Central Office staff was in communication with other national associations similarly affected by the budget cuts. The Office of Policy Research was collecting data relevant to the case for continued funding, as well as preparing projections of future societal needs for psychologists. The Ad hoc Committee on Human Resources had been asked to explore as rapidly as possible additional sources of information and data relevant to the problems.

At this same meeting, the Board met with Bertram Brown, Director of the National Institute of Mental Health, to discuss the current changes in NIMH organization and funding and their implications for psychology. The Board also heard from the Executive Officer on the importance of work at the state and local levels in connection with federal revenue-sharing programs, both general and special, and that Central Office staff was working with state and local associations to ensure that funds potentially intended for psychological training and services not be dissipated.

B. The following resolution was voted by Council at its Special Meeting on March 31–April 1, 1973:

> The Council of Representatives of the American Psychological Association, in special meeting called "to consider the phaseout and termination of federal support of training, research, and service," notes with deep concern and dismay the human consequences of present budget policies. As representatives of a science and profession dedicated to human welfare, we call upon the members of our Association to urge the administration and the Congress to reassert human priorities. We are especially concerned because the emerging patterns of federal revenue sharing involve a radical reduction in our national investment in human welfare.

C. Council also voted at its Special Meeting that Central Office and the Board of Directors be given a vote of confidence and thanks for their efforts in the attempt to remedy the effects of the funding cuts.

D. At its November 1973 meeting, the Board heard an informal report of activity by the Scientific Affairs department and other segments of Central Office with respect to proposed HEW regulations and possible Congressional action concerning research with human subjects. It was the consensus of the Board that regulations were much to be preferred over legislation, and encouraged Central Office to continue its monitoring efforts.

E. By mail ballot September 14, 1973, the Board voted to have the Association join the National Association for Mental Health (NAMH) in a suit to release impounded training and research funds. It was the Board's judgment that the action was in keeping with the sentiments expressed by Council at its Special Meeting, and that the release of the funds for support of training of mental health personnel and for mental health research would be in the public interest as well as in the interests of APA members. The basis of the suit was reviewed and approved by APA's attorneys in consultation with the NAMH counsel.

F. Council was informed at its January 1974 meeting that the Standards for Educational and Psychological Tests had been approved for publication (early in 1974) by the three participating organizations: American Education Research Association, National Council on Measurement in Education, and the American Psychological Association (approved by the Board of Directors in November 1973).

G. Upon recommendation of the Committee on Scientific Awards and the BSA, the Board of Directors at its November 1973 meeting voted an additional $500 (over the $1,000 approved earlier) for the Early Career Awards, so that each of three recipients could receive a citation and $500. The Board was informed that the Committee plans to make three awards each year, from the following areas: Year 1, human learning/cognitive, psychopathology, physiological; Year 2, animal, personality, developmental; Year 3, methodological, social, sensation/perception.

H. Council was informed that a Task Force on Health Research has been formed under the aegis of BSA's Ad hoc Committee on Newly Emerging Areas of Research (NEAR), with the following objectives: compile and disseminate information on psychological research in health; facilitate communication among researchers; survey resources for expansion and stimulation of health-related research (survey interest and resources, define potential roles for psychologists, identify challenges); identify research problem areas; encourage training innovations; and establish liaison between the American Psychological Association and other appropriate groups in health research.

I. Council was informed that a Task Force on Human–Environment Relations has been formed under the aegis of the NEAR committee of BSA, with the following objectives: stimulating psychological research in the area of human–environment relations; providing information about alternative educational models and courses of instruction in human–environment relations; stimulating an understanding of the potential application of research knowledge in the human–environment field to public policy issues; and developing interdisciplinary contacts.

J. The following reports were received:

1. Annual Report of the Board of Scientific Affairs and reports from the following committees, representatives, and task forces reporting to the Board:

a. Committee on Precautions and Standards in Animal Experimentation.

b. Committee on Scientific Awards.

c. Committee on Psychological Tests and Assessment.

d. Ad hoc Committee on Psychology, Family Planning, and Population Policy.

e. Task Force on Health Research.

f. APA Representative to AAAS.

g. APA Delegation to the Inter-Society Color Council.

h. APA Representative to the National Society for Medical Research.

i. APA Representative to the Social Science Research Council.

XII. EXTERNAL AFFAIRS

A. At its February 1973 meeting, the Board received a communication from the Policy and Planning (P&P) Board, suggesting that clarification was needed in the stand the Board of Directors had taken on the principles laid down by the 1969 Ad hoc Committee on Public Affairs (*American Psychologist*, 1969, 24, 1–4). The Board agreed with the P&P Board.

The Board feels that the ordering of scientific concerns, professional concerns, and social problems on a dimension of centrality to the purposes of APA is reasonable, in the sense that the Association's mandate to take public positions becomes less clear and broad as one moves from policies that affect psychology as a science, through policies that affect professional practice in specific settings, to general social policies. The Board does not feel, however, that this ordering should necessarily determine priorities for action or the level of involvement represented by actions, as proposed in the 1969 report. That report did not concern itself with the question of the *obligation* of psychology and psychologists to take action on public issues involving the effects of psychological work on individuals, groups, or society as a whole, a matter discussed by the Board in May 1972. For example, where psychological tools or findings are used inappropriately or in a way that exacerbates a social problem, or conversely, where psychological tools or findings could potentially contribute to the solution of a social problem, a high level of priority and action involvement may well be indicated. Furthermore, the Board also recognizes that the distinction between scientific, professional, and social concerns is often difficult to draw. It may be particularly misleading in view of recent emphases on scientific research applied to social problems and on the conduct of research in professional or action contexts.

B. At its November 1973 meeting, the Board authorized the Executive Officer to seek external funding for a 1974 Public Policy Conference.

C. At its February 1973 meeting, the Board was informed that an additional $1,500 had been received from the Asia Foundation for use in paying Foreign Affiliate fees and journal subscriptions of Asian psychologists.

D. At its June 1973 meeting, the Board referred to the Editor of the *American Psychologist* a recommendation from the Committee on International Relations in Psychology that a special issue of the *AP* be devoted to overviews of psychology in countries other than the United States and Canada. The Editor indicated that he would consult with the Committee for suggestions on a special editor to be named for the issue. The Committee also reported its support for the membership of APA on the International Council of Scientific Unions–Abstracting Board.

E. At its January 1974 meeting, Council received an information item from the Committee on International Relations in Psychology with respect to procedures for requesting action by the International Union of Psychological Sciences. (See Item III-G, these Proceedings.)

F. The Executive Committee reported to the Board at the February 1973 meeting that it had referred to the Editorial Board of the *APA Monitor* a recommendation from the Policy and Planning Board that there be a student section of the *Monitor* and that the *Monitor* be distributed in bulk to graduate departments.

G. At its meeting in August 1973, Council heard a motion calling for mandated space in the *APA Monitor* for segments of the governing structure, with a committee of Council to receive the material and monitor the activity. The motion was referred to the Board of Directors, which discussed the matter at its meeting in November 1973. Under present concepts, the *Monitor* is a nonarchival publication which avoids competition with the *American Psychologist* by journalistic reporting of "news" that may affect the membership, rather than publishing mandated articles from the APA governing structure. It attempts to maintain its credibility by reporting pro and con positions in the development of possible policies under consideration by all parts of the APA structure. The Board encourages Council and all boards and committees to be critical of the *APA Monitor* and its staff in the selection, impartiality, and comprehensiveness of its news stories. The Board recommended, therefore, that Council defeat the motion for mandated space and the establishment of a Council committee to receive material for space and to oversee its publication. A substitute motion, offered by the Psychologists for Social Action through Hannah Levin, Council Representative from Division 27 (Community), was introduced at the January 1974 meeting of Council, calling for mandated space in the

Monitor for any APA committee or "to diverse groups on an impartial basis, and a Council committee of three to handle all grievances relating to the publication practices of the *APA Monitor*." The first part of the motion was withdrawn, and Council voted to defer until its New Orleans meetings action on the proposed Council committee.

H. By mail ballot Council approved the affiliation of APA with the American Council of Learned Societies, with Rule of Council 10-5.2 to be amended accordingly.

I. At its meeting in February 1973, the Board was informed that the Central Office historical display project had received a contribution of $50 from Division 1 (General) and "up to $100" from Division 26 (History). So far, there have been displays on E. G. Boring, E. B. Titchener, R. M. Yerkes, and timing devices.

J. At its January 1974 meeting, Council received the Final Report of the Ad hoc Committee on Legislative Affairs and took the following actions with respect to the report, recommendations from the Board of Directors, and motions introduced by Council members:

1. Defeated a recommendation that an Office of Legislative Affairs be established in the Central Office;

2. Approved continuing liaison with governmental officials on the utilization of clinical psychologists in Medicare and Medicaid, the Executive Officer instructed to call upon appropriate consultants as needed;

3. Defeated a recommendation calling for a study as to the feasibility and cost of an in-house attorney for APA, on the grounds that investigations already undertaken by Central Office and the Board of Professional Affairs have indicated that the legal needs of APA are so diverse that the total costs of an in-house attorney plus outside consultation would be substantially greater than the present arrangement;

4. Concurred with the recommendation that the composition and function of the continuing Committee on Legislative Affairs (see below) be reviewed at the end of that committee's first year;

5. Concurred with the decision of the Executive Officer that the Legislative Affairs Officer be assigned to the office of the Director of Programs and Planning;

6. Amended Rule of Council 10-9.1 establishing the Committee on Legislative Affairs (COLA), adopted by Council at its August 1973 meetings, to substitute a member of the Policy and Planning Board for the Chairman of the Committee on State Legislation as a mandated member of COLA, and to add the Chairman of the Committee on Psychological Tests and Assessment as a mandated member, thus increasing the size of the continuing committee from nine to ten members. The new Rule of Council reads as follows:

There shall be a Committee on Legislative Affairs reporting to Council through the Board of Directors. The Committee is responsible for recommending the initiation and modification of governing units dealing with critical legal, statutory, and budgetary practices and proposals which are pertinent to the support of and utilization of psychology by the public, and for recommending the most effective structural and procedural arrangements to accomplish these ends. Specifically, the Committee will recommend legislative policy; determine current and future legislative, regulatory, and budgetary targets of probable attainability; recommend priorities for action; provide consultation to staff, boards, and committees as to goals, strategies, and existing deficiencies in the legislative or regulatory area; and recommend appropriate liaison by APA when needed with other professional and scientific groups with whom we share common legislative interests.

The Committee shall consist of ten members: the Chairman of the Committee on Health Insurance, a member of the Policy and Planning Board, a member of the Board of Professional Affairs, a member of the Board of Scientific Affairs, a member of the Education and Training Board, a member of the Board of Social and Ethical Responsibility for Psychology, the Chairman of the Committee on Psychological Tests and Assessment, and three elected members who shall be members of the Council of Representatives at the time of their election. The term of office of elected members shall be for three years so staggered that at least one member is elected each year. In making nominations the Committee on Nominations and Elections of Council shall attempt to ensure that candidates possess nationally recognized expertise in legislative or administrative affairs in the area of the educational, professional, or scientific contributions of psychologists to human welfare.

K. Summaries of the discussions and actions leading to the establishment of a continuing Committee on Legislative Affairs appear in the Minutes of Council in December 1972, March 1973, and August 1973, together with the Minutes of the Board of Directors meetings of February 1973, May 1973, June 1973, August 1973, November 1973, and January 1974.

L. At the Special Meeting of Council in March 1973, a motion was received directing that the APA join, as participant, the class-action suit proposed by the Council for Advancement of the Psychological Professions and Sciences (CAPPS) against Blue Cross/Blue Shield under the Federal Employees Benefit Plan, for failure to permit freedom of choice for the delivery of psychological services. The issue was referred to the Board of Directors, with a request for a report back to Council within 60 days, the sense of Council at that meeting being that APA should become involved, unless the Board saw possible dangers to the Association.

Also at the Special Meeting, Council voted that the President of APA appoint an ad hoc committee to examine the issues, interactions, and relationships between the APA and CAPPS, and to propose, recommend, and specify the relationships, and the terms and provisions of any relationships, which will continue after September 1973, to the Board of Directors and Council. An interim report from the Ad hoc Committee on APA–CAPPS Relationships was received by Board and Council in August 1973, and a Final Report in January 1974.

Discussions by the Board of Directors at its meetings in May, June, and August of 1973, as reported in

the Minutes of these meetings and to Council, resulted in a recommendation that APA not participate in the CAPPS suit against Blue Cross/Blue Shield, and at the Council meeting in August 1973 action on a resolution that APA participate was tabled, by vote of Council.

M. At its meeting in January 1974, Council received the Final Report of the Ad hoc Committee on APA–CAPPS Relationships, recommendations from the Board of Directors with respect to the report, motions from Council members, together with material distributed to Council members at the meeting by CAPPS. Council heard statements by Wilse B. Webb, Chairman of the APA–CAPPS committee, and Norman Garmezy, a member of the committee. In addition, Council received a report from the Policy and Planning Board presenting a series of recommendations and proposed Articles of Incorporation and Bylaws for the new advocacy organization recommended by the Ad hoc Committee and the Board of Directors. The sequence of actions voted was as follows:

1. Approval of the Board of Directors recommendation that the APA does not accept CAPPS as an advocacy organization which will meet appropriately the needs of all psychologists.

2. Referral to the Policy and Planning Board, for study and recommendations, the following actions proposed by the Ad hoc Committee: (*a*) guidelines for a legally independent advocacy organization, (*b*) actions to be taken to establish an advocacy organization structured upon the guidelines, and (*c*) support to be provided by APA of the proposed advocacy organization.

3. John Stern, Chairman of the Policy and Planning (P&P) Board, immediately introduced the P&P report, summarized its provisions, and moved that Council take action on the eight P&P recommendations. After extended discussion, Council voted to approve all eight, as follows:

a. That the draft Bylaws and Articles of Incorporation for an Association of American Psychologists (AAP; *Secretary's note:* name changed later to Association for the Advancement of Psychology) be approved in principle by the Council subject to review and minor modification by the initial Board of Trustees of that organization;

b. That the Board of Directors be requested to appoint an initial 15-person Board of Trustees of the AAP from the list of APA members provided by P&P, the Board of Directors, and accepted nominations from Council;

c. That the initial Board of Trustees of AAP be requested to file articles of incorporation for AAP;

d. That the initial Board of Trustees be requested to convene in early February 1974 to (1) arrange for the election of an additional nine trustees and establish staggered terms of office by lot for all trustees, (2) set priorities for programmatic efforts, (3) employ an Executive Director, and (4) otherwise attend to initial organizational and administrative problems;

e. That a loan of APA funds to AAP be authorized for expenses incurred in Steps b, c, and d, and for conduct of a membership and dues pledge drive among APA members and affiliated psychological associations, such drive to be done at cost;

f. That it be specified in the note of agreement that repayment of the loan will be made from funds collected in Step e above;

g. That the Treasurer of APA be authorized to develop a contract with AAP for provision of business and administrative services, including collection of dues by APA;

h. That a Rule of Council be passed forbidding common membership on the APA Board of Directors and the AAP Board of Trustees, or any like advocacy organization for psychologists with a 501(c)-6 IRS tax-exempt classification.

In connection with h above, the Rule of Council is to be written in such a way that officers of affiliated state associations with a 501(c)-6 status may be elected to the APA Board of Directors or the AAP Board of Trustees.

4. The Ad hoc Committee on APA–CAPPS Relationships was discharged with thanks.

5. Under a voted suspension of the rules, Council received a statement from CAPPS calling for an APA statement in support of CAPPS activity with respect to the suit against Blue Cross/Blue Shield. Council voted that the exact wording of such a statement be left to the Board of Directors, with circulation to Council as soon as possible. The vote was unanimous, and expressed Council sentiment that the language should be in legal form, to protect the legal action by CAPPS, as well as the position of the APA.

N. The following reports were received:

1. Annual Report of the Committee on International Relations in Psychology.

2. Annual Report from the APA Representative to the International Union of Psychological Science.

3. Interim and Final Reports of the Ad hoc Committee on Legislative Affairs.

4. Interim and Final Reports of the Ad hoc Committee on APA–CAPPS Relationships.

5. Final Report of the Ad hoc Committee on P.L. 92-157 and P.L. 92-603.

XIII. CENTRAL OFFICE

A. The following actions were taken during the year in the area of employee benefits:

1. In consideration of the fact that all former Central Office participants in the APA Retirement Plan with John Hancock elected to transfer their funds to TIAA/CREF, and further noting that all funds had been

transferred, the Board voted at its February 1973 meeting to dissolve the APA Retirement Plan Trust and authorized the Executive Officer to notify Meredith P. Crawford, Charles S. Gersoni, and Boris E. Cherney that they were relieved as Trustees and that their services in that capacity were appreciated. At its August 1973 meeting, the Board voted to authorize the Executive Officer to investigate, evaluate, and if appropriate, implement the TIAA/CREF Supplemental Retirement Annuity Plan as an optional feature for staff personnel with the understanding that, if implemented, the option will not entail any cost to APA. At its meeting in January 1974, the Board received a recommendation that the President of the APA convey the Association's concerns about the group health plan offered by TIAA/CREF, a plan which recognizes the services of psychologists only under medical referral, voting to instruct the Executive Officer to implement this suggestion.

2. At its meeting in February 1973, the Board voted that the APA Pay Table for graded employees be revised in two steps to grant pay raises comparable to the federal pay raise with a 3% increase July 1, 1973, and 2.14% January 1, 1974. At its November 1973 meeting, the Board received information about the new federal pay scale, implemented in October 1973, voting to continue the present pay scale for graded personnel in Central Office for 1974 but to adopt a revised scale for 1975.

3. At its May 1973 meeting, the Board considered the appropriateness of its methods for handling grievances in the Central Office. The consensus was that if a hearing committee were required to arbitrate a grievance it would be preferable that it not be made up of members of the Board of Directors.

4. At its November 1973 meeting, the Board received copies of the APA Central Office Affirmative Action Program and a memorandum concerning the Program from the Board of Social and Ethical Responsibility for Psychology (BSERP). The Board of Directors voted its unanimous approval of the Program. The Board noted BSERP's concerns with sympathy, but with respect to specific recommendations, the Board regarded the Program as genuinely responsive to legal and ethical requirements, and reluctantly decided that recommended further activity in special staffing and research is not financially feasible. The Board took note of BSERP's specific suggestions to the Executive Officer concerning proposed goals and objectives for the Program, and observed that the recommended restriction of recruiting for specific positions to minority applicants is contrary to legal requirements and to the spirit of affirmative action in the context of equal opportunity. The Board suggested, with respect to the BSERP recommendation, "that APA initiate, develop, and support programs for the training of psychologists as affirmative action experts," that BSERP consider sponsoring a workshop at the annual convention for psychologists involved in such programs. The Board agreed that research relevant to affirmative action programs should be disseminated, and the Board encouraged BSERP to devote a part of its resources to this purpose. Copies of the Plan are to be widely distributed when it has been officially accepted by the Office of Federal Contract Compliance.

B. The following actions were taken at the January 1974 meeting of Council in the area of Central Office organization:

1. Deferred until the New Orleans meeting a motion to establish a management study task force to study the Central Office and its relation to the APA governing structure, asking Central Office to gather information on possible costs of such a study. The Board of Directors also indicated its plans to study this matter at its May 1974 meeting.

2. Received a motion asking for more information about Central Office pay scales, conditions of employment, etc., and was informed that the information is available in the Central Office Organization and Procedures Manual, copies of which were provided at the meeting.

3. Was informed that the Board of Directors concurred in a motion asking that staff study current operations in Central Office that might be moved to a more outlying location effectively and with net savings, with a report to Council at the New Orleans meetings.

C. Central Office staff developed a new system for the handling of annual reports from boards and committees. Action items from such reports were excerpted for consideration by the Board of Directors and the Council of Representatives, but the reports themselves were assembled in a separate book, also distributed to Council members but not a part of the agenda books, as in the past.

D. The Executive Officer reported the following appointments and terminations of senior staff personnel during 1973: *appointment*, Arthur Centor, Administrative Officer, Professional Affairs; *terminations*, Tena Cummings, Assistant Director, Administrations and Operations; William Learmonth, Associate Director, Psychological Abstracts Development System; Henry Salmon, Manager, Publications Business Services; Richard Caputo, Manager, PAIS Educational Services; Judith Cates, Administrative Associate, Policy Studies; Robert Gorman, Systems Analyst; Frank Whiting, Project Director, Standards for Psychological Service Facilities; Estelle Mallinoff, Editor, Separates; and Gottlieb Simon, Associate Administrative Officer, Professional Affairs.

XIV. FINANCIAL AFFAIRS

A. The Finance Committee, and both the Board of Directors and the Council of Representatives, paid

considerable attention during the year to the need for maintaining appropriate controls over the financial ramifications of decisions made and adopted by various segments of the Association. These discussions led to the following actions, voted by the Council at its August 1973 meeting:

1. A new Rule of Council, 150-1.7, to read as follows:

Any action taken by the Board of Directors during an operating year which would increase the originally budgeted expenses by $2,500 or more in that year will be reported to Council members in writing within thirty days after the action(s) authorized. This notification will include an explanation of the reasons for the action(s) and the manner by which these additional expenses will be financed, e.g., by increased prices for existing products and/or services, sales of new products and/or services, reductions in other activities, or absorbed from surplus.

2. To amend Rule of Council 20-4.1 by the addition of the following sentence:

Any matter involving an expenditure of $15,000 or more shall not be adopted on the day of the Council meeting at which it is introduced but shall be referred to the Finance Committee for evaluation and recommendation in terms of financial feasibility, and the Finance Committee shall report its findings to the Council on the next day that Council meets at which time Council will take formal action

B. The importance of long-range financial planning was also emphasized, and the following action was voted by Council at its January 1974 meeting, upon recommendation of the Finance Committee and the Board of Directors: to establish as the overall Association financial goal for the period 1974–1978 the accumulation of a $1 million liquid reserve by the end of 1978, a reserve that would represent about the equivalent of one month's operating expenses in 1978, where the "liquid reserve" constitutes the Association's equity, exclusive of equity in nonliquid assets (land, building, furniture, and equipment) and, to achieve the goal, that subgoals be assigned to the major programs as follows:

PROGRAM	FIVE-YEAR GOALS (1974–1978)
Services and Activities	Gross contribution[7] amounting to 20% of direct expenses
Publications and Communications	Gross contribution[7] amounting to 20% of direct expenses
All Other (mainly investments)	$500,000 surplus
General Administrative Expense	Shall not exceed approximately 15% of the total direct expenses of the Services and Activities and Publications and Communications programs

To ensure that plans are developed to achieve these goals, Council directed the Board of Directors to assign responsibility to boards and committees and/or the Executive Officer, as appropriate, to develop plans for the period 1974–1978 such plans to be reported to Council by September 1974.

C. The following changes were voted in the 1973 Budget:

1. By the Board of Directors at its February 1973 meeting, an increase of $33,500 in estimated expenses for membership in the American Council of Learned Societies, membership in the Accreditation Council for Facilities for the Mentally Retarded, for increased expense allowance to the Board of Professional Affairs and increases reflected in a new Pay Table for graded employees in Central Office.

2. By Council at its Special Meeting in March 1973, an increase of $59,000 in estimated expenses for a new Student Aid Committee, an additional staff member in the legislative affairs area, a new Committee on Legislative Affairs, mailing tapes to state associations, reimbursement of travel expenses for the Special Meeting, and an Ad hoc Committee on APA–CAPPS Relationships.

3. These changes, as of April 1, 1973, resulted in estimated budgeted revenues of $6,358,800, estimated budgeted expenses of $6,324,400, with a net of revenues over expenses of $34,400.

D. The Executive Officer alerted Council, at its meeting in March 1973, to its obligations in view of the financial crisis facing the Association: 1971 and 1972 were deficit years; 1973 showed a strong possibility of being a deficit year; and an even more serious deficit situation was indicated for 1974, on the basis of early budget planning. Council members were urged to submit their suggestions for meeting this crisis to the Finance Committee and to be prepared for serious consideration of the Preliminary 1974 Budget at the Montreal meetings in August 1973.

There was extended discussion of the Preliminary Budget at Montreal, with general agreement that a key problem is how to develop a system for setting priorities, in view of the limited funds available. Straw votes were taken—essentially negative toward any increase in dues greater than $5, toward special assessments, toward increasing membership by lowering requirements for Member status; essentially positive toward devising a system for Council members to indicate priorities, and to try to increase sales of present and new products and/or services.

The Board of Directors took a number of actions at its November 1973 meeting intended to turn the anticipated deficit in the 1974 Budget into a surplus, including a major reduction in the budgets for boards and committees.

At the January 1974 meeting of Council, the 1974 Budget was adopted, amended to restore the amounts available to boards and committees to the levels spent in 1973, or to 1974 requests, whichever is lower, and

[7] Excess of revenues over direct expenses.

with a reduction in anticipated income of around $15,000, due to the decision to delete the Convention Registration Fees for students. As amended, the 1974 Budget anticipates revenues of $6,164,000, expenses of $6,126,000, and a budgeted excess of revenues over expenses of $38,000.

At the January 1974 meeting, there were two sessions of discussion of the 1974 Budget as a Committee of the Whole. Discussion in the first session emphasized the proposed cuts in board and committee budgets, as well as the way in which the budget for Publications and Communications was presented. In connection with the latter, Council accepted a motion to divorce, in future budget presentations, the allocation of General Administrative Expenses and Transferred Office Expenses from net income and/or deficit figures on a formula basis. The purpose of the motion was to adopt a system of "Direct Costing" as the basis for accounting and budgeting to make clearer the revenue, direct expenses, and, hence, the gross contributions of the several journals and activities of the various programs—particularly "Services and Activities" and "Communications," thereby eliminating arbitrary allocations from the expenses of the programs. Implementation of this was left to the Finance Committee. (The program goals indicated in Item XIV-B of these Proceedings were developed in accordance with the "Direct Costing" motion.)

In the second discussion in Committee as a Whole, a number of suggestions were made of ways to increase income or cut expenses. The Treasurer, in his capacity as Chairman of the Finance Committee, reported that a dues increase as of 1975 could be anticipated, and that the Committee will be considering the level of such a dues increase, also taking into account the various suggestions of Council. Straw votes were taken on a number of them, as follows: essentially against the elimination or reduction of division dues allocations, special assessments for 1974, dues set according to income level, encouragement of members to make APA a beneficiary under life insurance plans; essentially for voluntary contributions solicited from members for specific functions, going to a per diem approach for APA travel (but see below); about even on the idea of an automatic increase in dues tied to a relevant government index.

After rising from the Committee as a Whole, and with further information about various problems connected with adopting a per diem plan for APA travel, Council voted to table the motion, with a request to Central Office to continue ways of reducing travel expense, not only of boards and committees but also of staff.

E. At its Special Meeting in March 1973, Council voted to establish an Ad hoc Committee on Student Aid (COSA), to be appointed by the President and composed of six members of Council knowledgeable about the financial aid needs of students, the Committee to be charged with developing plans for a suitable APA-administered doctoral student aid program (loans and/or scholarships), to which any graduate student in a state-approved or regionally accredited doctoral program in psychology shall have the right to apply for aid. In addition to the six Council members, the President, Executive Officer, Treasurer, and Business Manager of the Association shall be ex officio members of COSA without vote. The Committee was to report its final plans to Council no later than the Montreal meetings and was given a budget of up to $10,000 in 1973. The Committee submitted a progress report in June 1973, and Council, at its August 1973 meeting, voted to authorize COSA to negotiate with the United Student Aid Fund and an insurance company or companies, with a view toward establishing a loan program for doctoral candidates in psychology. Referred to the Finance Committee was a COSA recommendation that a $10 increase in dues be authorized, or a special assessment, effective in 1975, if the above negotiations are successful, to be continued for a period not to exceed three years. At its meeting in January 1974, Council received the final report of the Ad hoc Committee, concurring in the Board of Directors recommendation that the committee be discharged with thanks and the Director of Business Affairs charged to pursue the matter of a Student Loan Fund.

F. At its meeting in November 1973 the Board considered the desirability and feasibility of allowing members to allocate dues as a means of establishing priorities (a motion on this had been tabled at the Council's Special Meeting, and a similar motion had been introduced at the August 1973 meeting). The Board voted to recommend to Council that allocation of dues not be adopted at this time and asked staff to develop a proposal for a method of inquiring from members as to their programs' areas of prime interest, to permit Council to consider these expressions of member interest in arriving at priorities in future budgets. Staff recommended to the Board in January 1974 that the Policy and Planning Board be asked to develop a method of polling members on these issues.

At its meeting in January 1974 Council voted that allocation of member dues (to indicate priorities) not be undertaken at this time, having been informed also that the Finance Committee is planning to develop a procedure for getting priority judgments from Council members, and that the Policy and Planning Board is continuing its planning for a system of soliciting priority judgments from the membership.

G. The Board of Directors, upon recommendation of the Finance Committee, approved the appointment of Ernst and Ernst to conduct the 1973 Audit.

H. The Board of Directors voted to accept the following grants:

1. A continuation grant from the Department of

Health, Education, and Welfare (HEW) for $39,645 to support the project "Psychology Graduate Training Standards and Evaluation," for the period July 1, 1973, to June 30, 1974.

2. A continuation grant from HEW for $72,216 in support of the project "Standards for Psychological Service Facilities," for the period May 1, 1973, to April 30, 1974.

3. A continuation grant from HEW for $11,880 in support of the project "Visiting Psychologists to Field Services" for the period July 1, 1973, to June 30, 1974.

4. A continuation grant from HEW for $89,000 to support the project "Conference on Professional Training in Psychology," for the period July 1, 1973, to June 30, 1974.

5. A grant from HEW for $150,000 to support the project "Headstart Program," for the period June 30, 1973, to June 29, 1974.

I. At its February 1973 meeting, the Board was informed that as of January 10, 1973, the total of $54,000 approved as a loan according to an agreement dated July 23, 1970, between APA and the Black Students Psychological Association (BSPA) had been advanced. Board members expressed the hope that by the termination date, June 30, 1973, a report of BSPA's activities during the three years would be available. As reported in Item IX-C of these Proceedings, the Board voted at its November 1973 meeting that BSPA need not repay the loan.

J. At its May 1973 meeting, the Board of Directors voted to add three Council members to the Finance Committee, on an ad hoc basis for 1973, so that Council would have a larger reservoir of expertise and understanding of APA's financial problems. The three added were David G. McDonald, Samuel H. Osipow, and Robert Perloff. A Rule of Council is to be developed to increase the size of the Committee from four to seven on a continuing basis.

K. At its November 1973 meeting, the Board voted to extend signature authority to the Controller and to rescind previous authority extended to the Manager, Financial and Computer Services Department, by passing the following resolution:

Resolved, that the American Security and Trust Company, Washington, D.C., is hereby continued as a depository for the funds of this Corporation, and is hereby authorized and directed to pay checks and other orders for the payment of money when signed by any one of the following:

Executive Officer	Unlimited
Treasurer	Unlimited
Director of Business Affairs	Unlimited
Controller	Up to $500 on the general account and unlimited on the payroll account.

L. The following reports and minutes were received:

1. The Minutes of its meeting on July 12–13, 1973, were submitted as the Annual Report of the Finance Committee.

2. Interim and Final Reports from the Ad hoc Committee on Student Aid.

3. Final Report of the Ad hoc Committee on APA Dues Structure.

XV. COMMUNICATIONS FROM OUTSIDE ORGANIZATIONS

A. At its February 1973 meeting, the Board of Directors received a request from the American Psychological Foundation (APF) that APA give consideration to increasing the size of its contribution to APF of gross revenues from APA-owned copyright material, now at 20%, to 50%. The Board voted to refer the matter to the Publications and Communication (P&C) Board for a recommendation, and, upon recommendation of the P&C Board, voted at its November 1973 meeting to increase the APF's share of copyright permissions revenues to 40% of the gross (equivalent to a 50–50 split between APA and APF after expenses) to be implemented with calendar year 1975.

B. At its June 1973 meeting, the Board was informed that the APF had been named as the residuary legatee in the will of Esther Katz Rosen, a dues-exempt Fellow of APA who died on April 13, 1973.

C. At its November 1973 meeting, the Board received with thanks a report from the Insurance Trust and also heard from the President of the Trust, taking the following actions with respect to the Trust's recommendations:

1. Voted to amend the Agreement and Declaration of Trust, Article IV, Section 6, as follows: [1]

Section 6. [SEGREGATION OF ASSETS.] *FUND ACCOUNTING.* In the event the Insurance Trustees shall negotiate or otherwise obtain more than one policy of insurance, separate accounting *funds* shall be maintained by such Trustees for each policy [and all monies pertaining thereto shall remain segregated and the investments of such monies shall likewise be segregated by the Investment Trustee] *in order that the assets, liabilities, and equity of each principal policy fund can be separately identified and through which retrospective adjustments and premium refunds attributable to each insurance policy can be recorded.* Notwithstanding any provision to the contrary contained herein, no premium payments or refunds shall be permitted to be made by the Insurance Trustees other than in connection with the specific policy for which said monies are directly attributable. This limitation shall not be construed as restricting the Insurance Trustees from using income derived from the Fund for the purpose of developing new insurance programs. In no event will any income of the Fund be distributed to any participant at any time and if upon termination of the Trust there remains any accumulation of income the same shall be distributed to the American Psychological Association. *In order to segregate the income derived from each principal policy, a separate*

"Income Fund" shall be established into which income derived from dividends and capital gains (and losses) realized by the several principal policy funds shall be credited (or charged) and out of which the expenses of the Insurance Trust will be paid.

2. Voted to elect Gardner Lindzey and Joseph Weitz as new members of the Trust, expressing gratitude to those serving previously on the Trust and with special thanks to Benjamin Shimberg for his long-time dedication and valuable contributions.

3. Heard with interest of the Trust's plans to offer newly elected members (as of 1974) a free $10,000 life insurance policy for a stated period.

D. At its January 1974 meeting, the Board considered a communication from the Association of Gay Psychologists (AGP), including a list of the objectives of that Association as adopted at its Montreal meeting and a list of eight demands it wished APA to implement and publicize. The demands, and the actions voted by the Board, are as follows:

1. "A National Task Force on Homosexuals as Persons shall be appointed by the President of the American Psychological Association in consultation with the AGP with at least 50% of the Task Force being members of the AGP." Referred to the Board of Social and Ethical Responsibility for Psychology.

2. "APA shall advocate the removal of the listing of 'Homosexuality' from the Diagnostic and Statistical Manual II of the American Psychiatric Association." The Board noted that the latter association has already done this.

3. "All panels on homosexuality (at national and regional meetings) shall include at least one member of the AGP as a panel member, and all paper sessions (at national and regional meetings) shall include at least one member of the AGP to serve as discussant." The Board noted that divisions are responsible for developing their programs, and that it would be inappropriate to interfere with their prerogatives. The AGP would need to deal directly with the divisions (and the regional associations) if it wishes to pursue this matter.

4. "APA shall support the elimination of parents' sexual orientation as a factor in decisions relating to parents' rights to rear their own children." The Board felt this demand was outside APA's purview.

5. "APA shall advocate the repeal of all legislation restricting sexual relations between consenting adults." The Board felt this also was outside APA's purview.

6. "APA shall take affirmative action in the elimination of discrimination on the basis of sexual orientation in APA's Central Office and with all companies with which APA does business, and support adding sexual orientation clauses to antidiscrimination laws." The Board noted that to inquire about a person's sexual orientation would be illegal, probably, and in any case quite inappropriate.

7. "APA shall establish a Central Office personnel person to handle all APA members' and associates' charges of discrimination on the basis of sexual orientation." The Board noted that such charges should be filed with the Committee on Academic Freedom and Conditions of Employment, and that the machinery for handling such charges exists.

8. "APA shall include members of the AGP in the Visiting Psychologist Program." Selecting participants in this program is done on the basis of potential contribution; membership in any given association would not be a relevant criterion.

The President, Albert Bandura, agreed to report the Board's actions to AGP.

E. At its February 1973 meeting, the Board noted with concern a misstatement of fact concerning election of Council members to the Board of Directors, appearing in an advertisement in the February 1973 *APA Monitor*, calling for participation in a new organization to be called "Psychologists for a Representative and Responsible APA." The ad stated that the Board of Directors acts as a nominations and elections committee for the election of Council members to the Board, whereas Council itself nominates via a nominating ballot sent to each Council member, followed by an election ballot that asks each Council member to rank order the nominees who have received the most nominations on the previous ballot. The Board asked that the Recording Secretary write a letter to the Committee sponsoring the ad, asking that they publish a retraction, and that the letter be published in the *Monitor* in the form of a letter to the Editor.

F. See Item XII-L for actions with respect to the Council for the Advancement of the Psychological Professions and Sciences (CAPPS).

Officers, Boards, Committees, and Representatives of the American Psychological Association: 1974

APA OFFICERS

President: Albert Bandura (1974) [1]
Stanford University
President-elect: Donald T. Campbell (1974)
Northwestern University
Past President: Leona E. Tyler (1974)
University of Oregon
Recording Secretary: John J. Conger (1974–76)
University of Colorado
Treasurer: Robert Perloff (1974–77)
University of Pittsburgh
Executive Officer: Kenneth B. Little (1968–77)
APA Central Office

BOARD OF DIRECTORS

The President, the President-elect, the Past President, the Recording Secretary, the Treasurer, the Executive Officer, and:

Herbert C. Kelman (1972–74)
Leonard Krasner (1973–74)
Richard C. Atkinson (1973–75)
M. Brewster Smith (1973–75)
John I. Lacey (1974–76)
Rogers Wright (1974–76)

COUNCIL OF REPRESENTATIVES

The Council of Representatives is composed of the Board of Directors, Division Representatives, and State Association Representatives. The Representatives for the current year, with term of office, follow:

Norman Abeles (1973–75)—Michigan
George W. Albee (1973–75)—Div. 12
Richard C. Atkinson (1973–75)—Board of Directors
Albert Bandura (1974)—President
Allan G. Barclay (1972–74)—Div. 12
Morton Bard (1974–76)—Div. 27
Jules Barron (1974–75)—Div. 29
H. T. Bassett (1973–75)—Wisconsin
Josephine P. Beebe (1973–75)—Connecticut
Ernst G. Beier (1972–74)—Utah/Wyoming/Montana/New Mexico
Gershon Berkson (1974–76)—Div. 33
Sidney W. Bijou (1972–74)—Div. 12
Theodore H. Blau (1973–75)—Div. 12
Lyle E. Bourne, Jr. (1972–74)—Div. 3
Donald T. Campbell (1974)—President-elect
Irvin L. Child (1974–76)—Div. 10
Richard K. Clampitt (1974–76)—Minnesota
John J. Conger (1974–76)—Recording Secretary
Jonathan W. Cummings (1973–75)—District of Columbia
Nicholas A. Cummings (1973–75)—California
Mary Ellen Curtin (1972–74)—Kentucky/Tennessee
Robert C. Davis (1972–74)—Div. 26
Gordon F. Derner (1974–76)—Div. 12
Martin Deutsch (1972–74)—Div. 9
James A. Dinsmoor (1972–74)—Div. 25
Francis J. DiVesta (1972–74)—Div. 16
Herbert Dörken (1974–76)—California
Jan D. Duker (1973–74)—Div. 16
Frank W. Finger (1973–75)—Div. 3
Jo-Ann E. Gardner (1974–76)—Div. 9
Sol L. Garfield (1973–75)—Div. 12
Marcia Guttentag (1974–76)—Div. 8
Elizabeth P. Hagen (1973–75)—Div. 5
Leon P. Hall (1973–75)—Div. 16
Florence C. Halpern (1973–75)—Div. 12
Carmi Harari (1974)—Div. 32
Robert S. Harper (1972–74)—Div. 2
Harry Helson (1974–76)—Div. 1
Joseph S. Herrington (1972–74)—Pennsylvania
Lloyd G. Humphreys (1973–75)—Div. 3
Irwin A. Hyman (1974–76)—Div. 16
Ira Iscoe (1972–74)—Texas
Nelson F. Jones (1972–74)—Colorado
John E. Jordan (1972–74)—Div. 22
Frederick H. Kanfer (1972–74)—Div. 12
Herbert C. Kelman (1972–74)—Board of Directors
Wallace Kennedy (1974)—Georgia/North Carolina/South Carolina/Florida
Oliver J. B. Kerner (1973–75)—Illinois
Jane W. Kessler (1973–75)—Ohio
Marc Kessler (1974–76)—Maine/New Hampshire/Rhode Island/Vermont

[1] Terms of office have been adjusted in all cases to reflect calendar years.

Milton V. Kline (1972–74)—Div. 30
Arthur L. Kovacs (1972–74)—Div. 29
Leonard Krasner (1973–74)—Board of Directors
John I. Lacey (1974–76)—Board of Directors
Bibb Latané (1972–74)—Div. 8
Edward E. Lawler (1973–75)—Div. 14
Hannah A. Levin (1972–74)—Div. 27
James A. Lindemann (1973–75)—Oregon
Lewis P. Lipsitt (1972–74)—Div. 7
Kenneth B. Little (1968–77)—Executive Officer
Frank A. Logan (1974)—Div. 3
Joseph P. Margolin (1974)—Div. 13
Fred Massarik (1973–75)—Div. 32
Harry V. McNeill (1972–74)—Div. 18
Merle L. Meacham (1972–74)—Div. 16
Naomi M. Meara (1974–76)—Div. 17
Nancy K. Mello (1972–74)—Div. 28
Ronald Melzack (1973–75)—Div. 6
Marvin Metsky (1973–75)—New Jersey
Jack L. Michael (1973–74)—Div. 25
Bernice L. Neugarten (1974–76)—Div. 20
Donald P. Ogden (1973–75)—Virginia/West Virginia
Samuel H. Osipow (1972–74)—Div. 17
Ellis B. Page (1973–75)—Div. 15
Robert Perloff (1974–77)—Treasurer
Beeman N. Phillips (1973–75)—Div. 16
Ann D. Pick (1974–76)—Div. 7
Harold D. Proshansky (1974–76)—Div. 9
Bertram H. Raven (1974–76)—Div. 8
Henry D. Remple (1972–74)—Kansas
W. W. Renke (1972–74)—Indiana
Stanley P. Rosenzweig (1973–75)—Massachusetts
Ivan Ross (1973–74)—Div. 23
Herbert S. Roth (1972–74)—Iowa
Julian B. Rotter (1974–76)—Div. 12
S. Jay Samuels (1972–74)—Div. 15
Theodore R. Sarbin (1972–74)—Div. 24
Kenneth B. Schenkel (1973–75)—Georgia/North Carolina/South Carolina/Florida
William N. Schoenfeld (1974–76)—Div. 25
S. Don Schultz (1973–74)—California
John W. Senders (1972–74)—Div. 21
Max Siegel (1974–76)—Div. 29
M. Brewster Smith (1973–75)—Board of Directors
Janet T. Spence (1974–76)—Div. 3
Ezra Stotland (1974)—Div. 9
Mary L. Tenopyr (1974–76)—Div. 14
Richard F. Thompson (1974–76)—Div. 6
Carl E. Thoresen (1972–74)—Div. 17
Laura C. Toomey (1973–75)—Div. 31
Ralph H. Turner (1974–76)—Div. 2
Leona E. Tyler (1974)—Past President
J. E. Uhlaner (1972–74)—Div. 19
Victor H. Vroom (1973–75)—Div. 14
Elaine C. Walster (1972–74)—Div. 8
Gerald A. Whitmarsh (1974–76)—Maryland
Walter H. Wilke (1972–74)—New York
Robert L. Wrenn (1973–75)—Arizona/Oklahoma
Rogers H. Wright (1974–76)—Board of Directors
Charles F. Wrigley (1974–76)—Div. 5
Francis A. Young (1974–76)—Alaska/Idaho/Washington
Carl N. Zimet (1974–76)—Div. 29
Donald G. Zytowski (1974–76)—Div. 17

Edwin B. Newman—Parliamentarian

To be elected:
Alabama/Arkansas/Louisiana/Mississippi (1)
Missouri (1)
New York (1)
Div. 14 (1)
Div. 29 (1)

COMMITTEES REPORTING DIRECTLY TO COUNCIL OF REPRESENTATIVES

Committee on Structure and Function of Council

Marcia Guttentag (1972–74), Chair
Lyman W. Porter (1972–74)
Mary Ellen Curtin (1973–75)
Nelson F. Jones (1973–75)
Frank W. Finger (1974–76)
Ellis B. Page (1974–76)

Ad hoc Committee on Division–APA Relationships

Charles F. Wrigley (1974)
Jules Barron (1974)
Robert Perloff (1974)
John A. Stern (1974)
Francis J. DiVesta (1974)
John W. Senders (1974)
Harold M. Proshansky (1974)
Gordon F. Derner (1974)

BOARDS, COMMITTEES, AND REPRESENTATIVES TO OTHER ORGANIZATIONS REPORTING DIRECTLY THROUGH THE BOARD OF DIRECTORS

STANDING BOARDS AND COMMITTEES

Membership Committee

Edwin A. Fleishman (1972–74)
Clifford H. Swensen, Jr. (1972–74), Chair
E. Lowell Kelly (1973–75)
Arthur J. Riopelle (1973–75)

Norman L. Farberow (1974–76)
Fred E. Fiedler (1974–76)

Finance Committee

William McGehee (1967–74)
Marilee U. Fredericks (1972–74)
Dorothy Eichorn (1973–75)
Beeman N. Phillips (1974–75)
Harold Proshansky (1974–76)
William N. Schoenfeld (1974–76)
Robert Perloff (1974–77), Chair

Board of Convention Affairs

Murray Glanzer (1968–74)
Lee Sechrest (1970–75)
Henry Tomes, Jr. (1971–76)
Carl N. Zimet (1972–77), Chair
Doris R. Entwisle (1973–78)
Kay Standley (1974–79)
Theodore G. Driscoll, Jr. (1964), ex officio, Convention Manager

Committee on Scientific and Professional Ethics and Conduct

Carolyn R. Payton (1971–74)
Stanley D. Imber (1972–74)
Jane W. Kessler (1972–74)
Douglas W. Bray (1970–75), Chair
Carlos Albizu-Miranda (1973–75)
Leonard P. Ullmann (1971–76)
Stanley Seashore (1974–76)

Election Committee

Kenneth B. Clark (1972–74)
Anne Anastasi (1973–75)
Leona E. Tyler (1974–76), Chair

Policy and Planning Board

George W. Albee (1972–74)
John A. Stern (1972–74), Chair
Raymond A. Bauer (1973–74)
A. Angus Campbell (1973–75)
Isidor Chein (1973–75)
Edward Zigler (1973–75)
Eleanor Gibson (1974–76)
Brendan A. Maher (1974–76)
Wilbert J. McKeachie (1974–76)

Publications and Communications Board

Elaine C. Walster (1972–74)
Albert H. Hastorf (1970–75)
Robert M. Gagné (1973–75)
Arthur W. Melton (1971–76), Chair
Kenneth E. Clark (1973–76)
E. Mavis Hetherington (1972–77)
James Bieri (1973–78)
Gregory Kimble (1973–78)
Robert S. Daniel (1974–79)
Kenneth B. Little (1968–77), ex officio
Robert Perloff (1974–77), ex officio
Harold P. Van Cott (1971), Managing Editor

Education and Training Board

Alexander W. Astin (1972–74), Chair
Forrest B. Tyler (1972–74)
Paul J. Woods (1972–74)
Leslie H. Hicks (1973–75)
Richard Schmuck (1973–75)
Michael M. Wertheimer (1973–75)
Louis D. Cohen (1974–76)
Peter M. Lewinsohn (1974–76)
Virginia Senders (1974–76)

Board of Professional Affairs

J. Wilbert Edgerton (1971–74)
Vera S. Paster (1971–74)
Allan G. Barclay (1972–74), Chair
Jeanne S. Phillips (1972–75)
Eva R. Laskin (1973–75)
Floyd Martinez (1973–75)
Ruth M. Lesser (1974–76)
Asher R. Pacht (1974–76)
Mary L. Tenopyr (1974–76)

Board of Scientific Affairs

Herbert L. Pick (1972–74)
Irwin Altman (1973–74)
Lyle E. Bourne, Jr. (1973–75)
Edward E. Jones (1973–75)
Patricia Meyer (1973–75)
Jerome Singer (1974–75), Chair
Alphonse Chapanis (1974–76)
Sally Sperling (1974–76)
Robert B. Zajonc (1974–76)

Board of Social and Ethical Responsibility for Psychology

Helen Nash (1973–74)
Michael Scriven (1973–74)
Kenneth B. Clark (1973–75)
Kenneth Dinklage (1973–75)
Bernard Harleston (1973–75), Chair
E. Belvin Williams (1973–75)

David Bakan (1974–76)
William E. Henry (1974–76)
June L. Tapp (1974–76)

OTHER COMMITTEES

Committee on Constitutional Issues

Kenneth B. Clark (1972–74)
Anne Anastasi (1973–75)
Leona E. Tyler (1974–76), Chair

Committee on International Relations in Psychology

George V. Coelho (1972–74)
Ivan Mensh (1972–74), Chair
Stanley Schachter (1972–74)
Michael Cole (1973–75)
Bernice Wenzel (1973–75)
Albert H. Yee (1973–75)
C. R. Myers (1974–76)
Rafael Nunez (1974–76)
Sherman Ross (1974–76)
Nicholas Hobbs (1970–76), ex officio
Kenneth B. Little (1968–77), ex officio
Mary Ainsworth (1973–77), ex officio
Henry David (1974–78), ex officio

Committee on Legislative Affairs

Theodore H. Blau (1974)
Lloyd Humphreys (1974–75)
Marcia Guttentag (1974–76)
Herbert O. Dörken, Chair of COHI
E. Belvin Williams, Chair of Committee on Psychological Tests and Assessments
Forrest B. Tyler, Member of Education and Training Board
Kenneth Dinklage, Member of Board of Social and Ethical Responsibility in Psychology
Herbert Pick, Member of Board of Scientific Affairs
Allan Barclay, Member of Board of Professional Affairs
Isador Chein, Member of Policy and Planning Board

Ad hoc Committee on Archival History

Charles W. Bray (1966), Chair
Jane D. Hildreth, ex officio

Ad hoc Committee on Manpower–Human Resources

George W. Albee (1971), Chair
Kenneth E. Clark (1971)
J. Frank Whiting (1971)
Virginia Douglas (1972)

REPRESENTATIVES TO OTHER ORGANIZATIONS

International Union of Psychological Science

Mary D. Ainsworth (1973–77), Senior Delegate
Henry P. David (1974–78)

United States Commission for UNESCO

Nicholas Hobbs (1970–75)

American Council of Learned Societies

Theodore R. Sarbin (1974)

Scientific Manpower Commission

Robert Lockman (1974)
C. Alan Boneau (1974)

COMMITTEES REPORTING THROUGH THE BOARD OF CONVENTION AFFAIRS

Operations Committee

Henry Tomes, Jr. (1972–74)
Carl N. Zimet (1973–74), Chair
Doris R. Entwisle (1974)

Committee on Films and Other Media

Ruth Formanek (1971–74)
Richard I. Evans (1971–75), Chair
William Dember (1973–76)

Committee on Program Innovations

Carolyn Sherif (1972–74), Chair
Ernst G. Beier (1972–75)
Bruce R. Ekstrand (1974–76)

Committee on Research and Evaluation

Gerald M. Senf (1971–74), Chair
David L. Hamilton (1973–75)
William A. Hayes (1974–76)

Task Force on Convention Child Care

Dorothy J. Sciarra (1974)
Kay Standley (1974)
Castellano B. Turner (1974)
Raymond K. Yang (1974)

COMMITTEES AND REPRESENTATIVES TO OTHER ORGANIZATIONS REPORTING THROUGH THE PUBLICATIONS AND COMMUNICATIONS BOARD

COMMITTEES

Communications Committee

[illegible]
Darwyn Linder (1973–76), Chair
[illegible]
Mark [illegible]
[illegible] Webb (1973–75)
[illegible]
[illegible]
[illegible]
[illegible] S. Nuberson (1974–76)
[illegible]

Council of Editors

The Council of Editors consists of editors of the APA journals.

[illegible] Kenneth B. Little [illegible]
[illegible]
[illegible] Daniel Miron[illegible]
[illegible]
[illegible] Journal of Abnormal Psychology Leonard D. [illegible]
[illegible] Journal of Applied Psychology Edwin A. Fleishman [illegible]
[illegible] Journal of Comparative and Physiological Psychology [illegible]
[illegible] Journal of Consulting and Clinical Psychology Brendan A. Maher [illegible]
[illegible] Journal of Counseling Psychology Ralph F. Berdie [illegible]
[illegible] Journal of Educational Psychology Joanna Williams [illegible]
[illegible] Journal of Experimental Psychology David A. Grant [illegible]
[illegible] Learning and Memory Lyle E. Bourne Jr. [illegible]
[illegible] Journal Contents Service [illegible]
JRSP Perception and Psychophysics Michael I. Posner [illegible]
[illegible] Journal Behavior Therapy Allen R. Wagner [illegible]
[illegible] Journal of Personality and Social Psychology John T. [illegible]
[illegible] Psychonomic Monitor James F. Dowse [illegible]
[illegible] Richard I. Henneman [illegible]
Psychonomic Review George Mandler [illegible]
Psychonomic Scientific Donald K. [illegible]
[illegible]
Psychonomic Corporate Robert G. Kirkpatrick [illegible]
Executive Editor
Managing Editor. Harold P. Van Cott (1971–)

REPRESENTATIVES TO OTHER ORGANIZATIONS

National Federation of Science Abstracting and Indexing

Harold P. Van Cott (1971–)

COMMITTEES AND REPRESENTATIVES TO OTHER ORGANIZATIONS REPORTING THROUGH THE BOARD OF PROFESSIONAL AFFAIRS

COMMITTEES

Committee on Health Insurance

A. Eugene Shapiro [illegible]
Herbert [illegible] Chair
Jack [illegible]
[illegible]
David Rodgers [illegible]

Committee on State Legislation

[illegible]
Ronald R. Fox (1973–75), Co-chair
Stanley Moldawsky (1972–76), Co-chair
Morton L. Fine (1973–77)
William Dunn (1974–78)

Committee on Professional Awards

[illegible]
[illegible]
[illegible]
[illegible]
[illegible]

Committee on Specialty Practice

[illegible]
[illegible]
Ralph [illegible] Chair
Murray [illegible]
[illegible]
Richard Weinberg [illegible]

[illegible]

Sandi Bliss (1974)
[illegible] G. Spence (1974)
Richard [illegible] (1974-75)
[illegible] Moyne (1974-75)
Richard Ross (1974-76)
Charles F. Mahoney (1974-76)
[illegible] (1974-76)

Ad hoc Committee on Promoting Public Interest Activities

Lorraine Hyde (1974)
[illegible] (1974)
Larry Johnson (1974)
Lloyd Martinez (1974)
Raphael Minsky (1974)
Carolyn Payton (1974)

Task Force on Sex Bias and Sex Role Stereotyping in Psychotherapeutic Practice

To be appointed

Task Force on Behavioral Classification

Theodore H. Blau (1968)
Mortimer Brown (1968)
Martin M. Katz (1968)
Maurice Lorr (1968)
J. R. Newbrough (1968), Chair

Task Force on Standards for Service Facilities

Milton L. Blum (1970)
Jacqueline C. Bouhoutsos (1970)
Jerry Clark (1970)
Harold A. Edgerton (1970)
Morton D. Hall (1970)
Durand F. Jacobs (1970), Chair
Floyd Martinez (1970)
John Muthard (1970)
Asher Pacht (1970)
William Pierce (1970)
Sue A. Warren (1970)
Alfred M. Wellner (1970)

Task Force on Psychological Services of Law Enforcement Agencies

William J. Deering (1974)
[illegible] McAfee (1974), Chair
Robert Proctor (1974)
[illegible] M. [illegible] (1974)
Fred Spencer (1974)

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] Wright [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] Mental Health [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

Raymond A. Katzell (1974–75)
Robert L. Linn (1974–76)
E. Belvin Williams (1974), Chair

COMMITTEE ON SCIENTIFIC AWARDS

R. Duncan Luce (1971–74)
Brendan Maher (1972–74)
Jerome E. Singer (1972–74)
James J. Jenkins (1973–75)
Harold H. Kelley (1973–75), Chair
Dorothea J. Hurvich (1974–77)

AD HOC COMMITTEE ON INSTRUCTIONAL PROGRAMS

Richard C. Atkinson (1971)
Theodore H. Blau (1971)
John M. Darley (1971)
Arthur Lumsdaine (1971), Chair
Susan M. Markle (1971)

AD HOC COMMITTEE ON NEWLY EMERGING AREAS OF RESEARCH

Herbert L. Pick (1974)
Lyle E. Bourne (1974–76)
Sally E. Sperling (1974–77)

TASK FORCE ON ENVIRONMENT AND BEHAVIOR

Irwin Altman (1974), Chair
Robert L. Helmreich (1974)
Henry M. Parsons (1974)
Edwin P. Willems (1974)
Joachim F. Wohlwill (1974)

TASK FORCE ON HEALTH RESEARCH

Claus B. Bahnson (1974)
Edward J. Kelty (1974)
John E. Rasmussen (1974)
William Schofield (1974), Chair
Lee B. Sechrest (1974)
Lisa Shusterman (1974)
Walter L. Wilkins (1974)

REPRESENTATIVES TO OTHER ORGANIZATIONS

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Jerome E. Singer (1974–76)

INTER-SOCIETY COLOR COUNCIL

Leo M. Hurvich (1955–74), Voting Delegate
William R. Biersdorf (1966–76)
Sherman L. Guth (1967–77)
Sidney Stecher (1972–77), Voting Delegate, Chair
Lorrin A. Riggs (1968–77)
Robert Boynton (1974–79
Harry Helson (1970–80)
Jo Ann S. Kinney (1974–80)

NATIONAL RESEARCH COUNCIL

Frances K. Graham (1971–74)
Hershel W. Leibowitz (1972–75)

SOCIAL SCIENCE RESEARCH COUNCIL

Gardner Lindzey (1963–74)
Edward E. Jones (1973–75)
Janet T. Spence (1974–77)

NATIONAL SOCIETY FOR MEDICAL RESEARCH

Winfred F. Hill (1974)

COMMITTEES AND REPRESENTATIVES TO OTHER ORGANIZATIONS REPORTING THROUGH THE EDUCATION AND TRAINING BOARD

COMMITTEES

COMMITTEE ON ACCREDITATION

Barbara Kirk (1972–74)
Beeman N. Phillips (1972–74)
William L. Simmons (1972–74), Chair
Irwin Altman (1973–75)
Marilee U. Fredericks (1973–75)
Robert Schaefer (1973–75)
Floyd Martinez (1974–76)
Charles Thomas (1974–76)
Alfred Wellner (1974–76)

COMMITTEE ON PRECOLLEGE PSYCHOLOGY

Delbert G. Eberhardt (1972–74)
Robert H. Koff (1972–74)
Eli Bower (1973–75)
Jean La-Yvonne Valletta (1973–75)
John Bare (1974–76)
Frank Hawkinshire (1974–76)
Mary N. Moffett (1974–76)

Ad hoc Advisory Committee on the Visiting Psychologist Program

William D. Pierce (1970)
Arthur N. Wiens (1970), Chair
Ruth M. Lesser (1971)
Stanley L. Brodsky (1972)
Mildred R. Buck (1972)

Ad hoc Committee on the Two-Year College

John K. Bare (1971)
Robert S. Daniels (1971)
Judith Kuppersmith (1971)
Fay Tyler M. Norton (1971)
Janice Page (1971)
Thomas E. McCloud (1972)
Ralph J. Schilloce (1972)
Aiken Connor, Consultant

Task Force on Evaluation of Graduate Training

Alexander Astin (1974)
Lewis Goldberg (1974)
Barbara Kirk (1974)
Robert Perloff (1974)
Leonard Rorer (1974)

Steering Committee for Secondary School Curriculum Project on Human Behavior

Anne Anastasi (1973)
Wendell Bell (1973)
Launor F. Carter (1973)
Fred Eggan (1973)
Robert M. Gagné (1973)
H. Bentley Glass (1973)
Donald O. Hebb (1973)
Ernest R. Hilgard (1973)
James J. Jenkins (1973)
Jerome Kagan (1973)
Gardner Lindzey (1973)
Helen Nash (1973)
Carl Pfaffmann (1973)
Janet Spence (1973)

Vail Conference Follow-up Commission

Douglas Bray (1972)
John G. Darley (1972), Chair
Thomas Hilliard (1972)
Erasmus L. Hoch (1972)
Miriam Keiffer (1972)
Maurice Korman (1972)
Abel Ossorio (1972)
Karl Pottharst (1972)
Alan O. Ross (1972)
Shalom Vineberg (1972)

REPRESENTATIVES TO OTHER ORGANIZATIONS

National Commission on Accrediting

Ronald Kurz (1970)

American Council on Education

John A. Stern (1970)

Joint Commission on Teaching of Science and Mathematics

James R. Nazzaro (1972)

COMMITTEES AND REPRESENTATIVES TO OTHER ORGANIZATIONS REPORTING THROUGH THE BOARD OF SOCIAL AND ETHICAL RESPONSIBILITY FOR PSYCHOLOGY

COMMITTEES

Committee on Academic Freedom and Conditions of Employment

Sylvia M. Obradovic (1970–74)
Edward J. Casavantes (1971–75)
Leonard G. Rorer (1972–76), Chair
Edwin B. Newman (1973–77)
Joy D. Osofsky (1974–78)

Committee on Equality of Opportunity in Psychology

Jacqueline Jackson (1972–74), Chair
R. Nevitt Sanford (1972–74)
René A. Ruiz (1973–75)
Charles M. Waddell (1973–75)
Carl Eisdorfer (1974–76)
Marigold Linton (1974–76)

Committee on Women in Psychology[2]

Helen S. Astin (1974)
Ellen Kimmel (1974)

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[2] Formerly the Ad hoc Committee on the Status of Women in Psychology

Tena Cummings (1974–75)
Martha Mednick (1974–75), Chair
Tressie W. Muldrow (1974–76)
Eli Rubenstein (1974–75)
James Deese (1974–76)

OFFICERS OF DIVISIONS [a]

1. Division of General Psychology

President: Daniel E. Berlyne
President-elect: Leo J. Postman
Past President: Wilbert J. McKeachie
Secretary-Treasurer: Virginia W. Voeks (1965–74)
Members-at-large of Executive Committee:
William S. Verplanck (1971–74)
Edward L. Walker (1971–74)
Delos D. Wickens (1973–76)
Representative to APA Council:
Harry Helson (1974–76)

2. Division on the Teaching of Psychology

President: Wilbert S. Ray
President-elect: James B. Maas
Past President: John K. Bare
Secretary-Treasurer: Lawrence E. Murphy (1972–75)
Representatives to APA Council:
Robert S. Harper (1972–74)
Ralph H. Turner (1974–76)

3. Division of Experimental Psychology

President: Richard C. Atkinson
President-elect: Wendell R. Garner
Secretary-Treasurer: Frank A. Logan (1973–76)
Members-at-large of Executive Committee:
Victor H. Denenberg (1971–74)
Arthur J. Riopelle (1972–75)
Donald R. Meyer (1973–76)
Representatives to APA Council:
Lyle E. Bourne, Jr. (1972–74)
Frank A. Logan (1974)
Frank W. Finger (1973–75)
Lloyd G. Humphreys (1973–75)
Janet T. Spence (1974–76)

[a] Terms of office for all Council Members now reflect the calendar years during which they have served or will serve. Terms of office for other Division Officers are still shown as starting during a year and ending during a year, usually with a term of three years.

5. Division of Evaluation and Measurement

President: Samuel J. Messick
President-elect: William G. Mollenkopf
Secretary-Treasurer: Elizabeth P. Hagen (1972–75)
Members-at-large of Executive Committee:
Benjamin Fruchter (1973–74)
Ledyard R Tucker (1972–75)
John W. French (1973–76)
Representatives to APA Council:
Elizabeth P. Hagen (1973–75)
Charles F. Wrigley (1974–76)

6. Division of Physiological and Comparative Psychology

President: Brenda A. Milner
President-elect: Donald R. Meyer
Secretary-Treasurer: Robert L. Isaacson (1971–74)
Members-at-large of Executive Committee:
Elliot S. Valenstein (1971–74)
William A. Wilson (1972–75)
Richard M. Held (1973–76)
Representatives to APA Council:
Ronald Melzack (1973–75)
Richard F. Thompson (1974–76)

7. Division of Developmental Psychology

President: Alberta E. Siegel
President-elect: Edward F. Zigler
Secretary-Treasurer: Lucy R. Ferguson (1973–76)
Members-at-large of Executive Committee:
Herbert L. Pick (1971–74)
Virginia C. Crandall (1972–75)
David S. Palermo (1973–76)
Representatives to APA Council:
Sidney W. Bijou (1972–74)
Lewis P. Lipsitt (1972–74)
Ann D. Pick (1974–76)

8. Division of Personality and Social Psychology

President: Urie Bronfenbrener
President-elect: Paul F. Secord
Secretary-Treasurer: Marcia Guttentag (1973–76)
Representatives to APA Council:
Bibb Latané (1972–74)
Elaine C. Walster (1972–74)
Marcia Guttentag (1974–76)
Bertram H. Raven (1974–76)

9. The Society for the Psychological Study of Social Issues—A Division of the APA

President: Bertram H. Raven
President-elect: Albert Pepitone

Secretary-Treasurer: John P. Kirscht
Members-at-large of Executive Committee:
Kenneth B. Clark (1972–74)
Mark Pilisuk (1972–74)
Jack Sawyer (1972–74)
June L. Tapp (1972–74)
Harry Triandis (1972–74)
Reuben M. Baron (1973–75)
Lois W. Hoffman (1973–75)
David Levine (1973–75)
Martha S. Mednick (1973–75)
Irwin G. Sarason (1973–75)
Representatives to APA Council:
Martin Deutsch (1972–74)
Ezra Stotland (1974)
Jo-Ann E. Gardner (1974–76)
Harold M. Proshansky (1974–76)

10. Division of Psychology and the Arts

President: Frederick Wyatt
President-elect: Daniel E. Berlyne
Secretary-Treasurer: Robert W. Lundin (1966–75)
Members-at-large of Executive Committee:
Frank Barron (1971–74)
Marianne L. Simmel (1972–75)
Evelyn Perloff (1973–76)
Representative to APA Council:
Irvin L. Child (1974–76)

12. Division of Clinical Psychology

President: Theodore H. Blau
President-elect: Hans H. Strupp
Secretary-Treasurer: Allan G. Barclay (1973–76)
Representatives to APA Council:
Allan G. Barclay (1972–74)
Frederick Kanfer (1972–74)
George W. Albee (1973–75)
Theodore H. Blau (1973–75)
Sol L. Garfield (1973–75)
Florence C. Halpern (1973–75)
Gordon F. Derner (1974–76)
Julian B. Rotter (1974–76)

13. Division of Consulting Psychology

President: Fred E. Spaner
President-elect: Malcolm L. Meltzer
Past President: Gordon F. Derner
Secretary-Treasurer: Doris R. Kraemer (1972–75)
Members-at-large of Executive Committee:
Doris R. Kraemer (1970–75)
Fred E. Spaner (1971–74)
Robert A. Harper (1972–75)
John R. Barry (1973–76)
John J. Brownfain (1973–76)
Representative to APA Council.
Joseph B. Margolin (1974–75)

14. Division of Industrial and Organizational Psychology

President: Edwin A. Fleishman
President-elect: Donald L. Grant
Past President: Robert M. Guion
Secretary-Treasurer Paul W. Thayer (1972–75)
Members-at-large of Executive Committee:
Wayne K. Kirchner (1971–74)
Frank Friedlander (1972–75)
John P. Campbell (1973–76)
Representatives to APA Council
To be elected (1974)
Edward E. Lawler (1973–75)
Victor H. Vroom (1973–75)
Mary L. Tenopyr (1974–76)

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Joe L. Byers (1972–75)
Robert L. Grinder (1973–76)
Richard E. Ripple (1973–76)
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S. Jay Samuels (1972–74)
Ellis B. Page (1973–75)

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Joseph L. French (1972–75)
Margaret M. Bernauer (1973–76)
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Jan D. Duker (1973–74)
Leon P. Hall (1973–75)
Beeman N. Phillips (1973–75)
Irwin A. Hyman (1974–76)

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Treasurer: Norman L. Kagan (1971–74)
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Thomas M. Magoon (1972–75)
Lyle D. Schmidt (1973–76)
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Carl E. Thoresen (1972–74)
Naomi M. Meara (1974–76)
Donald G. Zytowski (1974–76)

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Neil W. Coppinger (1972–75)
James G. Kelly (1973–76)
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Frances M. Carp (1973–76)
Merrill E. Elias (1973–76)
Morton A. Lieberman (1973–76)
Representative to APA Council:
Bernice L. Neugarten (1974–76)

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Jerome Siller (1971–74)
Leonard Diller (1972–75)
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Franklin G. Shontz (1973–76)
Shalom E. Vineberg (1973–76)
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William D. Wells (1972–75)
Alin Gruber (1973–76)
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Ivan Ross (1973–74)

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Herman Feifel (1973–75)
Francis T. Severin (1973–76)
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Vice President: Roger E. Ulrich (1973–76)
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Montrose M. Wolf (1971–74)
Ellen P. Reese (1972–75)
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Jack L. Michael (1973–75)
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Morton Bard (1974–76)

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Donald A. Overton (1972–75)
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Victor Raimy (1972–75)
Nicholas Cummings (1973–76)
Alvin A. Mahrer (1973–76)
Rosalea A. Schonbar (1973–76)
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To be elected (1974)
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Jules Barron (1974–75)
Robert A. Harper (1974–75)
Max Siegel (1974–76)
Carl N. Zimet (1974–76)

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31. Division of State Psychological Association Affairs

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Janette Rainwater (1973–74)
Margaret Jorn Fagan (1972–75)
Robert D. Mann (1972–75)
Monroe G. Gottesgen (1973–75)
Bernard S. Aaronson (1973–76)
Stanley R. Graham (1973–76)
Nora Weckler (1973–76)
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Fred Massarik (1973–75)

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Legislation: A. Jack Turner
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Professional Affairs: J. Wesley Libb

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Department of Psychology
Trinity College
Hartford, Connecticut 06106
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Legislation: Allan Shiffer
Insurance: Same as Legislation

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Wilmington, Delaware 19801
Treasurer: Albert E. Conway
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Overholser Division
Saint Elizabeths Hospital
Washington, D.C. 20032
Treasurer: Joan A. Lorr
Council Representative: Jonathan W. Cummings

Newsletter: Charles A. Ullmann
Ethics: Sherman Nelson
Legislation: Lee Gurel
Insurance: Melvin A. Gravitz

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Treasurer: Espy D. Ball
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Newsletter: Alan Gessner
Ethics: Juanita H. Williams
Legislation: Miles W. Hardy
Insurance: Erwin Lessor
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North Georgia Unit
Central State Hospital
Milledgeville, Georgia 31062
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Legislation: Sidney Q. Janus
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Honolulu, Hawaii 96822
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Ethics: Jerome Boyar
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Katherine Hamilton Mental Health Center
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Professional Affairs: Thomas B. Orr

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Ethics: Roy E. Warman
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Council Representative: Henry D. Remple
Newsletter: James W. D. Hartman

Ethics: Terry D. Keeley
Legislation: Jack W. Martin
Insurance: Jack W. Martin
Professional Affairs: William H. Cadman

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West Central Mental Health Center
730 West Market Street
Louisville, Kentucky 40200
Treasurer: William H. Watkins
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Newsletter: David Rush
Ethics: David Wright
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Department of Psychology
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Professional Affairs: Same as Insurance

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Professional Affairs: René A. Ruiz

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Legislation: John G. Watkins
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Legislation: Howard J. Slepian
Insurance: Muriel Fox

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Ethics: Robert McCarthy
Professional Affairs: Same as Ethics

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Howard M. Cohen
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Professional Affairs: John C. Brantley

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Legislation: Harold Hase
Professional Affairs: John Wallace

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Ethics: Ed Wagner
Legislation: Solveig Wenar
Insurance: Jack Wiggins
Professional Affairs: Arthur MacKinney

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Insurance: Richard E. Sternlof

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Ethics: Sheila Link
Legislation: Richard Lazere
Insurance: Same as Legislation
Professional Affairs: Same as Legislation

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Newsletter:
Zita Levin
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Professional Affairs: Laura Leticia Herrans

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Legislation:
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Johnson City, Tennessee 37684
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National Conference on Levels and Patterns of Professional Training in Psychology

The Major Themes

MAURICE KORMAN *University of Texas Health Science Center at Dallas* [1]

Since the Chicago Conference eight years ago, American psychology has felt the effects of severe professional and social crosswinds, bringing with them serious, seemingly intractable problems. These expanding concerns and the mounting criticisms of substantial numbers of psychologists eventuated in a call for a National Conference on Levels and Patterns of Professional Training in Psychology. Supported by a grant from the National Institute of Mental Health (NIMH), the Conference took place in Vail, Colorado, July 25–30, 1973. The developments since 1965 included the following.

1. Many professional psychologists continued, even after the Chicago Conference, to express strong dissatisfaction with the apparent lack of appropriateness of training provided by many doctoral programs, their low responsivity to social issues, and their uncritical allegiance to the traditional scientist-professional model. Some of this dissatisfaction led to the formation of the National Council on Graduate Education in Psychology and found expression in new training ventures such as the Doctor of Psychology Program at the University of Illinois and the founding of the California School of Professional Psychology.

2. Early in 1969, an Ad hoc Committee on Professional Training was created under the joint aegis of the Education and Training Board and the Board of Professional Affairs of the APA. As the Committee launched into its work under the chairmanship of John Darley, it was soon charged with additional tasks, as newly articulated problems reached the APA governance.

3. Toward the end of 1969, the Black Student Psychological Association presented the APA Council of Representatives with a number of basic demands and a plan for action. This eventuated in a program which included guidelines for the following:

(*a*) The recruitment of black students and black faculty members into psychology;

(*b*) The gathering and dissemination of information concerning the availability of various sources of financial aid for black students;

(*c*) The design of programs offering meaningful community experience for black students in the field of psychology;

(*d*) The development of terminal programs at all degree levels that would equip black students with the tools necessary to function within the black community.

4. In October 1970, the Council of Representatives began deliberations designed to insure that women would be accepted as fully enfranchised members of the profession. A Task Force on the Status of Women in Psychology was appointed for the purpose of addressing itself to three major objectives:

(*a*) The collection of information to document the status of women in psychology;

(*b*) The development of recommendations and guidelines;

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[1] This summary of the deliberations of the Vail Conference was written on behalf of the Conference's Follow-Up Commission which includes the following among its members: D. W. Bray, L. D. Cohen, J. G. Darley, W. A. Hayes, T. Hilliard, E. L. Hoch, M. G. Keiffer, J. G. Kelly, M. Korman, R. B. Kurz, P. M. Lewinsohn, B. G. Little, D. P. Mejia, A. G. Ossorio, W. D. Pierce, K. E. Pottharst, A. O. Ross, G. O. Seymour, W. S. Verplanck, S. Vineberg, and J. L. White. Although the editorial wisdom of a number of the aforementioned is gratefully acknowledged, they share no blame for any inaccuracies or distortions that may be present in the report. A fuller account of the Conference's work and of the issues it confronted will appear in the Conference proceedings.

Requests for reprints should be sent to Maurice Korman, University of Texas Health Science Center at Dallas, Dallas, Texas 75235.

(*c*) The establishment of communications with other agencies and institutions regarding the status of women.

In its 1972 report, this Task Force made extensive proposals concerning curricula and the advising of undergraduate and graduate students; graduate training and the recruitment of women faculty members; discrimination and barriers to women in the federal government, schools, industry, and clinical practice.

5. Concern with the desirability and the implementation of less than doctoral professional training has a long (albeit undistinguished) history in organized psychology. In the last few years, additional developments revived this issue once again:

(*a*) Two regional conferences on the technical and professional preparation of psychologists at the master's level issued final reports strongly endorsing the MA as a professional degree based on broad psychological training and involving a potentially important role as an active agent in the community.

(*b*) The Education and Training Board's Task Force on Master's Level Education likewise issued a strongly worded statement proposing full APA membership status for the non-doctorally-trained psychologist and outlining an explicit program for supporting and regularizing professional master's programs.

(*c*) Although the Council of Representatives endorsed the Task Force's report in 1971, it reversed itself the following year, calling for a straw vote of the membership at large on some of the issues placed before it. A small segment of the APA membership voted; strong reservations were voiced regarding full APA membership for master's-level psychologists.

Such were the major concerns of organized psychology in 1971 when the Ad hoc Committee on Professional Training applied to NIMH for the necessary funds to launch a national conference on training. Shortly thereafter, a more broadly representative steering committee was convened to begin the preliminary work that led to the Conference itself.

The Vail Conference

Selection of participants lies at the heart of any conference. In this regard, Vail represented a clear departure from earlier, similar events. The steering committee sampled from a large number of prospective participants along such traditional dimensions as specialty, role, and area of responsibility; the committee also used criteria that resulted in the selection of 14 trainees, 40 women (including 7 from minority groups), and 29 male minority group members out of 116 invited participants.

Prior to the Conference itself, participants were assigned to 1 of 10 task groups which were provided with a specific charge summarizing the most salient, critical issues. At Vail, participants were also encouraged to form interest groups which might develop their own agendas. All conferees met in frequent plenary sessions to discuss and vote on resolutions presented by these working groups.

The end product of the Conference's five days at Vail was approximately 150 resolutions. Although some of these resolutions addressed different levels of concerns, others were too general or too specific, and still others conveyed a lack of precision with regard to priorities, it is clear that some of the same major themes are reiterated throughout the record of the Conference. These are summarized in the pages that follow.

A Professional Training Model

The Conference explicitly endorsed professional training programs as one type of heuristic model to guide those programs defining themselves by a basic service orientation. It did so *without* abandoning comprehensive psychological science as the substantive and methodological root of any educational or training enterprise in the field of psychology and *without* depreciating the value of scientist or scientist-professional training programs for certain specific objectives. It is important to view the Conference's entire work in the light of a continuing ideological commitment to the tradition of empiricism and as a clear affirmation of the fundamental importance of the scientific endeavor.

Psychology, however, is coming of age and has matured sufficiently to justify the existence of unambiguously professional programs in addition to the more traditional varieties. The choice of training model, as far as a particular program (and prospective student) is concerned, is determined by the kinds of skills its graduates will need in order to function effectively in the particular roles being chosen by and for them.

Professional programs have begun to develop in a variety of new organizational settings: medical schools; departments, schools, and colleges of educa-

tion; free-standing schools of professional psychology; autonomous professional schools in academic settings—in addition to departments of psychology in universities. Although the shaping influence of the administrative and organizational setting on program quality and effectiveness must be recognized, the Conference preferred to examine the criteria that *any* setting must meet in order to conduct high-quality professional training in psychology.

The issue of the highest professional degree label (Doctor of Philosophy versus Doctor of Psychology) was given close examination. Where primary emphasis in training is on the direct delivery of professional services and evaluation and improvement of those services, the PsyD degree is appropriate. Where primary emphasis is on the development of new knowledge in psychology, the PhD degree is appropriate. Although it is desirable to move toward uniformity of degree labels, current local administrative and political constraints must, of course, be taken into account.

Multilevel Training

In the development of professional training programs, priority must be given those that either address multiple levels of training (from the associate of arts, AA, degree to the doctorate) or at least demonstrate clear articulation with degree programs at other levels. Examples would include the development of multiple levels of training within one institutional setting or the coordination of local or regional programs at differing levels. It is clear that much waste of time and effort has resulted from poorly coordinated entrance requirements and overlapping curricula. Since the term *professional* is often narrowly reserved for the products of doctoral training, some conferees argued for a reconceptualization in terms of a *psychological service orientation*. Different services with different objectives and involving different skills can be offered to various target groups by workers at quite different levels, all of them trained in psychological skills. In our society only some of these may typically be called professionals; all of them, however, share a service orientation.

A comprehensive task analysis is urgently needed that would match the competencies for which workers are trained at each step on a service ladder with the roles, functions, and related responsibilities implied in a series of psychological tasks (e.g., advocate, community planner, behavior changer, evaluator, etc.).

The need for "portability" of credit for completed work was stressed, not only as it applies to movement from one level to another, but also for persons desiring to resume their education in a different location at a later point in their lives. It was recommended that heretofore indivisible courses and programs be replaced by smaller sized modules (particularly at subdoctoral levels) that would permit interchangeability and more flexible curriculum building. Credit for certain modules could be obtained on the basis of life or work experience.

The idea of a *career ladder* should be replaced by the more inclusive concept of a *career lattice*—an open-ended occupational structure which encourages broader skill acquisition at any given level in addition to upward professional movement; this would encourage continued training and development, leading to functional differentiation of skills at every performance level.

The Conference participants were concerned lest their endorsement of multilevel training systems be viewed as relegating typically disenfranchised groups such as ethnic minorities and women to "lower level" programs. It is incumbent on the "upper level" programs to set a very high priority on the recruitment of students from such groups; in addition, individuals at the lower levels of professional training should have the opportunity to receive the additional training necessary to upgrade their status.

The careful coordination of educational practices at different points on the training continuum and the encouragement of additional skill acquisition by workers at any given level were considered to be of major importance by the Conference. In fact, it was felt that the accreditation process must give very serious consideration to the degree and quality of appropriate interrelatedness among level-defined programs.

Desirable Characteristics of Professional Training

Because participants labored to a large extent in small groups, many of the resolutions adopted by the Conference as a whole dealt specifically with one or another of the three main levels of training (doctoral, master's, bachelor's and below) or with special group concerns (e.g., minority groups, women, trainees). A review of the work of the

Conference quickly established some pervasive themes, all the more remarkable because they emerged independently from the work of separate groups. Some of these conclusions are discussed below, organized around components that must be considered in a systematic evaluation of a service-oriented training program at any level.

ASSESSMENT OF CONSUMER NEEDS

In their program development (or retrenchment), trainers of psychologists must be concerned with the current job market, as well as with the long-range needs of society. It is important that current information about existing and expanding demands for psychologists in particular applied areas designed to meet societal needs lead promptly to appropriate curricular changes.

APA needs to assume a more aggressive stance in exploring and developing potential career markets for services employing individuals trained in psychology. Obvious examples abound: for example, prison psychology; psychological problems in transportation and in urban life, in population trends, and in meeting the human service needs of several underserved populations.

It was observed that more extensive and frequent interchanges of university training personnel and field psychologists would lead to a sounder perspective on new career options and potential social contributions. Arranging for fieldwork experiences in nontraditional settings will likewise sensitize students to emerging service possibilities. The potential arena of endeavor, it was felt, was nothing less than the total area of human services. Community control and consumer rights are affirmed as key service approaches; hence, consumer help in both assessing community need and in shaping program objectives is critical. Periodic monitoring with reference to the adequacy with which social needs are being met by a particular program is a necessary feature of the evaluation process. The voice of consumers of psychological services must be heard in the process of accrediting such programs.

ADMISSIONS STRATEGY

Disproportionate reliance on traditional selection criteria for professional training (e.g., grades and graduate record examination scores) has proved inadequate in providing society with a pool of socially responsive, culturally diverse, and professionally sensitive psychologists. More attention must be paid to the applicant's socially relevant experiences and goals, his or her interpersonal skills, and a variety of attitudinal and motivational factors. It is important that training programs maximize the degree of cultural diversity characterizing their students. Facilitating the admission, retention, and graduation of students from underrepresented groups will not only correct an obvious injustice, but will simultaneously add multicultural dimensions to the training context, a clearly more efficient way of preparing professionals to function in a pluralistic society.

The Conference endorsed the "truth in packaging" concept with regard to program description as necessary for intelligent decision making on the part of the student and as representing an important ethical stance on the part of the trainers. What is required is not only a clear statement of the orientation of the program and the emphasis it places on various program components and "hurdles," but also detailed information regarding specific requirements, and faculty interests and commitments. A detailed breakdown of past and present student characteristics and job placement is likewise recommended.

Programs must be prepared to accept students on a less than full-time basis; to do otherwise frequently discriminates against the older, more mature student (often the more socially committed and personally motivated trainee). Alternative life-styles and unpopular political beliefs are likewise no bar to professional training: A strong program seeks heterogeneity along many dimensions.

TRAINING STRATEGY

The Conference sought to reexamine the functional relationship between training content and the roles and activities for which it is preparing future professionals, and to do so in the light of existing social needs and ethical responsibilities. Periodic self-study is recommended to assure that nonfunctional requirements are modified or deleted.

A specific commitment to a particular training ideology or design was avoided by the Conference. It was felt that each program should capitalize on the resources available to it, seeking to become the finest possible example of the specific training and service philosophy it espoused. Nonetheless, a few guiding notions were offered:

1. Heavy emphasis was placed on field training in multiple contexts and on a concerted effort at integrating these experiences with the skills and knowledge learned in the classroom. Field experience should be consonant with the objectives of the particular program and with the unique needs and opportunities of the community.

2. The totality of settings in which professional training is provided must be congruent with the needs of a significant range of clients in a community, including persons of culturally diverse backgrounds. Psychologists have an obligation to provide services on a genuinely broad and nondiscriminatory basis. Inadequate preparation for this goal, the conferees felt, represents a shirking of an ethical obligation and does not relieve either trainer or trainee of this obligation.

3. Wherever possible, faculty and students should increase their involvement with underserved populations by providing needed services as a part of training programs. Such efforts merit funding priority from granting agencies.

4. Professional work frequently has social and political ramifications. Much that the psychologist does (or does not do) affects other individuals, his community, and society. It is important that these implications be examined and evaluated dispassionately, and training programs should provide such opportunities.

FACULTY AND TRAINEES

Faculty must provide a convincing professional role model and present a clear commitment to and expertise in the work of the applied psychologist. It is of considerable importance, therefore, that the director and senior trainers themselves be currently engaged in some form of professional work. It is in fact desirable that the training faculty maintain close and meaningful involvement with institutional or community service agencies; concomitantly, field supervisors from applied settings can strengthen the university's training base through appropriate academic appointments and through opportunities for genuine contributions to policy and decision making. The Conference further suggested periodic exchanges of faculty and field supervisors for the purpose of providing continuing professional development for both groups.

Comparable weight must be given to outstanding performance in professional training and service as is earned by distinguished empirical and theoretical endeavors. These values should have the endorsement of administrators responsible for the operation and financial support of the program.

If the training faculty needs appropriate opportunities, encouragement, and rewards, so do the students. Any program that admits a student assumes a particular responsibility to maximize his or her chances of graduating. This calls for the continued availability of basic student resources: financial support, availability of counseling, nondiscriminatory and noncapricious treatment, provisions for redress of grievances. Additional support systems such as adequate access to appropriate role models, relevance of curriculum, and opportunities for skill development must also be provided. It is expected that students will participate in frequent periods of self-study undertaken by the departments as a whole. They should also have a voice in shaping their own curriculum, possibly through specific academic contract.

EVALUATION AND ACCREDITATION

Considering the financial cost and human effort represented by professional education, there has been a curious lack of concern over product and program evaluation. This seems to be true of most of the major professions, but its absence is the more remarkable in a discipline that prides itself on its expertise in evaluation research.

Accreditation as currently practiced is not equal to the task of adequately evaluating the efficacy of training, the quality of graduates, and the value of the services to the ultimate recipient or the nature and significance of its impact on society. Substantially more is needed.

The Conference accepted the suggestion that a demonstration project be undertaken with the ultimate aim of revising present accreditation philosophy and procedures by focusing on the competencies of multilevel graduates and their eventual impact on society.

Samples of current graduates would undergo an intensive evaluation of their professional skills and of factors related to a readiness for ethical and productive psychological practice. Comparable data would be obtained from journeyman field psychologists at various levels of training. Finally, a broad survey would be undertaken of the careers of graduates from a wide sample of institutions in terms of the graduates' career choices and their psychological contribution to society. It is hoped that these data will contribute to a reexamination of the implicit values, beliefs, and assumptions un-

derlying training philosophies, and that this will in turn be reflected in accreditation criteria.

Additional suggestions for a revamping of the accreditation process included: (*a*) reiterated demands for the development and implementation of unambiguous, workable affirmative action goals, as well as timetables for increased participation of minority groups and women in the roles of students and faculty; (*b*) the inclusion of representatives from state psychological associations, student groups, and consumer on-site visiting teams; (*c*) a major restructuring and strengthening of the Education and Training Board, with special consideration being given to the possible creation of an independent accrediting body in order to fully meet these demands.

Doctoral Training

The Conference called (in the professional model) for an extensive and thoroughgoing preparation in the skills necessary for effective practice. At the same time, it concluded:

> Many of the psychological services currently performed by Ph.D., Ed.D. or Psy.D. degree holders could be performed equally well by personnel trained at the master's or lower levels. The real cost of such services to the public could be appreciably lowered by training such personnel to provide them directly.

What then are the appropriate functions of the doctoral psychologist? One might conclude that while "journeyman" skills appropriately belong at the master's level (or even below), "master" professional competence should emerge with doctoral training. The Conference, however, felt that much more should be demanded of the higher level practitioner. The training of these professionals should also prepare them for the following kinds of activities: (*a*) evaluation of service programs and new procedures, (*b*) design of new service delivery systems, (*c*) development of new conceptual models, (*d*) integration of practice and theory, (*e*) program development and administration, (*f*) supervision and training.

A special contribution (or hurdle, depending on one's point of view) of the PhD student is the dissertation. The PsyD degree, on the other hand, permits appropriate substitution for the traditional dissertation. Since many institutions will, however, not be able (or willing) to shift their programs to the PsyD model, some additional suggestions were advanced by the Conference.

Flexible criteria for defining the appropriateness of dissertation proposals are recommended in order to insure that the student's project is relevant to the professional role for which he or she is preparing. Serious consideration should also be given to the inclusion on dissertation committees of psychologists or other competent persons from field agencies and/or other campuses who may be especially qualified for such appointment. Faculty members presently excluded due to existing policies (such as those regarding tenure) should be designated as chairpersons when they represent a logical and appropriate choice.

Master's-Level Training

The position taken by the Conference on the issue of the master's degree reflected a deep concern with the legitimatization of persons trained at the master's level as psychologists and as professionals. It called for the development of strong professional master's programs (of which there are few currently), differentiated by specialization title (e.g., master's in industrial psychology), with explicitly stated objectives and with carefully integrated didactic and field training. It should be noted that this process of legitimatization rests on the restructuring of master's programs into strong, well-rounded professional training sequences.

Departments already engaged in master's training are called on to organize themselves into a Council of Professional Master's Programs and to begin the processes of disseminating curricular information and setting up standards through intensive self-review.

It was the concensus of the Conference that the properly trained master's individual has every right to be called a psychologist and should be admitted to full APA membership. In taking this position, the conferees were not unmindful of other (and potentially contradictory) concerns shared by a substantial number of professional psychologists. With the country moving toward a national health insurance system, our claim for the status of *independent* practitioner is bolstered by a definition of the psychologist as a doctoral-level professional exclusively. The conflict is a difficult one to resolve; convincing arguments can be made by both camps.

One side emphasizes the need for professional autonomy—a goal sought by every profession worthy of the name. Because of a traditional interest in preventive and developmental approaches and in

experimentation with a broad panoply of service strategies, psychology seems capable of a very unique contribution to the total health effort. Thus society, not only psychology, shall be the loser if a narrowly subservient role is assigned to it.

The other side sees dangers ahead: (*a*) a continuation of the inequities engendered by a fee-for-service, middle-class-oriented health system; (*b*) a disregard for cost control, accessibility, and availability of services, particularly where underserved target populations are concerned; and (*c*) a perpetuation, well into the third decade, of a serious injustice carried out at the expense of the master's-level psychologist.

The Conference could not, in good conscience, turn its back on this problem once more; it called for the inclusion of master's psychologists among its professional ranks. It did, however, reject a recommendation for their licensing or certification; such statutory implementation was left to future developments. This compromise, many participants felt, would maximize psychology's chances of becoming a clearly recognized independent profession while initiating a series of extensive, positive alterations internal to the profession with regard to master's-level professional personnel.

Training at Bachelor's Level and Below

It has become increasingly clear that many persons with less than master's-level training render a psychological service, frequently of professional caliber. Since 1966, for example, some 150 AA degree mental health programs have sprung up, typically training generalists who render a large number of services through their knowledge of specific techniques and skill in forming and maintaining human relationships. Little work has been done on carefully examining and analyzing the parameters of the submaster's-training competencies and parallel service functions. In addition, the extent of professional psychology's responsibility toward such programs and their trainees is a generally ignored issue.

The Conference recognized two types of submaster's programs, those that primarily emphasize academic psychology and those that include specific training in applied skills. With regard to the latter type of program, the following points were made:

1. These efforts include AA and bachelor's degree programs, as well as nondegree continuing education and skill training for various groups. Although focused primarily on mental health, other specialties (particularly in new human services areas) represent likely possibilities for program expansion. A number of these programs produce workers with a distinctly *psychological* service orientation.

2. The Conference recognized the lack of clear information concerning manpower needs, training settings, and outcome evaluation in this area. Nonetheless, it made a number of preliminary suggestions in this context: (*a*) The selection process must include the evaluation of interpersonal and related skills; (*b*) sound field training must be a central part of all such programs; (*c*) some mechanism must be developed; (*d*) consideration must be given to affording both horizontal and lateral mobility to such trainees.

3. Are such persons psychologists? The Conference did not have a definitive answer to this question. At the very least, it was felt that the psychological establishment has some responsibility for establishing a vehicle that might provide a means for affiliation under the overall APA umbrella. It seems difficult to think of a responsible answer to many current social/professional issues that ignore this group altogether.

Continuing Professional Development

A recognition of the need for relevant continuing professional development (CPD) experiences for all professional psychologists constituted a major theme of the Conference. Not only must CPD experiences permit psychologists to update their skills in the context of a knowledge explosion and the emergence of new professional roles, but they must also facilitate career changes for psychologists wishing to prepare themselves for new functions.

A number of specific recommendations emerged, designed to facilitate the prompt implementation of CPD programs:

1. Institutions already involved in professional training are encouraged to establish or cooperate in CPD programs. Since these programs should encompass broader resources than are typically available in single instructional units, they should look to multidisciplinary content and participation.

2. Training formats must be eminently flexible because such programs often deal with part-time students. Evening sessions, one- or two-day workshops, short-term apprenticeships, cable TV, and other innovative approaches should be considered.

3. Educational institutions need to devise sys-

tems for giving "credit" for CPD participation, perhaps via "CPD units" reflecting a block of hours of participation. The convertibility of CPD units into regular academic credit would open the door to eventual completion of degree requirements for part-time students.

4. One can conceive of a series of distinct CPD offerings ranging from one-time presentations (suitable for professional psychologists wishing to stay informed about new developments) to planned sequences of educational experiences (geared to psychologists preparing themselves for new professional roles). In either case, program effectiveness must be determined by built-in evaluative mechanisms.

Professional Training and Minority Groups

The Conference's concern with the implementation of affirmative action programs—viewed as a basic ethical obligation—for the identification, recruitment, admission, and graduation of minority group students has been discussed above. In addition, the participants felt that it was important for *all* students to be prepared to function professionally in a pluralistic society. To this end, it was suggested that (*a*) training experience should occur in a multicultural context both within the university and in fieldwork settings; (*b*) the content of training must adequately prepare students for their eventual professional roles vis-à-vis a wide diversity of target groups; (*c*) students must be helped to maintain a balance between acculturation into a professional and scholarly role, on the one hand, and retention of their group identity and cultural sensitivity, on the other.

Because of dissatisfaction with the scope of APA's response to the concerns of ethnic minorities, the Conference accepted a resolution which recommended: (*a*) that a Board of Minority Advocacy, composed of representatives of ethnic minority groups, be created in APA and be responsible for examining policies touching on minority concerns; (*b*) that this Board have a prior review function regarding other organizational units of APA and that it advise the Board of Directors in the general area of minority affairs; (*c*) that an Office of Minority Affairs be created, with responsibility for monitoring and evaluating APA projects, programs, and policies, for developing and disseminating information related to minority concerns, and for initiating appropriate relationships with public and private agencies.

To the extent that federal funding continues to be a major variable in initiating training and service projects, the recommendation was made that appointments to grant review panels should parallel, through significant minority membership, the composition of target populations.

Professional Training and Women

Large numbers of well-prepared women apply for admission to graduate programs. They should be admitted at least in proportion to their numbers among the institution's applicant population. Obstacles to graduation unrelated to student performance (frequently related to family status and geographical moves) must be eliminated.

The Conference deplored the application of sex role stereotyping in the counseling and advisement tracking of undergraduate women. It was also noted that pressures on employers are creating opportunities for women in fields that heretofore were not readily open to them.

The Conference endorsed the notion that extensive empirical research is needed to examine many prevalent biases in such areas as sex role stereotyping, the definition of psychological adequacy in women, the effects of child care roles on women and men, and concepts of masculinity and femininity.

Schools and the media were singled out as institutional settings that often reflect harmful sex role stereotyping; psychologists must help modify such practices wherever they exist.

Finally, the concept of *personism* was offered as a suitable alternative to sexism. This is a term designed to reflect the awareness and acceptance of the individuality of all persons, male and female; both employers and trainers have an ethical obligation to exemplify this value in their professional behavior.

Service Delivery Systems and the Social Context

Professional training programs must develop strong linkages with service delivery systems in the community and maintain a continuing dialogue which includes the *recipients* of the services as well. It is anticipated that the introduction of the client's perspective into what then becomes a collaborative effort will significantly modify values and beliefs held by professional trainees.

Field settings present the student with a laboratory opportunity to study the interplay between

social systems and life events, as well as the parameters of system change. No other experience can drive home so forcefully the extent to which psychological distress and social dysfunction are intertwined. Ideally, a number of these community settings will be characterized by the availability of a full range of human services in addition to innovative mental health technology: job and legal counseling, welfare, vocational and physical rehabilitation, and a variety of child and family services.

The Conference alluded frequently to the fundamental importance of a problem-oriented service commitment, perhaps because society's pressing concerns with its psychological casualties demand no less of professional psychologists. At the same time, there was a definite undercurrent in what seemed to some the opposite direction: a motivation to build programs that foster and maintain human competencies and prevent dysfunction or disruption. Psychology, the conferees felt, must be fully responsive to both sets of demands, although individual psychologists may have a more focused commitment. Training opportunities should therefore encompass the full range of remediation, prevention, and development.

It is desirable that students have the opportunity to learn advocacy roles with regard to the recipients of psychological services. It is particularly those underserved populations—the poor, the aged, the prisoner, the alcoholic, and the retarded—who are the appropriate target populations for *both* service and training efforts. New service modalities designed to expand the scope of services while reducing their unit cost are badly needed. Fledgling psychologists must be sensitized to the need for the multiplicative transfer of human skills through layers of professionals, subprofessionals, and other helpers.

Evaluation of the impact of service delivery systems must take place in an interdisciplinary context, with objectives so clearly specified that their attainment or nonattainment can be adequately assessed. Finally, because of the growing disparity in quality of service between various sectors of our society, the Conference recommended that APA, together with state associations, explore the legal procedures necessary to bring about in public agencies and institutions a uniform standard of psychological service equal to that now existing in the private sector.

Conclusion

The NIMH grant which funded the Vail Conference had the unique provision of a two-year follow-up plan to disseminate as widely as possible the work of the Conference and to help initiate the process of implementation of the proposals that emerged from Vail. To that end, a Follow-Up Commission, composed of the original 10-member Steering Committee and 10 additional members chosen by the Conference participants from among themselves began to function in the waning minutes of the meeting at Vail.

The Commission continued its work at a meeting in Washington, D.C., in October 1973, where it laid plans to review and reorder the Vail resolutions along the dimensions of implementability and priority. It also decided to initiate a dialogue with all the relevant elements of the APA governance and affiliated organizations regarding the issues at hand.

The resolutions themselves were conceived of as a significant starting point in this dialogue, a deliberative explication of a certain point of view. Following consensus seeking, discussion, and feedback, it is hoped that forward-looking action plans will be developed that will quicken the emergence of American psychology into a new era of professional competence and responsivity.

Composition of the Council of Representatives

Report on Apportionment Ballot

COMMITTEE ON STRUCTURE AND FUNCTION OF COUNCIL [1]

The third annual apportionment ballot held under the revised APA Bylaws dealing with the composition of Council for 1975 showed some unexpected changes from previous years. Divisions lost a total of five seats, while coalitions and state associations gained five seats. Looking behind these figures one finds that divisions fared even less well than it appears. Two new divisions appeared on the apportionment ballot for the first time. These were Division 34 (Population Psychology) and Division 35 (Psychology of Women). Division 34 failed to gain representation, while Division 35 acquired three seats on Council. Division 16 (School Psychology) was the only division improving by gaining one seat, but eight divisions each lost one seat including Division 10 (Psychology and the Arts), which lost the one seat it held. This means that, for the first time, there are now two divisions of APA (10 and 34) that do not have representation on Council. The change of five seats by coalitions and state associations is confined to just five units, two losing and three gaining. The two losing were the coalition of Utah/Wyoming/Montana/New Mexico and the Connecticut State Association, both losing their one seat on Council. The three that gained were Florida regaining the seat it lost last year, New York gaining two seats, and California gaining four seats.

Table 1 shows the ratio of Council representatives from divisions and states for the period 1966 to 1975. Under the new method of determining the composition of Council, the states appear to be faring slightly better than in the past. The gain of 4.8% by the states from 1974 to 1975 may be the result of greater efforts by the states in the last apportionment ballot, or it may be a shift representing a change in the interests of the voters. The next apportionment ballot will indicate if this is a continuing trend.

In examining the distribution of votes as shown in Table 2, it is noted that California succeeded in attracting the most 10-point allocations. Apparently, efforts at convincing its members to allocate all 10 votes to California were more successful than others. Table 2 shows the obvious, that those units getting the most 10-point allocations are the ones that get the most seats on Council. This apparently says something for getting out the vote and loyalty to the division or state of your choice.

The results of the apportionment ballot are shown in Table 2. The number of members voting was 12,102, or 37.9% of the voting membership. This compares with last year's figures of 10,994 voting, or 35.2% of the voting members. These figures are normal for APA, since the percentage voting in any ballot to the membership averages between 35% and 40%.

The apportionment ballot provides for 104 Council members in 1975. This does not include members of the Board of Directors, who are members of Council in accordance with the Bylaws. Adding the 12 members of the Board brings the total on Council to 116. This total could increase if some of the units decide to form coalitions to gain representation.

Table 2 shows the changes from 1974 to 1975. Those divisions, coalitions, or states that lost seats on Council may have to arrange to recall one of their representatives in 1975 to get down to their allotted number.

TABLE 1

Division versus State Representatives, 1966–1975

Year	No. representatives from divisions	% of total	No. representatives from state associations	% of total	Total council representatives[a]
1966	82	73.2	30	26.8	112
1967	85	73.9	30	26.1	115
1968	94	75.2	31	24.8	125
1969	92	75.4	30	24.6	122
1970	99	73.9	35	26.1	134
1971	96	75.0	32	25.0	128
1972	98	73.7	35	26.3	133
1973	76	69.7	33	30.3	109
1974	73	70.2	31	29.8	104
1975	68	65.4	36	34.6	104

[a] Excludes Board of Directors.

[1] This report was prepared by Marcia Guttentag, Chair. Other committee members are Mary Ellen Curtin, Frank W. Finger, Nelson F. Jones, Ellis B. Page, and Lyman W. Porter.

TABLE 2

APA Ballots for Council Representation for 1975: Voting Results

Division	Vote allocations										Total value: points = votes	% total	Council seats		
	1-point votes	2-point votes	3-point votes	4-point votes	5-point votes	6-point votes	7-point votes	8-point votes	9-point votes	10-point votes			1974 allocation	1975 allocation	Change 1974–1975 (+ or −)
1	275	137	44	25	35	2	—	4	1	21	1,219	1.00	1	1	
2	392	237	98	44	92	8	4	8	2	76	2,714	2.24	2	2	
3	246	185	129	64	171	23	35	18	10	254	5,271	4.35	5	4	−1
5	263	128	89	48	114	12	10	11	2	62	2,416	1.99	2	2	
6	152	86	70	34	87	18	10	10	4	83	2,229	1.84	2	2	
7	335	194	112	57	102	15	20	10	1	75	2,866	2.36	3	2	−1
8	394	268	134	68	127	31	17	23	4	85	3,614	2.98	4	3	−1
9	305	276	146	78	123	19	7	13	4	82	3,345	2.76	4	3	−1
10	110	53	14	6	17	1	2	2	—	17	573	.47	1	—	−1
12	532	606	354	222	444	43	35	43	6	141	8,225	6.79	8	7	−1
13	178	109	32	8	26	4	2	1	—	13	830	.68	1	1	
14	121	100	75	65	118	29	26	30	9	225	4,323	3.57	4	4	
15	271	214	101	77	153	20	13	8	4	124	3,626	2.99	3	3	
16	208	161	124	73	223	45	26	48	9	349	6,716	5.55	5	6	+1
17	297	237	141	64	225	29	26	24	4	153	4,689	3.87	4	4	
18	129	61	30	15	32	2	1	1	—	17	758	.62	1	1	
19	43	30	21	16	37	3	2	5	1	86	1,356	1.12	1	1	
20	160	56	24	7	30	4	4	3	1	63	1,237	1.02	1	1	
21	36	28	24	16	49	6	7	3	1	45	1,041	.86	1	1	
22	128	89	38	16	43	7	1	4	—	48	1,260	1.04	1	1	
23	74	27	15	4	15	1	4	1	—	51	816	.67	1	1	
24	100	58	34	18	46	2	3	3	1	35	1,036	.85	1	1	
25	149	93	72	41	81	14	10	12	4	115	2,556	2.11	3	2	−1
26	98	51	29	18	29	5	—	6	—	40	982	.81	1	1	
27	206	150	71	30	62	10	4	5	1	36	1,646	1.36	2	1	−1

See footnotes at end of table, p. 453.

TABLE 2—(*Continued*)

APA Ballots for Council Representation for 1975: Voting Results

Division or state	Vote allocations										Total value points – votes	% Total	Council seats		
	1-point votes	2-point votes	3-point votes	4-point votes	5-point votes	6-point votes	7-point votes	8-point votes	9-point votes	10-point votes			1974 allocation	1973 allocation	Change 1974–1973 (+ or –)
28	109	71	41	14	32	5	2	4	1	27	945	78	1	1	
29	400	349	233	120	223	26	25	34	3	105	5,072	4 19	5	4	–1
30	77	42	23	10	20	1	3	1	—	21	615	50	1	1	
31	264	227	102	28	69	7	6	3	—	18	1,769	1 46	1	1	
32	280	188	92	48	95	12	7	14	2	66	2,510	2 07	2	2	
33	138	84	35	18	42	2	2	1	—	22	947	78	1	1	
34	63	37	15	7	12	—	—	1	—	6	338	27	—	—	
35	279	199	85	44	115	13	9	10	—	113	3,034	2 50	—	3	–3
Alabama/Arkansas/Louisiana/Mississippi*	29	22	13	18	34	3	3	4	1	31	744	61	1	1	
Alaska/Idaho/Washington*	28	24	18	9	24	25	9	7	3	20	782	64	1	1	
Arizona/Oklahoma*	14	16	4	10	40	6	15	3	1	59	1,062	.87	1	1	
Georgia/North Carolina/South Carolina*	40	45	30	20	52	9	8	6	1	46	1,187	98	1	1	
Kentucky/Tennessee*	27	32	15	15	26	7	3	3	—	34	753	.62	1	1	
Maine/New Hampshire/Rhode Island/Vermont*	26	17	10	10	28	2	2	6	1	34	693	57	1	1	
Utah/Wyoming/Montana/New Mexico*	19	18	7	12	24	1	3	3	—	26	555	45	1	—	–1
Virginia/West Virginia*	32	18	11	11	27	2	4	3	—	53	874	72	1	1	
California	99	100	70	56	188	39	42	83	21	557	8,624	7 12	3	7	+4
Colorado	18	11	12	9	25	10	8	10	4	46	929	76	1	1	
Connecticut	18	18	12	7	18	2	2	3	—	32	578	47	1	—	–1
Delaware	3	3	—	1	7	—	—	—	—	5	98	08	—	—	
District of Columbia	38	38	22	18	48	4	4	1	—	33	882	.72	1	1	

*See footnotes at end of table, p. 453.

TABLE 2—(*Continued*)

APA Ballots for Council Representation for 1975: Voting Results

State	Vote allocations										Total value points = votes	% Total	Council seats		
	1-point votes	2-point votes	3-point votes	4-point votes	5-point votes	6-point votes	7-point votes	8-point votes	9-point votes	10-point votes			1974 allocation	1975 allocation	Change 1974–1975 (+ or −)
Florida	24	25	16	13	40	8	5	13	2	74	1,319	1.09	—	1	+1
Hawaii	13	4	—	1	2	2		—	—	35	397	.32	—	—	
Illinois	60	65	27	21	42	1	2	4	1	30	926	.76	1	1	
Indiana	20	26	21	16	26	6	6	12	2	46	981	.81	1	1	
Iowa	13	7	8	2	23	4	3	8	1	34	632	.52	1	1	
Kansas	15	13	6	5	12	2	3	6	3	37	617	.51	1	1	
Maryland	47	44	21	16	44	5	2	2	1	44	991	.81	1	1	
Massachusetts	39	41	25	21	77	13	6	7	—	47	1,311	1.08	1	1	
Michigan	52	41	27	16	46	5	7	5	—	77	1,398	1.15	1	1	
Minnesota	21	24	9	13	32	6	2	3	2	43	830	.68	1	1	
Missouri	12	17	12	19	25	8	5	9	—	44	878	.72	1	1	
Nebraska	6	4	2	5	7	1	—	1	—	27	359	.29	—		
Nevada	1	4		1	1	1	—	—	—	—	24	.01	—	—	
New Jersey	38	53	22	21	63	16	5	14	—	79	1,642	1.35	1	1	
New York	109	159	107	75	190	46	33	30	2	189	4,653	3.84	2	4	+2
North Dakota	4	2	1	1	3	—	—	—	—	5	80	.06	—	—	
Ohio	63	57	31	23	46	5	2	1	1	17	823	.68	1	1	
Ontario	10	7	5	4	9	1	1	—	—	8	193	.15	—		
Oregon	8	8	4	6	27	5	4	3	—	46	737	.60	1	1	
Pennsylvania	75	86	32	18	65	5	—	1	1	27	1,057	.87	1	1	
Puerto Rico	1	3	1	—	1	—	—	—	—	2	35	.02	—	—	
South Dakota	5	1		—	2	—	—	—	—	4	57	.04	—	—	
Texas	48	43	18	15	49	12	2	6	—	80	1,427	1.17	1	1	
Wisconsin	18	20	1	6	60	12	6	4	3	70	1,258	1.04	1	1	
Totals	7,905	5,947	3,267	1,917	4,522	693	520	625	126	4,715	120,960	99.63	104	104	

Note. Total returns = 12,[illegible] ballots = 7, ballots counted = 12,167 (12.[illegible]% of voting membership).
* Coalitions.

American Psychological Foundation 1973 Contributors

The American Psychological Foundation was established in 1953 to receive gifts from psychologists who wish to contribute to the advancement of psychology as a science and a profession in the service of knowledge and the welfare of man. Through its awards program the Foundation honors annually a senior psychologist who in his whole career has made distinguished contributions to research and scholarship (the Gold Medal Award), and it also recognizes outstanding achievements in teaching and in interpreting psychology in public media (books, newspapers, television.) Through its small grants program it supports activities of importance to psychology that would otherwise go unfunded. For example, it provides APA journals to libraries in developing nations. In addition, in appropriate cases the Foundation contributes funds to support programs of other organizations which are consistent with the purposes of the Foundation.

In accordance with the Bylaws, the Foundation is placed in the hands of a Board of Trustees, consisting of the seven most recent past presidents of the APA and other members of the Association elected by the Trustees then in office.

The Foundation's program depends on the concern and generosity of psychologists. Some contribute in the form of specific cash contributions. Others assign all or part of their royalties from books, particularly books that draw heavily on the work of other psychologists. Others routinely assign income from reprinting of their papers in books of readings or other collections. Some have designated the Foundation a beneficiary in their wills. Contributions are tax deductible.

Below are listed the names of all whose contributions to the APF were received in 1973:

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Eighty-Second Annual Convention of the American Psychological Association

Convention Information

THEODORE G. DRISCOLL, JR. *Convention Manager*
CARL N. ZIMET *Chair, Board of Convention Affairs*

This is New Orleans! Air conditioning . . . Al Hirt . . . Andrew Jackson . . . antebellum plantations . . . antiques . . . Antoine's . . . Arnaud's . . . Audubon Park . . . bananas Foster . . . Basin Street . . . Battle of New Orleans . . . bayous . . . Bourbon Street . . . breakfast at Brennan's . . . Café du Monde . . . café au lait and beignets . . . Canal Street . . . cemeteries . . . chicory coffee . . . Commander's Palace . . . Court of the Two Sisters . . . courtyards . . . Creole cuisine . . . dixieland . . . female impersonators . . . five-cent telephone calls . . . French Market . . . French Quarter or "Vieux Carre" . . . funeral marches . . . Galatoire's . . . Garden District . . . gumbo . . . hot and humid . . . Jackson Square . . . jazz . . . Jean Lafitte . . . Jim Garrison . . . lace balconies . . . "ladies" of the night . . . Lake Pontchartrain . . . levees . . . Longue Vue Gardens . . . the Longs of Louisiana . . . Mardi Gras . . . mausoleums . . . Old Absinthe House . . . oysters Rockefeller . . . Pat O'Brien's . . . pecan pralines . . . Pete Fountain . . . prawns . . . Preservation Hall . . . Ramos gin fizz . . . river boats . . . St. Charles streetcar . . . streetcar named Desire . . . Sugar Bowl . . . Superdome, finished or unfinished . . . topless dancers . . . voo-doo.

And that is where the 1974 Convention of the American Psychological Association will be held. The dates of the Convention are **August 30 through September 3.** While August is not the ideal time to be in New Orleans, the attractions of the city should compensate for the heat and humidity. Meetings are scheduled in the **Fairmont Hotel**, the **International Hotel**, the **Marriott Hotel**, the **Monteleone Hotel**, the **Royal Orleans Hotel**, and the **Rivergate Exhibition Center.** The bulk of the Convention program is scheduled between 9:00 a.m. and 6:00 p.m. during the five days of the meeting. Because hotels are fairly close to one another, APA is not planning to run a shuttle bus among the hotels. Public transportation buses run up and down Canal Street every few minutes at a cost of 15 cents per person.

Canal Street, New Orleans' main thoroughfare, separates the French Quarter from the business district and uptown area of the city. There are historical sights, shops, nightclubs, and restaurants within the French Quarter. In order to sample the cuisine in local restaurants it would be wise to make reservations for all meals. A handy reference on inexpensive and good restaurants is *The New Orleans Underground Gourmet* by Richard H. Collin.

Transportation from the New Orleans International Airport to the downtown area is available by taxicab or limousine. It takes about 30 minutes to make the trip. The taxicab fare is approximately $9 plus tip for one to three persons; there is an additional charge of $3 per person for more than three. Limousine service is available directly to the hotels at $3 per person. Public transportation is also available from the airport for a fare of 35 cents, but it may be necessary to transfer to another bus within the city in order to get directly to your hotel. See you in New Orleans!

Housing

In cooperation with APA, all of the hotels have set aside, at guaranteed rates, substantial blocks of rooms. These hotels have guaranteed rates for the Convention only when registration is made through the APA Housing Bureau on the official Hotel Reservation Form. Also, these rates are guaranteed only when the Advance Registration Form and Hotel Reservation Form are returned *prior to August 1, 1974.* After August 1, every effort will be made to assign rooms at the guaranteed rate, but such assignments cannot be certain, and it is quite unlikely that late requests can be honored.

Members should also bear in mind that with the large number of rooms used, some members may not be assigned a room in the specific hotel they requested nor the type of room at the rate requested. It is advisable to return the Advance Registration Form and the Hotel Reservation Form as early as possible. These forms appear on pages 459–460.

All housing forms are sent from the Central Office to the New Orleans Convention Bureau for room assignments. We anticipate a three- to four-week lag between receipt of request in the APA office and your receiving confirmation from the hotel. If after four weeks you have not heard from New Orleans, please contact *di-*


New Orleans
Business District
and French Quarter
Downtown Hotels
© 1970 Greater N.O. Tourist
and Convention Commission
FRENCH QUARTER
MISSISSIPPI RIVER
FRENCH MARKET
JACKSON SQUARE
BEAUREGARD SQUARE
MUNICIPAL AUDITORIUM
INTERSTATE 10
CANAL ST.
TULANE AVE.
GRAVIER
UNION
COMMON
POYDRAS
LAFAYETTE
GIROD
NOTRE DAME
HOWARD AVE.
POST OFFICE
UNION STATION
PONTCHARTRAIN EXPRESSWAY
TO INTERSTATE 10
TO MISSISSIPPI RIVER BRIDGE
INTERNATIONAL TRADE MART
RIVERGATE
CIVIC CENTER
EARHART BLVD.
MELPOMENE
TERPSICHORE
ST. CHARLES AVE.
ST. JOSEPH
ST. PETER
ST. ANN
ORLEANS
TOULOUSE
CONTI
BIENVILLE
BERVILLE
BARRACKS
ESPLANADE
DAUPHINE
BOURBON
BURGUNDY
DUMAINE
ST. PHILIP
ROYAL
DECATUR
N. RAMPART
ST. CLAUDE
TREME
CROZAT
BASIN
N. CLAIBORNE AVE.
N. ROBERTSON
N. VILLERE
MARAIS
S. ROBERTSON
S. VILLERE
CLEVELAND
S. LIBERTY
SARATOGA
ELK PL
UNIVERSITY
CARONDELET
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S. SARATOGA
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O'KEEFE AVE.
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CAMP
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WELLS
CROSSMAN
BD. TRADE
PICAYUNE
NATCHEZ
CONVENTION
CONCERT
CONVERSE
LAFAYETTE
FR. MKT. PL.
LA. PETERS

rectly the New Orleans Convention Bureau, 334 Royal Street, New Orleans, Louisiana 70130.

Wheelchair Accessibility

To facilitate making hotel reservations for those people concerned with wheelchair accessibility, Division 22, the Division of Rehabilitation Psychology, compiled the following information on the accessibility of some of the Convention hotels. Wherever possible, the information has been corroborated by site visits made by knowledgeable persons. Detailed information about the accessibility of various churches, restaurants, museums, and stores may be obtained by writing for a free copy of *Guide to New Orleans for the Handicapped* from Louisiana Chapter, National Society for Crippled Children and Adults, Inc., 843 Carondelet Street, New Orleans, Louisiana 70130.

Rivergate Exhibition Center
Main entrance: Accessible
Restrooms: Accessible
No sleeping rooms

International Hotel
Main entrance and elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Two meet accessibility requirements and *must be requested.*

Monteleone Hotel
Garage entrance and three elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Hotel has 56 accessible sleeping rooms which *must be specifically requested.* They are rooms on the fifth floor in the following series: 53, 56, 57, 58, 63, and 69.

Fairmont Hotel
One of the main entrances to the hotel is ramped.
Public restrooms: Women's—accessible
Men's—inaccessible
Sleeping rooms: Inaccessible

Royal Orleans Hotel
Main entrance: Assistance available to negotiate the steep ramped entrance from garage and staired main entrance.
Elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Inaccessible

Marriott Hotel
Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Royal Sonesta Hotel
Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Persons requesting wheelchair-accessible accommodations should check the appropriate box on the Hotel Reservation Form as well as indicate the hotel and type of accommodation requested.

Registration

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting, but advance registration represents a saving to the registrants as well as the opportunity to obtain desired hotel space and rates. At its meeting on January 18–20, 1974, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For those registering in advance (prior to August 1, 1974):

$30—Nonmember
$20—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For those registering on-site in New Orleans:

$35—Nonmember
$25—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Key to Map of New Orleans

1. Fairmont Hotel
2. International Hotel
3. Marriott Motor Hotel
4. Monteleone Hotel
5. Royal Orleans Hotel
6. Bienville House
7. Bourbon Orleans Ramada
8. Braniff Place
9. Chateau Motor Hotel
10. Dauphine Orleans Motor Hotel
11. de la Poste Motor Hotel
12. Delta Towers Hotel
13. Downtowner du Vieux Carre
14. Downtown Howard Johnson's Motor Lodge
15. Governor House
16. La Salle Hotel
17. Le Pavillon
18. Le Richelieu Motor Hotel
19. Place d'Armes Motor Hotel
20. Prince Conti Motor Hotel
21. Provincial Motor Hotel
22. Royal Sonesta Hotel
23. Saint Louis Hotel
24. Tamanaca (Best Western) Downtown Motel
25. Thunderbird (Quality Inn)
26. Vieux Carre Motor Lodge

Students and other authorized exemptions from these fees appear on the Advance Registration Form.

The convention badge with name and institutional affiliation, will be mailed in advance of the Convention to those who preregister. Advance registrants will need only to obtain a badge holder at the APA registration area in New Orleans to complete the procedure—thus avoiding possible delays.

Complete member and nonmember registration facilities will be maintained at three locations: the Fairmont Hotel, the Marriott Hotel, and the Rivergate Exhibition Center—according to the following schedule:

Thursday, August 29, 1:00 p.m. to 9:00 p.m.
Friday, August 30, 8:30 a.m. to 5:30 p.m.
Saturday, August 31, 8:30 a.m. to 5:30 p.m.

From Sunday, September 1 through Tuesday, September 3, registration facilities will be available only at the Rivergate Exhibition Center. Hours will be 8:30 a.m. to 5:30 p.m. on Sunday and Monday; 8:30 a.m. to 12 noon on Tuesday.

Placement

The APA Convention Placement Office will be located in the Rivergate-South Hall. Hours will be from 9:00 a.m. to 5:00 p.m., August 30–September 3, except 1:00 p.m. closing day. Fees required are as follows:

Applicants—$2 for reproducing (1) availability information. $3 for use of Convention Placement facilities includes a copy of the *Position Openings Bulletin*. (Nonmembers will be charged $20 for use of Convention Placement facilities.) July 19, 1974, is the deadline for applicant listing. Appropriate fee must accompany listing.

Employers—$5 for reproducing (1) Job Description. $10 for use of Convention Placement facilities includes a copy of the *Availability Notices Placement Bulletin*. July 26, 1974, is the deadline for employer listing. Appropriate fee must accompany listing.

Employers: Please note the addition of the GENERAL OPENINGS category to be used by organizations having widespread needs for psychologists who may be recruited through a central office.

Prelisting in the more widely distributed printed booklet is encouraged, but Xerox forms will be available for on-site listing. The forms, accompanied by the appropriate listing fee, must be received by the date indicated above. Please do not include the Convention Placement facilities fee which will be collected ONLY when you register at the Placement Office. After the above date, Xerox forms will be available for listing, and requests should be directed to the address below.

A complete set of applicant availabilities will be available to employers: $15 for each category—please indicate choice. After the Convention, order from the Convention Placement Office, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Mail orders for the printed *Availability Notices Placement Bulletin* and the *Position Openings Bulletin* are $1.50 per copy (available August 7). Your check (no billing) may be sent to the above address.

Convention Personnel

You may wish to contact one or more of these people before or during the Convention:

Board of Convention Affairs: Carl N. Zimet, Division of Psychology, University of Colorado School of Medicine, Denver, Colorado 80220.

Convention Manager: Theodore G. Driscoll, Jr., Driscoll and Associates, 7109 Masters Drive, Potomac, Maryland 20854.

Assistant for Convention Management: Candy Won, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Convention Placement Office: Jim Beauter, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Please Note

Meetings for Divisions 19 and 21 will be located in the *Monteleone Hotel* rather than the Royal Orleans Hotel as noted in the April convention insert.

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR REGISTRATION ONLY. THE REVERSE SIDE IS FOR HOTEL RESERVATIONS.

General Convention Information **Deadline August 1, 1974**

1. Please indicate those APA divisions to which you belong (Check one or more for list of divisions) **None**

Check your most senior status regardless of whether you belong to a division

... **Fellow** ... **Member** ... **Associate**

2. Please indicate the number of persons who are accompanying you to this Convention but who are not completing a registration form and are not accounted for on another registration form ... **Adults** ... **Children**

How many of them are planning to attend any meetings? ...

3. Please provide the address to which the convention badge is to be mailed. Be sure to fill in this space even if it is the same as the information given elsewhere on this form.

Street Address: ...

City, State, Zip Code: ...

Registration Fee Status **(check one):**

$30: ☐ **Nonmember**

$20: ☐ **APA Member Fellow Associate** ☐ **Foreign Affiliate** ☐ **High School Teacher Affiliate**

Exempt ☐ **APA Dues Exempt Member** ☐ **Student** ☐ **Nonmember Invited Participant** ☐ **Nonmember Spouse, Dependent of Registrant**

Badge Information **(Please Print):**

Institution/Company Affiliation: ...

City and State: ...

PLEASE PRINT CLEARLY - NAME BELOW FOR VISIBLE LOCATION

Title	Last Name	First Name	Initial

This card and these fees are only applicable for advance registration prior to August 1, 1974. Members who have advance registration cards received in the APA C[illegible] will have to do so on-site in New Orleans. If possible [illegible] rather than photocopied forms. Use another card for additional persons who wish to register. Additional cards can be obtained in any issue of the *American Psychologist* from April through July.

PLEASE COMPLETE BOTH SIDES OF THIS FORM.

Convention attendees are asked to register for the meetings at Atlanta. Not only will advance registration assist APA in planning for the meeting, but advance registration represents a saving to the registrant. At its January 18-20, 1974 meeting, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For Those Registering in Advance Prior to August 1, 1974:

$30 Nonmember $20 APA Member Fellow Associate Foreign Affiliate High School Teacher Affiliate

For Those Registering On-Site in New Orleans:

$35 Nonmember $25 APA Member Fellow Associate Foreign Affiliate High School Teacher Affiliate

Send completed form and check for registration fee to:

American Psychological Association
1200 17th Street, N.W., Room 601
Washington, D. C. 20036

Make checks payable to APA.

List of divisions for Question 1 on registration form:

1 [illegible] Testing 3 Experimental 5 Evaluation and Measurement 6 Physiological and Comparative 7 Developmental 8 [illegible] 9 [illegible] and Personality 10 Psychology and the Arts 12 [illegible] 16 [illegible] 17 [illegible] 18 Public Service 19 Military 20 Adult Development and Aging 21 [illegible] 22 Rehabilitation 23 [illegible] 24 Philosophical 25 Experimental Analysis of Behavior 27 [illegible] 28 [illegible] 29 Psychotherapy 30 Hypnosis 31 State Psychological Association Affairs 32 [illegible] 33 Mental Retardation 34 - Population, 35 - Psychology of Women, [illegible]

Please fill out this form in COMPLETE detail, particularly ARRIVAL and DEPARTURE dates, and type of ACCOMMODATIONS.

SEND APPLICATION AS SOON AS POSSIBLE to address shown at bottom of page to insure desired accommodations at convention rates. DEADLINE: August 1, 1974.

☐ Check here if NO accommodations are requested.

Choice of Hotel

First: ....

Second: ....

Third: ....

Arrival ..... at ..... a.m. ..... p.m.

Departure ..... at ..... a.m. ..... p.m.

Type of Accommodation Requested

Note: Single rooms are at a premium. Please share a twin bedroom wherever possible.

..... Single Room @ $.....

..... Double Bedroom @ $.....

..... Twin Bedroom @ $.....

..... Parlor Suite @ $.....

☐ Check here if wheelchair-accessible room is required.

I will be sharing a room with

(If any of the above will be attending the convention, they must register also unless otherwise exempt.)

Mail confirmation to: Name.....

Address.....

City..... State..... Zip Code.....

Telephone Number.....

Mail this form and check for registration fee to:

AMERICAN PSYCHOLOGICAL ASSOCIATION
1200 Seventeenth Street, N.W., Room 601
Washington, D.C. 20036

No telephone reservations will be accepted. Advance Registration and Hotel Reservation forms will be returned if not accompanied by the check for registration fee where required.

PLEASE COMPLETE BOTH SIDES OF THIS FORM

PLEASE COMPLETE BOTH SIDES OF THIS FORM. THIS SIDE IS FOR HOTEL RESERVATIONS ONLY. THE REVERSE SIDE IS FOR ADVANCE REGISTRATION.

HOTEL AND RATE INFORMATION

HEADQUARTERS HOTELS

Hotel	Single	Double	Twin	3rd Person
Fairmont	$26	$32	$32	$8
International*	26	32	32	8
Marriott	26	32	32	3
Monteleone*	23	32	32	4
Royal Orleans	26	34	34	6

OTHER HOTELS

Bienville House	24 50	25 50	28	
Bourbon Orleans	24-32	28-36	36-45	4
Braniff Place	18.50-20	22.50-24 50	24.50-26.50	3
Chateau	17-22	20 24	21-26	
Dauphine Orleans	22 75	27 75	32.75	
de la Poste	26	30	32-35	
Delta Towers	18	24	24	4
Downtowner	22	30	30	
Downtown Howard Johnson	22	27	27	
Governor House	16-17	20-22	25-27	3
La Salle	13	15.50	17	2
Le Pavillon	26	32	32	5
Le Richelieu	22	26	28	
Place D'Armes	18	20-22	22-26	
Prince Conti	18	24-34	24-34	
Provincial	18-21	22-25	23.50-25	
Royal Sonesta	26	32	32	8
Saint Louis	—	—	32	
Tamanaca	15.75-17.75	17.75-19.75	21.75-23.75	
Thunderbird	16-18	18-20	19-22	
Vieux Carre	20.50-21.50	24.50-29.50	26.50-30.50	

*Wheelchair-accessible rooms available. See text for details.

Rates for suites are regular rates Rates vary with each hotel and range from $45 to $250.

Persons requesting family plan accommodations should indicate their requirements on a separate sheet of paper and attach it to the housing form The Housing Bureau will fulfill these requirements if such accommodations are available

Divisions are assigned to meeting facilities as follows:

Fairmont: 1, 2, 5, 10, 15, 16, 27, 30
Marriott: 3, 6, 7 14, 17, 20, 23, 25, 28
Monteleone. 24, 26
Royal Orleans 19, 21
Rivergate Exhibition Center/International Hotel: 8, 9, 12, 13, 18, 22, 29, 31, 32, 33, 34, 35

Rivergate Exhibition Center: Placement, Exhibits, Convention Locator System

Convention Child Care Service

Provision has been made for a professional child care service during the 1974 Convention. CON-SERV has been engaged and will offer an extensive program.

Parents desiring to use child care facilities must preregister no later than July 15, 1974. To preregister, complete the form at the bottom of this page. Please note that upon filing of the preregistration form and payment of the advance deposit as indicated on the form, CON-SERV will provide complete detailed information about the services offered and final registration materials necessary to plan for your children's comfort and safety. If for any reason your plans change, the deposit will be refunded in full if CON-SERV is notified of cancellation by August 10.

Some of the essential features of this service, as planned, are indicated below:

- The facility will be located in Braniff Place Hotel and will be open from 8:00 a.m. to 6:30 p.m., August 30–September 3, 1974. While the cost will be about $1.50 per hour per child, APA will subsidize $.75 of this cost. Hence, the cost to parents during the daytime will be $.75 per hour per child.

- If there is sufficient need, the facility will remain open after 6:30 p.m. until 12 midnight to accommodate parents who may wish to leave their children in the facility during evening hours. The parents will bear the full cost during these hours. If the need for evening child care does not justify keeping the facility open after 6:30 p.m., personnel at the facility will make every effort to help individual parents locate qualified babysitters to stay with children in their hotel rooms for those who might need this service. In any event, every effort will be made by CON-SERV to assist parents with evening child care arrangements.

- During the regular daytime hours, a variety of optional tours will also be offered at modest cost to parents. APA will subsidize part of the cost to the extent of $.75 per hour per child as though the child were in the facility.

- In order to evaluate the child care services at past conventions, the Task Force on Convention Child Care would like you to answer the following questions: (*a*) What was your general reaction to past child care facilities? (*b*) What was the least-liked aspect? (*c*) What was the most-liked aspect? (*d*) What was needed and not provided? Please attach your answers to the preregistration form.

Preregistration Form: Child Care Service—APA Convention
New Orleans, Louisiana, August 30–September 3, 1974

Preregistration form must be returned no later than July 15, 1974.

Name of Parent or Guardian: _____

Street Address: _____

City, State, Zip: _____

Name(s) of children to be registered in the child care facility:

Name	Age

If your child has a specific problem or handicap that we should be aware of, that is, speech impediment, retardation, epilepsy, etc., please let us know so that we may make the necessary provisions. _____

Please check this box if you are interested in tour information for your child

_____
Signature of Parent or Guardian

Please return this form to **Ms. Diane Snow, Con-serv, 1318 Arabella Street, New Orleans, Louisiana 70115,** and enclose a check in the amount of $7.50 for one child or $15.00 for two or more children as listed above. Make checks payable to "Con-serv." Indicate amount enclosed: $_____.

NOTE: This is only your preregistration form. Upon receipt of this form you will be furnished complete registration materials.

Standards for Educational and Psychological Tests

1974 revision.

Developed under a grant from the Russell Sage Foundation by a joint committee of members from the APA, the American Educational Research Association, and the National Council on Measurement in Education.

This current revision of **Standards** is directed equally toward establishing guidelines for test developers and test users. It proposes *essential, very desirable* and *desirable* considerations for the effective development and use of standardized tests. Additionally, each standard is accompanied by an exemplary or clarifying comment. 80 pp. $3 to APA, AERA, and NCME members; $5 to nonmembers.

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Employers of Psychologists:

If you have or anticipate openings for psychologists, you may wish to list them with the APA Convention Placement Service. The Job Description Form below is for this purpose. Additional copies supplied upon request.

A service fee of $5.00 per insertion will be required for listing in the **Convention Placement Bulletin.** FEE MUST ACCOMPANY COPY—no billing by APA—DEADLINE JULY 26. If you recruit at the Convention, an additional $10.00 fee will be required for use of the Placement Office facilities. This fee payable ONLY when you register at the Placement Office.

The FINAL FORM of your job description below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for publication. Please list your job data of the kind and in the order illustrated. Do not skip any lines. Thank you.

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$14,000-16,000/12 months.  Internship + 2 yrs experience in mental health
setting; teaching experience desirable.
Teach intro clinical psych to interns, nurses in university hospital: perform
diagnostic & therapeutic activities in univ mental health clinic; do research
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dept possible.
JA Smith, Chairman, Dept Psych, Univ of Erie, Centerville 3, NY.
See him Aug. 30, 31, Sept. 1, 2, am & pm, or write.
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Tentative interview schedule (Aug. 30-Sept. 2, am &/or pm, Sept. 3 until 1 pm are the available times).
If none, enter name to whom inquiries may be mailed.

KING COPY—Draft your job description here, following the above guidelines.

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L FORM—TYPE YOUR FINAL JOB DESCRIPTION WITHIN BOUNDARY ONLY.

Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research ☐ General Openings

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LEO SCHOPPMEYER, Fairmont, New Orleans, 529-7111. 3456 Shadygrove Drive,
Anytown, Ohio. APA member. PhD 1958 U of Minn.
1963-present: sr clinical psychologist, Fairview Psychiat Hosp, Ohio, diag &
ther wi children & adults, exp'l resch on commun rehab program, teaching
interns. 1960-63: clinical psychol, Brooklyn Mntl Hlth Clinic, NYC, diag &
ther incl supvsn of spec classes for retarded. 1959-60: clin psycho, VA Hosp,
Mpls, Minn., obj & proj test admin, supvsd therapy after 1 yr internship.
1 paper, 2 articles. Desire supervisory clinical psychology position in
forward looking community clinic setting. $18,500. East or midwest.
See Aug. 30, 31, Sept. 1, am & pm, or write.

Tentative interview schedule (Aug. 30-Sept. 2, am &/or pm, Sept. 3 until 1 pm are the available times). If not attending, so indicate.

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2. Check: ☐ Member ☐ Associate ☐ High School Teacher Affiliate ☐ Foreign Affiliate ☐ Student Journal Group ☐ Non-Member

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Comment

Two Views on IQs

In December 1969, Arthur Jensen published a now famous article in the *Harvard Educational Review* entitled "How Much Can We Boost IQ and Scholastic Achievement?" In that article he put forward a number of theses about the nature of intelligence tests, their correlation with learning ability, the influence of environment on the variance in test scores, the lack of success of compensatory programs, and the causation of racial differences in performance on standardized IQ tests. Exactly two years before, in November 1967, Professor Hyde published a paper in *Educational Research* entitled "The Culturally Disadvantaged: Psychological and Educational Aspects." This article has attracted little attention. Even Jensen's thorough bibliography to his 1969 article, containing some 159 references, made no mention of this paper although, as we shall see, he had read the manuscript prior to publication. In his article, Hyde discussed many of the same issues as Jensen but came to rather different conclusions. Philosophically and methodologically it is an interesting exercise to try and determine why two qualified investigators working with much of the same data reached rather different conclusions. I shall first set out the contrasts and then provide a hypothesis to explain the differences.

The Nature of Intelligence Tests

Hyde: Standard intelligence tests, such as the Stanford-Binet and the Wechsler, are measures of specific knowledge and problem solving skills which have been acquired by the testee at some time prior to the test situation. . . . The validity of the IQ as a measure of learning ability, therefore, depends to a large extent upon equal opportunity for exposure to knowledge and skills that the test calls upon. Since intelligence tests were originally devised to predict school performance, they call upon knowledge and cognitive skills similar to the kinds of learning required in school . . . [educability depends] upon a fund of prior knowledge, skills and acquired cognitive habits, much of which is tapped by intelligence tests [Hyde, 1967, p. 7].

Jensen: The term "intelligence" should be reserved for the rather specific meaning I have assigned to it, namely, the general factor common to standard tests of intelligence. Any one verbal definition of this factor is really inadequate, but, if we must define it in so many words, it is probably best thought of as a capacity for abstract reasoning and problem solving [Jensen, 1969, p. 19].

Thus we see Hyde pointing out the role played by the acquisition and retention of specific items of knowledge and acquired skills, and the importance of equal exposure of subjects to such items, while Jensen considers only those aspects of intelligence tests which measure the capacity for abstract reasoning and problem solving. A matter of emphasis, but as we shall see, the policy implications of the respective articles are determined precisely by what is put in the foreground and what is relegated to the background.

Correlation with Learning Ability and Test Bias

Hyde: Now, if IQ is a measure of learning rate, we should expect that learning tasks of the type used by experimental psychologists to study learning should show substantial positive correlations with IQ. This, in fact, is exactly what our research has found. . . . But here is the interesting thing: the correlation between IQ and learning ability, as measured directly in a controlled laboratory learning task, is much higher among middle-class children than among lower-class children. . . . This suggests that the IQ is almost totally unpredictive of learning ability in the low-IQ range of low socio-economic-status children. It should be noted that the majority of low socio-economic-status children are in the below-average IQ range. This is especially true for Negroes in the U.S.A. [Hyde, 1967, p. 7].

Jensen: We are finding that a unidimensional concept of intelligence is quite inadequate as a basis for understanding social class differences in ability [p. 109]. I have hypothesized two genotypically distinct basic processes underlying this continuum [of learning ability —GD], labeled Level I (associative ability) and Level II (conceptual ability) [p. 110]. In general, we find that Level I associative learning tasks correlate very substantially with IQ among middle-class children but have very low correlations with IQ among lower-class children [p. 113]. Standard IQ tests . . . tap mostly Level II ability. Thus, ordinary IQ tests are not seen as being "unfair" in the sense of yielding inaccurate or invalid measures for the many disadvantaged children who obtain low scores [Jensen, 1969, pp. 109, 110, 113, 115].

There are two usual senses of "test bias": (1) unequal predictivity in different groups and (2) measuring the intended variable less adequately in some groups than in others. Hyde argues that IQ tests are biased in Sense 1; Jensen argues that they are unbiased in Sense 2.

Environmental Influences on IQ Variance

Hyde: If the Stanford-Binet is administered to a large and truly representative example of the total population . . . we find that the distribution of IQs departs in a very systematic way from the normal Gaussian distribution. There is a bulge (i.e. excess frequency) in the lower half of the distribution, especially in the IQ range from about 65 to 90 [p. 8]. It has been hypothesized that the bulge in the lower half of the distribution of IQs is due to the proportion of the population reared under conditions which are below the threshold of those environmental influences necessary for the full development of genetic intellectual potential. Thus, presumably, if these environmental lacks were eliminated, the bulge in the distribution of IQs would be smoothed out. . . . It would be difficult to estimate precisely the average expected gain, but it is likely to be somewhere between 10 and 20 IQ points [p. 9]. Another line of evidence is quite consistent with this threshold hypothesis, namely the studies concerned with upward changes in the

IQ as a result of rather drastic environmental changes, either from "natural" causes or by means of experimental manipulation of the environment. . . . Thus, when children are removed as infants from very poor homes, in which the natural parents have subnormal IQs, and are placed in foster homes, in which the foster parents are of average or superior intelligence, the children will grow up to obtain IQs that may be from 10 to 30 points higher than would be predicted if they had been reared by their natural parents, and their educational attainments will be even higher (Skodak and Skeels, 1966) [Hyde, 1967, pp. 8, 9].

Jensen: Below a certain threshold of environmental adequacy, deprivation can have a markedly depressing effect on intelligence. . . . When I speak of subthreshold environmental deprivation, I do not refer to a mere lack of middle-class amenities. I refer to the extreme sensory and motor restrictions in environments such as those described by Skeels and Dye (1939) and Davis (1947), in which the subjects had little sensory stimulation of any kind and little contact with adults. . . . Typical culturally disadvantaged children are not reared in anything like the degree of sensory and motor deprivation that characterizes, say, the children of the Skeels study [Jensen, 1969, pp. 60–61].

Here the differences between Hyde and Jensen are extremely subtle. While they both agree on the plausibility of a threshold hypothesis, and agree that the threshold is relatively low, they nevertheless disagree as to how low it is—as can be seen from the contrasting studies that each refers to. Hyde uses as his example the Skodak and Skeels (1949) adoption study. The children involved in this study came from mothers whose average IQ was 86—below normal but not mentally retarded. The study that Jensen refers to, that of Skeels and Dye (1939), has true mothers whose average IQ was under 70—the threshold sometimes used as the borderline between mentally retarded and low normal. Thus, while it may be true that typical culturally deprived children are not reared in the degree of deprivation that is characteristic of those children involved in the Skeels and Dye study, it may be true that many are raised in homes typical of the Skodak and Skeels study. Again this is a matter of emphasis, but a crucial one in determining whether one is optimistic or pessimistic in estimating how much IQ can be changed by environmental influences.

Racial Differences in Intelligence

Hyde: There are probably socio-economic-status differences in innate intellectual potential *within* any particular racial group, but these innate differences would be diminished to the extent that intellectually irrelevant genetic factors, such as lightness of skin color and other caucasoid features, are important as determinants of social and occupational mobility. Therefore, the fact that Negroes and Mexicans are disproportionately represented in the lower end of the socio-economic-status scale cannot be interpreted as evidence of poor genetic potential. . . . Since we know that the Negro population for the most part has suffered socio-economic and cultural disadvantages for generations past, it seems a reasonable hypothesis that their low-average IQ is due to environmental rather than to genetic factors [Hyde, 1967, p. 10].

Jensen: So all we are left with are various lines of evidence, no one of which is definitive alone, but which, viewed all together, make it a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-white intelligence difference [Jensen, 1969, p. 82].

The difference in emphasis is obvious.

Compensatory Programs

Hyde: Finally, it must be emphasized that all educators who have worked with the disadvantaged are agreed that pre-school intervention without adequate follow-up in the first years of elementary school is inadequate, because the culturally disadvantaged child does not go home after school, as does the middle-class child, to what is essentially a tutorial situation. . . . We are gradually having to face the fact that, in order to break the cycle of poverty and cultural deprivation, the public school will have to assume for culturally disadvantaged children more of the responsibilities of good child-rearing. . . .The brutal fact is that for culturally disadvantaged children, these responsibilities are not being met . . . [Hyde, 1967, p. 19].

Jensen: Compensatory education has been tried and it apparently has failed. . . . The chief goal of compensatory education—to remedy the educational lag of disadvantaged children and thereby narrow the achievement gap between "minority" and "majority" pupils—has been utterly unrealized in any of the large compensatory education programs that have been evaluated so far. . . . What has gone wrong? . . . *Here is where our diagnosis should begin—with the concept of the IQ* . . . [Jensen, 1969, pp. 2, 3, 5].

Given Jensen's views on IQ, and the relative fixity of the IQ, it is clear how his views of social policy will differ from those of Hyde.

What accounts for the differences between Professor Hyde and Jensen? Can it be the mere passage of time? After all, Jensen's article was written some two years later. Is it the case that we had a more adequate theory of intelligence testing in 1969 than in 1967? Did we have better learning theories in 1969 than in 1967? Did we know more about the threshold for environmental deprivation in 1969 than in 1967? Had we discovered new data on the genetic basis of black–white differences in 1969 that we were unaware of in 1967? I find this implausible.

Is it perhaps that Professor Hyde is more "intelligent" than Professor Jensen or vice versa? Certain facts about intelligence testing make this explanation unlikely.[1]

In my view the best explanation for the divergence in views relies not on factors internal to the scientific discipline—such as new data or improved theories—but on the social and political climate within which the discipline functions. It *is* just a coincidence that 1968 (a national election year) fell between the dates of the two publications, but the election reflected changing intellectual and social currents in American society. Dissatisfaction with the accomplishments of various social welfare policies; the dim awareness that, in a society with a much reduced growth rate, issues of income distribution will be harder to handle on a consensus basis; the failure of social scientists to provide adequate predictive theories—all these issues were coming to the surface. The election

[1] Beyond age 8, correlations between repeated tests of general intelligence, corrected for unreliability of measurement, are between .9 and unity. See Bloom (1964).

transferred political power from an administration sympathetic to those theorists who promised data and hypotheses that justified the social welfare programs of the Great Society, to an administration prepared to listen to and support the work of more pessimistic and conservative theorists, and prepared to use their findings as a rationale for cutting back such social welfare programs. The time was right for the expressions of just those emphases in which Jensen's article differs from Hyde's.[2] I suspect, if and when a more favorable social climate develops, that Professor Hyde's contribution will be rescued from its undeserved obscurity. If this comment contributes to that end, I shall be thankful.[3]

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GERALD DWORKIN [4]
Department of Philosophy
University of Illinois
at Chicago Circle

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[2] Lest the reader suspect me of committing the genetic (sic) fallacy, I am not trying to appraise which of the two views is more adequate. For detailed treatment of this question, see Block and Dworkin (in press).

[3] As the reader probably will have perceived by now, Professor "Hyde's" article was in fact written in 1967 by Professor Jensen. See Jensen (1967).

[4] The author is now a visiting fellow at the Battelle Seattle Research Center, Seattle, Washington.

The Strange Case of Dr. Jensen and Mr. Hyde?

Substantive criticism has been tried and it apparently has failed.

Probably no other single article in the history of psychological publications has been subjected to so many niggling and nit-picking commentaries in so brief a time as my essay in the *Harvard Educational Review* (*HER*; Jensen, 1969). (For a bibliography of 117 articles about the *HER* article, see Jensen, 1973b, pp. 356–364.) Now we have Dworkin's (1974) comment, which adds a new wrinkle by pointing out some changes of position or emphasis between one of my articles published in 1967 and the *HER* article of 1969. Ascribing the *HER* article to Jensen and the pre-*HER* article to Hyde is a most novel twist indeed. One is used to seeing it the other way around.

Views and conclusions based on ideological dogma, it is true, should not be expected to change. But on the frontiers of an empirical science, why should anyone be surprised if an investigator's hypotheses, interpretations, and emphases show some changes over a two-year period? Any differences in this respect between my 1967 and 1969 articles simply reflect the results of my continuing study of the matters in question. As any scientist, my thinking is influenced by the research of others and by the results of my own investigations, which, since 1967, have involved the testing by me and my assistants of more than 15,000 children. When I wrote the 1967 article the extremely important Coleman Report had not yet appeared, nor the nationwide survey of compensatory education programs by the U.S. Commission on Civil Rights. A number of studies related to my Level I–Level II theory of mental abilities were undertaken between 1967 and 1969. The evolution of my thinking during this period is clearly reflected in several of my pre-*HER* articles (e.g., Jensen, 1968a, 1968b, 1968c, 1968d). Between the 1967 and 1969 articles I spent a year at the Center for Advanced Study in the Behavioral Sciences, which provided the opportunity to delve more thoroughly into the literature on the genetics of mental abilities. I had actually gone there with plans for a book on the psychology of the culturally disadvantaged, a book I conceived of as an elaboration of my 1967 article (Jensen, 1967). But the further I studied the research literature of the 1960s on the causes of individual differences in mental abilities and scholastic achievement, the more clearly it appeared to me that the doctrinaire disparagement of genetic thinking, and even more often the complete neglect of genetics in the study of human differences related to educability, amounted to a corruption of behavioral science by social ideology. I have always regarded behavioral science as continuous with biology.

I doubt that the changes in my views between 1967 and 1969 are of any greater magnitude or are essentially different in character than are the changes that can be noted in any other two-year period of my research career, which I have chronicled elsewhere (Jensen, 1974c). One should have no difficulty noting the continuing changes of a similar nature throughout my writings since the *HER* article: (*a*) I have been led to a clearer distinction between learning ability and intelligence by my continuing investigations and modifications of the Level I–Level II theory of mental abilities and of the interaction of Levels I and II with social class and race (Jensen, 1970, 1971, 1973e, 1974b); (*b*) the notion of the environmental threshold in intellectual development, first expressed in 1967, has undergone modifications in light of evidence, criticism, and parsimony in theoretical formulation, because the same phenomenon can be explained without this concept and in terms of a simpler model of genetic and environmental influences (Jensen, 1973a, pp. 175–179); (*c*) my studies of culture bias in mental tests have led me to disbelieve the most popular explanations of certain ethnic group and social class differences in IQ (Jensen, in press); (*d*) I have summarized the evidence that

has led me to even greater doubts than I had in 1969 about purely environmental explanations of particular racial and social class differences in intelligence (Jensen, 1973a, 1973f); (*e*) my 1967 interpretation of the Skodak and Skeels (1949) study did not have the benefit of my reanalysis of their original data by the methods of quantitative genetics (Jensen, 1973d); (*f*) measures such as Head Start, performance contracting, and busing to achieve racial balance, in light of studies since 1969, appear to me now less promising as means of improving education than at the time I wrote the *HER* article. Consequently, I have since put more emphasis on taking account of developmental readiness for school learning and on diversified educational programs in accord with *individual* differences in abilities (Jensen, 1973b, 1974a).

Finally, I believe that psychology can develop as a science only as it stays clear of subservience to political ideologies. I will continue to deplore any trends to the contrary.

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ARTHUR R. JENSEN
Institute of Human Learning
University of California, Berkeley

Further Comment on the Risky Shift

A comment on Dorwin Cartwright's (March 1973) article is, by the Journal's formal rules, no longer timely. But because I saw letters on the subject in the December 1973 issue, and because my own primary reaction to this research has not been reflected in anything I have read, I will make my reaction explicit and brief.

I think my chapter "Group Dynamics" in *Social Psychology* (Brown, 1965) bears some responsibility for having made risky shift experiments popular for a time among young social psychologists. In fact, I have read only a small part of the risky shift literature because my main work leaves little time for this reading. Cartwright's article seems to make its points in a scholarly, gentle fashion, and I have no quarrel with it. However, there is an important idea here, much more abstract than the risky shift, which, as far as I know, has been largely overlooked.

In *Social Psychology*, the term *risky shift* was not used because it is an incorrect nominalization for the phenomenon in question. What happens is that people usually make advance, private recommendations of action to a hypothetical *other* person in a particular set of story problems which are less risky than the recommendations made by group consensus after the group convenes, all private positions are made known, and often there is a discussion of the story problem. As far as I can see, there is no risk in the shift. It is not a shift that is risky to the persons making it, but rather a shift to recommendations involving greater risk than the central tendency of the original recommendations. Better, then, to say "shift to increased risk" phenomenon, but obviously it is not worth writing a letter to criticize the choice of a name for a phenomenon. Neither is it worthwhile making the defensive observation that the phenomenon was used in *Social Psychology* simply as a concrete example around which to build a discussion of thinking about group dynamics. What follows is, perhaps, worth a letter.

The explanation of the term *shift to increased risk*, which the chapter in my *Social Psychology* particularly favored, is as follows: For a certain set of story problems, certainly not all, most Americans probably think it desirable to be a bit more audacious

or risky than the average person. A degree of derring-do is probably valued when your advisee takes most of the burden of the risks on himself. Let us suppose that is true: that it is considered a virtue, or good quality, to be fairly, but not wildly, risky in certain situations by a majority of Americans, perhaps especially young ones. Giving advice in private, then, each participant means to be somewhat audacious. But how can he know *how* to be so since the situations and options are novel and invented? He cannot know what the average person would do. After the group has convened and each individual has made known his private recommendation, the central tendency can then be estimated and, therefore, the way the average person behaves calculated. Thus, in arriving at a group consensus, or at new individual recommendations, those who have in fact been exceptionally conservative or just average, but who *meant* to be audacious, will feel inclined to favor a new recommendation riskier than their first recommendations. It should also be true that those whose first recommendations turn out to be absurdly or wildly risky would feel some inclination to get closer to the central tendency, though still above it. The net result with just the right story problems and action options will be a shift to increased risk from the central tendency of initial private recommendations to the recommendation on which the group agrees or which is obtained a second time, from each person alone. The basic idea is that knowledge of a distribution of options, taken for a completely new situation, teaches one how to *realize* or make manifest the value he held all along.

I think *Social Psychology* did not make it clear enough that the interesting problem is not the existence or nonexistence of a risky shift. Quite obviously, from the data reported in the book, some story problems cause a shift to increased caution. But it is the abstract process, conceived in terms neither of risk nor caution alone, that is really interesting. The suggestion is that just as there are many situations in which individuals will be motivated to conform, to huddle up close to the central tendency (as in perceptual judgments), there is another class of situations in which people will be motivated to fall on one or the other side of the central tendency because they seek not to be average but better than average, or virtuous. To be virtuous, in any of an indefinite number of dimensions, is to be different from the mean—in the right direction and to the right degree.

Imagine if you can, though it is certainly false, that everyone who contributes to his community's annual united charities drive means to be "generous." In fact, of course, very many individuals give as little as they can get away with, or give only as much as is respectable (conformists). But suppose not. Then the question arises: How do you go about manifesting generosity in this situation? What *do* most people of your economic bracket give to the United Fund? I certainly don't know. Harvard University does not publish any sort of distribution of contributions made—probably wisely so because I suspect many would feel more inclined to be "fiscally cautious" than generous. But suppose this were a situation in which just the same value, *generosity*, were predominantly activated in everyone, and the facts were confidential so that no one knew how to be generous, which is a relative thing after all. If we recorded our intentions in private and in ignorance, and then recorded them again after full disclosure of the intentions of whatever group we take as a point of reference, should there not be a shift to increased generosity, a "generosity shift"? Incidentally, and obviously, it would depend in part on whether contributions were to be published or confidential.

It is the abstract class of cases that I think is interesting, not the risky shift. Suppose people do want to be, in some situations, more than average honest, truthful, sexually faithful, or whatever, but not so much more so as to risk canonization. How do they go about learning what to do? What are the central tendencies for cheating on one's income tax? Behaving with patriotic courage as a prisoner of war? Being scrupulous in the administration of research grants? I think this general class of problem—manifesting reasonable virtue—is at least as general and important as the conformity problem. This problem of virtue is very timely since judicial and congressional inquiries and empirical studies of sexual behavior are providing more information about actual behavior distributions in some areas than has previously been available—and are often generating surprise. The cry for "openness" on whatever level creates a need to know some of the probable consequences of increased openness.

As it has happened, researchers have not themselves been very audacious but have, most of them, stayed close to the original design. The studies multiplied, and so it came to seem as if the interesting problem was whether a risky shift exists, rather than the laws determining how one manifests a value or virtue in completely novel circumstances, which includes most circumstances. The risky shift, as such, is rather discontinuous with social-psychological theory generally and seems scarcely worth the effort and ingenuity devoted to it. In addition, the research story has become complicated and full of seeming contradictions, and psychology has wearied of it as it has wearied of many other problems when they reached this stage. In turning away from the risky shift, it would be a great pity if we also turned away from the abstract problem, which has simply not been represented adequately in the literature. The problem of learning from groups how to manifest in concrete situations the virtues or values one holds is important, timely, and perfectly continuous with social psychology generally.

If, in what I have said, I have overlooked someone's research report or shown ignorance of a published theoretical statement, then I apologize and admit to ignorance but not

deliberate slight. I have not, in my limited reading, come across what I think are the essential abstract issues, and it would be sad if psychology declared a vast important terrain out of fashion or trivial just because one small tract in that expanse may have grown tiresome.

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ROGER BROWN
Harvard University

Comment on Gergen

I should like to comment on the article by Gergen (October 1973). It is a basic contravention of human rights and human dignity to argue that we can set aside the possible effects of research procedures on humans until they are demonstrated to be harmful. If this attitude "reflects [in Gergen's words] only the historically dependent sentiments of but one segment of the plurality," so be it.

Gergen's attitude is similar to that of a client of mine who countered my objection to his failure to provide for informed consent with the remark, "If I tell them what I'm doing, they won't let me do it."

Gergen is entitled to investigate in an ethical fashion whether or not various research practices do indeed have their purported effects. The outcomes of such experiments do not bear on the ethical question. Ethical principles and standards are not held on such a contingent basis. One has regard for the rights of others quite independently of whether they have regard for their own rights, or for one's own for that matter.

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ARTHUR F. JOHNSON
Veterans Administration Center
Hines, Illinois

The Civil Liberties of the Participants in Psychological Research

Gergen (October 1973) contends that psychologists should make use of experimental techniques to identify the consequences which result both from behaving in accordance with the various ethical principles and from violating them during the course of psychological research. "If subjects remain unaffected by variations along these dimensions, then the establishment of the principles becomes highly questionable [p. 907]." What is missing from Gergen's analyses is any consideration of the personal rights of individual research participants. Such selective inattention should not be permitted to continue.

From Gergen's statements I infer that, if research on the consequences of adhering or not adhering to a particular principle yields results which show the null hypothesis to be tenable, then he would have no objection to the violation of that principle. I will leave aside the question of whether this research should be conducted prior to establishing a principle (as Gergen recommends) or whether it should constitute a means of evaluating an already established principle. In either case, it should be recognized that before conclusions could be drawn, it would be necessary that all relevant outcome variables be identified and studied. A violation of an ethical principle which does not affect a person's anxiety level as measured on a pencil-and-paper test may change his physiological responses; a violation which does not affect reported hostility toward the experimenter may change the subject's trust in others. The short-term consequences of a procedure may differ widely from the long-term consequences of the same procedure. The task Gergen proposes is virtually limitless. In addition, it would be necessary that steps be taken to minimize the occurrence of β errors since the consequences of failing to identify a true difference between the conditions are of such gravity. How would an "acceptable" level of error be determined?

My primary concern with Gergen's proposal is not with the methodological difficulties inhering in it but with the view Gergen has of the civil rights of research participants. Gergen wishes to consider research subjects as a class possessing certain right as a class. He believes that if damage can be demontrated to the class as a whole, then an ethical principle should be observed. He misunderstands the nature of civil liberties. Rights belong to individuals not to collectives. When a violation of rights occurs, it is some individual who suffers, not some abstract conceptual grouping. By continually referring to "subjects" as if they were an undifferentiated group, Gergen obscures the fact that each participant is a distinct individual entitled to the full protection of his rights. Even if no statistically significant effects of violating an ethical principle can be identified, the failure to adhere to the principle could still entail the violation of the personal rights of individual research subjects.

Consider a research project to evaluate the effects of adhering to or violating the principle of informed consent with respect to the inducing of a negative self-concept. The condition showing adherence to the principle would involve role playing while the other condition would entail deception. It may well be that a substantial majority of the subjects whose informed consent was not obtained actually enjoy their participation in the study and believe that they have benefited from finding out about their own gullibility. At the same time, a very small minority of the participants may be-

come disturbed by the experience and resent the deception that resulted in their participation in the study. The overall outcome of comparing the informed and noninformed groups may not yield a significant difference because the benefit to some subjects balances out the harm done to others. But note that the harm that ensues from the study is experienced by individuals, not by a collective termed "subjects."

What of those subjects who were harmed? It is of no solace to them to know that most people were not harmed by the research or even that "a significant increment in knowledge" was achieved from the study. A person does not lose his rights because others were not harmed by their participation under similar conditions

Consideration of the civil liberties of research participants has been much more extensive in the field of medicine than in the field of psychology. The Nuremberg Military Tribunal drafted a code on medical experimentation on human beings (*United States v. Karl Brandt*), and the World Medical Association has issued the Declaration of Helsinki on the subject (Katz, 1972). The Nuremberg Code contains the following statement:

> The voluntary consent of the human subject is absolutely essential. This means that the person involved should have legal capacity to give consent; should be so situated as to be able to exercise free power of choice, without the intervention of any element of force, fraud, deceit, duress, over-reaching, or other ulterior form of constraint or coercion; and should have sufficient knowledge and comprehension of the elements of the subject matter involved as to enable him to make an understanding and enlightened decision. This latter element requires that before the acceptance of an affirmative decision by the experimental subject there should be made known to him the nature, duration, and purpose of the experiment; the method and means by which it is to be conducted; all inconveniences and hazards reasonably to be expected; and the effects upon his health or person which may possibly come from his participation in the experiment [Katz, 1972, p. 305].

It is not sufficient for a physician to inform a research subject that on the average no harm is done by a particular procedure; he has an obligation to inform each individual participant of any possible negative outcomes that could reasonably be foreseen. If a researcher fails to do this he is subject to legal penalties. He may be sued for the recovery of any losses incurred as a result of the research and may, in addition, be sued for punitive damages.

There are very good reasons to believe that the same legal conditions which apply to medical research also apply to psychological research. A person does not waive any of his personal rights when he becomes a subject in a psychological study. The legal prohibitions on negligence, reckless endangerment, and fraud apply with full force in the relationship between a psychological experimenter and research participant. If a psychologist has any reason to believe that physical or psychological harm may result from participation in an experiment, then he has a legal, as well as an ethical, obligation to inform the prospective participant. It may be just a matter of time before this matter is tested in the courts, if, indeed, this has not already occurred.

Gergen's lack of concern with personal rights is to a large degree shared by the members of the Ad hoc Committee on Ethical Standards in Psychological Research (1973) who drafted the statement of ethical principles. Consideration is given to the aim of contributing to psychological science and to human welfare and concern is expressed over preserving the dignity and welfare of research participants. However, the personal rights of research subjects are seen as violable at the researcher's discretion. For example, in a discussion of mental stress the following sentence appears: "It must be ascertained that the importance of the research justifies the level of stress likely to be involved, and, *where possible*, the informed participant as well as the investigator must agree that such is the case [p. 70, italics added]." A subject in a study in which unanticipated stress was employed who experienced any damages as a result of his participation would have good legal grounds on which to sue the experimenter, no matter how important the particular research project.

The American Psychological Association may, for its own purposes, adopt a code of research ethics, but this in no way lessens the responsibility of the researcher to observe legal safeguards. A psychologist who is a defendant in a damage suit will not find himself absolved simply because he has followed the ethical principles endorsed by the Association, if at the same time he has violated the civil rights of the plaintiff. Psychologists must recognize that they are subject to the rule of law and to legal penalties. They ignore the civil liberties of research participants at their own peril.

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ALAN S. WATERMAN
Trenton State College

Back into Silk Purses

While in agreement with many of the specific points made by Bersoff (October 1973), I disagree with most of his conclusions and deplore the general tone of his article. In other words, I admire the cloth he has chosen to remake the "silk purse," but I am critical of the stitching.

I agree with the following points: (*a*) General intelligence testing should be replaced by "psychositua-

[illegible] assessment for evaluating [illegible] competence. [illegible] Mr. Bernard [illegible] has written an eloquent advocacy of the truth [illegible] tests as predictors of [illegible] performance [illegible] The Thematic Apperception Test (TAT) is [illegible] glossary for adequate assessment of an individual's emotional range and expressiveness [illegible] Projective tests are good predictors of specific behavior and outcome.

On the other hand I disagree with the following Bernard generalizations: a. Psychoanalysts and other psychodynamically oriented practitioners tend to a theory [illegible] that views behavior as relatively independent of the situation in which the person is observed (p. 97). The word relatively is ambiguous. If it means relative to behavior theory, then I take no issue with Bernard's point. However, if it means extremely, then I think that Bernard is guilty of at the very most overstatement and at the most ignorance of Schafer's third chapter on the test situation. b. I also disagree with the implication that traditional psychological tests in general and projective tests in particular are not like psychotherapy. Anyone who has had sought experience with projective techniques such as the Rorschach and TAT must appreciate the idiographic nature of the test results because truly projective test protocols and human personality stories can be described by the same phrase, namely, "No two are alike" (p). I disagree with the contention that projective techniques should be [illegible] in favor of "psychodynamic interviews" or other techniques that are more [illegible] specific and better at prediction. Correct better prediction is an important if not essential goal for psychodynamic theory, but understanding an individual's conscious, preconscious, unconscious cross sections, psychodynamics, motivational state and self-concepts is about having the kind of generality description that remainder from projective techniques. Also, considerable clinical and research evidence. For accurate single predictions and yes, prescriptions, the clinician must have more than the individual's specific reaction to specific situations. He must also be familiar with much of what the individual brings to each situation in the package commonly referred to as personality. I propose that a distinction between psychodiagnostic assessment and traditional projective testing is necessary for the ultimate in psychotherapy and decision making. I am sure, Dr. Bernard, that the evidence suggests that the [illegible] phenomenologies of perceptive [illegible] and mediating structures and mechanisms. Whether [illegible] p. [illegible] does indeed exist and calling it names will not make it go away or diminish its importance. If projective testing is eliminated then I believe that psychological assessment will be at the very best no better off than before and at the worst hopelessly redundant.

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Roger A. Ritzler
University of Rochester

On Bernard's "Will Parrots"

Bernard's (October 1974) comments on the decline and fall of psychological testing raised a number of interesting and [illegible] issues, some of which I find well taken and pertinent but in the context of what appears to be a gross lack of familiarity with the fundamental principles of psychodynamic psychological testing. For the record, some of his specific criticisms appear to [illegible] for they lead to a basic conceptual chaos which is by no means accurate. [illegible] and which [illegible] [illegible].

Now, I suspect, would question Bernard's observation that "[illegible] measure something [illegible] fairly related to a capacity to deal intelligently and creatively with the demands of the real world." [illegible] "IQ" tests are only [illegible] from a narrow vantage point. [illegible] psychodynamically viewed, they [illegible] rather a style or manner of thinking, a characteristic way of [illegible] when faced with tasks requiring a solution that is [illegible] qualitative and quantitative. This however is a minor point. More significant is Bernard's notion that because psychological testing traditionally occurs in an "artificial" setting, it is of limited relevance and generalizability to other contexts is highly speculative. Further, he suggests that "testers" tend not to be aware of the extent of contextual influences upon test performance. While it may be unwise for me to speak for others, these contentions fly in the face of what psychodynamic testers are typically taught and hopefully learn about the instruments that they use. Psychodynamic test data are not to be examined in a vacuum (although Bernard may be right that this is an ideal which we only imperfectly live up to), but rather arrive in an interpersonal and interpersonal matrix which must be noted and integrated into test findings. Presumably, [illegible] are with a psychodynamic orientation will, for example, take into consideration the likelihood that an individual referred for testing because he is a behavior problem in school will approach and participate in testing differently than an individual referred because he is being considered for an executive business position. Standard texts on psychodynamic testing (Allison, Blatt, & Zimet, [illegible] Schafer, 1968) devote much [illegible] to precisely these issues.

Another related misconception about the contextual factors in testing comes up in Bernard's description of the TAT. He seems unaware of the [illegible] of testers who would make something of a subject's

[illegible] might [illegible] to [illegible] depend the [illegible] consequences assessed. When a [illegible] gives [illegible] to the [illegible] exactly what he sees [illegible] what basis for [illegible] is there. Well if this were what happens and I suppose [illegible] I wouldn't agree [illegible] However again Bersoff reveals that he has much to learn about a psychodynamic approach to testing before he is on a valid path [illegible] That a subject sees a depressing story to a depressive picture is no news. What is news however is how he constructs the story with what particulars he fashions out the theme what motives he ascribes to the characters etc. These aspects of a story are not given to be said and their specification reflects upon the storyteller not the said.

These detailed examples of Bersoff's [illegible] experience about psychological testing prompt me to suggest that he may not be the most well [illegible] to delineate the [illegible] the apparent decline in the [illegible] of psychodynamic testing. This may in effect raise question about the informed nature of his [illegible] It is as if he had taken upon himself the task of criticizing the [illegible] of a language when he has not been the syntax of the language that is the basic rules by which a language is constructed. Instead Bersoff appears to have fallen [illegible] of precisely what he has criticized traditional testing for. He [illegible] to have failed to appreciate the context in which testing has [illegible] fashion. Among many [illegible] factors, I would suggest that the fall from fashion of [illegible] of testing has more to do with its time-consuming and therefore expensive qualities, its requirement for highly trained practitioners for administration and interpretation and its dynamic orientation which as he suggests its dubious validity. At a time when most of the [illegible] mental health funds are geared up to provide crisis intervention, brief rapid assessments and brief treatment psychotherapy testing as we have known it has been as such thematic history assessment and not event to only those factors well reckoned enough to justify for example longer term and more extensive therapeutic goals.

Another factor is also involved and this may be the most telling because it is the most conceptual and therefore involves basic assumptions about people and personality. Here I am referring to the question of what it is about people that one wishes to assess. Increasingly psychological tests have been used to predict behavior most observable behavior in situations different from the testing context. Bersoff's assumption is that this is what psychological tests ought to predict validity for him means the accurate prediction of behavior. If behavior is what we want our tests to predict then he is right when he says that our traditional tests do this imperfectly if at all and when he calls for the design of new tests which will improve upon this predictive ability. If my understanding is correct the standard projective tests were never meant to predict behavior patterns but rather to describe and predict *ideation*, mental processes. The Rapaport-Schafer orientation toward these tests is heavily loaded in the direction of *ideation*. Now clearly behavior and ideation are related, but in no simple and direct fashion. We cannot easily and with good professional conscience say precisely what behaviors will correspond to given ideational processes and clearly and with just as much force we cannot say precisely what ideational processes correspond with given behaviors. More is needed to bridge behavior and ideation since in our eagerness now to turn our attention to behavior and certainly much of contemporary society urges this shift in our attention let us not mindlessly assume that behavior is consonant with personality or persons and call for the abandonment of those testing techniques which appropriately gauge the ideational side of personality. It is just that as Bersoff notes in the [illegible] drawn from by Bersoff with which I cannot disagree.

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C. Norman Herrington
Department of Psychiatry
Duke University

On Bersoff et al

The empirical study reported by Bersoff et al. (October 1973) is a significant contribution toward the goal of making the revision of the Ethical Standards for Research with Human Subjects (Cook et al. 1970) regarding informed consent. However, this study does not live up to the Standards in "protecting the dignity of the research participants" (Principle 3) and in "assuring the quality of the relationship between investigator and research participant" (Principle 4).

As Bersoff (1973) has pointed out the danger in most of the studies is not that participants will be psychologically damaged but rather that the participants have not been informed. Nonetheless points should be explored by asking how even when a study does make a strong appeal for cooperation in the research, designers the perception of the research participants so that there have been wasted and that they were misplaced. Under such circumstances the study comes to be perceived mainly as an exercise in power, the greater power that is characteristic of the role of academician researcher as opposed to the role of academician research participant. Rosenthal (1973) in ordinary conversation [illegible]

the [illegible] research [illegible] [illegible] something which [illegible] [illegible] [illegible]

Psychology in Action

NEW SOVIET RESEARCH INSTITUTE OF PSYCHOLOGY

A MILESTONE IN THE DEVELOPMENT OF PSYCHOLOGY IN THE USSR

JOSEF BROŽEK *Lehigh University*[1]

LUCIANO MECACCI *Institute of Psychology*
National Research Council, Rome

The decision in December 1971 to form an Institute of Psychology attached to the USSR Academy of Sciences was a major event in the recent history of Soviet psychology. The formation of the Institute is comparable in significance to such milestones as the establishment in 1915 of a psychological journal (*Voprosy psikhologii*) after a lapse of some 20 years, the foundation in 1917 of the Soviet Psychological Society, the convening in Moscow of the 18th International Congress of Psychology in 1966, the establishment of a Faculty of Psychology in 1966–1967 at the Universities of Moscow and Leningrad, and the 1968 decree of the Council of Ministers of the USSR which permitted awarding advanced degrees in the field of "psychological sciences" (Brožek, 1970, 1972).

The Institute is a visible indicator of recognition, at the highest level, of the advances made by Soviet psychology. With B. F. Lomov (b. January 28, 1927) as Director of the Institute, the younger generation of Soviet psychologists is coming to the forefront.

The emphasis stressed by Lomov, to pay special attention to the structure of contemporary scientific psychology and the trends in its development, is in keeping with his open-mindedness and absorptive capacity demonstrated in the 1960s by Lomov as Director of the Laboratory of Engineering Psychology at the Leningrad State University.

The new scientific center is to become the leading Soviet research establishment in the fields of general experimental, social, industrial, and engineering psychology. The Institute's research personnel will include not only psychologists but also representatives of other disciplines, such as physiology and cybernetics.

As a result of the distinct role of the scientific academies, the administrative structure of Soviet research differs so radically from the American, for example, that it may be useful to examine the birth of the new Institute of Psychology within a broader historical perspective.

The Place of Academies in the Soviet Institutional Setting

Cole and Maltzman (1969) expressed vividly the "culture shock" experienced by Western psychologists first encountering the Soviet scene. Looking upon Soviet psychology and psychologists, visiting the field for the first time is a little like Darwin first visiting the Galapagos. Different forms of species have evolved as a result of isolation and interbreeding.

This applies also to the organizational aspects of psychology. While the [illegible] Moscow and Leningrad State Universities are also important centers of research, in the Soviet system the teaching function of universities is heavily emphasized. Research is the primary responsibility of the academies.

The organizational structure of the Soviet system of scientific academies is complex. We shall, in our discussion, here primarily with the Academy of Sciences, and reviews only the academies of the individual republics, even though some of the issues are relevant. For example, the Georgian Academy of Sciences in Tbilisi includes the prominent D. N. Uznadze Institute of Psychology (founded by A. S. Prangishvili).

The highest scientific organ of the Soviet Union, the Soviet Academy, is the USSR Academy of Sciences. Here is the Academy of Sciences established by Peter the Great in 1724 (Kovalev & Kostina, 1968).

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[1] Much of the background information for this article was obtained during exchange discussions with the USSR Academy of Sciences arranged by the National Academy of Sciences for Josef Brožek and by the Consiglio Nazionale delle Ricerche for Luciano Mecacci.

Requests for reprints should be sent to Josef Brožek, Department of Psychology, Lehigh University, Bethlehem, Pennsylvania 18015, U.S.A., or to Luciano Mecacci, Via del Monte Tibertino 60, 00117 Rome, Italy.

psychological phenomena, their synthesis, and the generalization of the findings, consideration will be given to the mutual relations between different fields of psychology cultivated at the Institute and to the ways in which they are "locked" into the general theory of psychology. A dependable general-theoretical basis will increase the effectiveness of the special branches of scientific psychology. The psychology to be pursued at the Institute is viewed as a comprehensive, coherent system dealing with man's activities in the areas of cognition, work, and communication. Heavy emphasis is being placed by Lomov on the need for close cooperation with other scientific disciplines and with other institutions of the Academy of Sciences.

In the humanities ("philosophical sciences," according to the Russian usage), psychological research is expected to contribute a creative approach to such problems as the role of the individual—the "subjective factor"—in historical processes, in mass movements, and in the organization and management of society but also in the generation of knowledge.

Among the problems at the interface of psychology and the "concrete social sciences" are noted the investigation of social groups, including the collectives of workers; increased productivity of workers; management of economic, sociopolitical, and cultural processes and ideological efforts; education, personality, and society, and psychological aspects of law, ethics, and aesthetics.

Psychology interacts with the complex of biological sciences, especially with the physiology of the human organism, its development, and the regulation of its functions, including "higher nervous activity" (conditioning), sensation, and speech. Modeling of mental functions brings psychology into contact with cybernetics.

Psychology is involved in the different aspects of the "man and technology" complex, for example, the interaction between the human operator and electronic computers, and exploration of outer space, including the selection and training of astronauts.

In summary, in Lomov's view, the principal responsibilities of the Institute will involve (*a*) basic research in psychology and provision of the theoretical foundations of applied psychology; (*b*) participation in interdisciplinary ("complex") investigations involving the "human factor," as in research on space travel and industrial work; (*c*) research required for the formulation of long-range plans for the development of psychology in the Soviet Union; and (*d*) advanced training of scientific personnel in selected specialties of psychology.

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A LOOK AT BRITISH GRADUATE STUDENTS

THOMAS I. DE VOL *University of Missouri–Columbia* [1]

The most recent survey of psychology training programs in the United Kingdom was conducted by Arthur Summerfield at Birkbeck College, University of London, in 1965 for the American Psychological Association (APA) and the International Union of Psychological Science (APA, 1966). The present survey was undertaken to identify critical student enrollment characteristics that were not surveyed by Summerfield. These characteristics included total postgraduate enrollment, doctoral student enrollment, age at entry into and graduation from doctoral-level training programs, and minority group representation.

A general survey form was constructed for the present purpose and mailed with a cover letter and return postage in late 1972 and early 1973 to 89 departments offering advanced courses and/or supervision for research leading to the PhD with a major in one or more subspecialties of psychology. The 89 departments were identified through three sources: the APA (1966), the British Psychological Society (1972), and the (British) Social Science Research Council (1972). Departments that failed to issue survey responses within 90 days were sent reminders.

Findings will be discussed in reference to comparable American survey findings. A total of 34 (38%) surveys were returned at various levels of completion. In a number of instances respondents representing small departments or newly established graduate programs were unable to provide accurate estimates of many of the statistics they were asked to report. In addition, two would-be respondents representing established university departments refused to provide any information.

General Enrollment

Thirty-four departments issued enrollment data for all postbaccalaureate psychology students. The total enrollment for these departments combined was 1,112. Only 277 of these students were enrolled in PhD programs. An estimate of total United Kingdom psychology graduate student enrollment was made by taking the average(s) for each of the departments that reported enrollment data and multiplying this by the total number of known United Kingdom postgraduate psychology training programs. The average for all postgraduate programs combined was 38.3. This figure times the total number of departments (89) yielded a projected total enrollment of 3,408.[2] A comparable projection based on El-Khawas and Astin's (1972) survey of 297 (89%) departments in the United States offering postgraduate training in psychology yielded a total enrollment figure 8.4 times the United Kingdom estimate: 28,737 ($\bar{X}$ 86.04 × 334 programs). Since the population of England, Wales, Scotland, and Northern Ireland is at least one fourth that of the United States (55,347,000; William Clowes & Sons Limited, 1973), it is not possible to discount the smaller United Kingdom postgraduate enrollment as a direct function of a smaller population base. With this taken into consideration it is compelling to assume that psychology as a discipline is not supported by the British to the same degree that it is supported by Americans. An even greater discrepancy exists between the two countries at the doctoral level. The ratio here is 14.2:1 favoring the United States. (United Kingdom: $\bar{X}$ 9.56 × 89 programs = 814; United States: $\bar{X}$ 92.92 × 125 programs = 11,615 [Khanna & Khanna, 1972]). Two factors that figure heavily in the very small doctoral enrollment in the United Kingdom are (*a*) full recognition of master's- or diploma-level psychologists given by the Department of Health and Social Security and the British Psychological Society (BPS, 1972) and (*b*) British tutorial training by research only at the doctoral level. In a number of cases, only one or two PhD students were listed. Certainly, no question can be raised about the quality of doctoral training in these departments. Given the shrinking job market for PhDs in this country along with major drawbacks inherent in

[1] The author is indebted to Sherman Ross, former Director of the International Opportunities for Advanced Training and Research in Psychology Project, and Richard Thoreson, American Visiting Professor, Reading University, for their timely advice, and to Robert S. Daniel, Professor of Psychology, University of Missouri, for supervising the project.

Requests for reprints should be sent to Thomas I. De Vol, Counseling Psychology Section (183 W), Veterans Administration Hospital, 10701 East Boulevard, Cleveland, Ohio 44106.

[2] This and other projections should be accepted with caution. The colleges, universities, and technical schools from whom responses were obtained selected themselves into the sample. It is possible, for instance, that a disproportionate share of larger departments selected themselves into the sample. If this were the case, estimates of total enrollment would be inflated.

"mass producing" PhDs (e.g., Khanna & Khanna, 1972, critique of the American comprehensive examination), the English tutorial system may be given increasing attention in the not too distant future by American educators.

Minority Representation

WOMEN

Approximately 30% (30.5%) of the students registered for doctoral study were female ($N_{programs} = 18$). El-Khawas and Astin (1972) reported that women comprised 40% of their sample of *all* graduate students (MAs and PhDs) in the United States. Thus, on the surface at least, it appears that females are not represented in the United Kingdom to the degree they are in the United States. However, if a survey of female doctoral students *only* were to be taken in this country it might well show a much smaller percentage of females (El-Khawas & Astin, 1972). An informal tally of new (1973) APA members (who it may be assumed are recent American university PhD recipients) taken by the writer, for instance, revealed that women were outnumbered by men roughly three to one ($417 \pm_{women}/1{,}742_{total}$).

NONWHITE

Eighteen departments provided estimates of the percentage of nonwhites enrolled in their respective doctoral programs. The mean percentage for these departments (combined) was 3.72, falling 3.28 percentage points short of the *combined* MA and PhD nonwhite enrollment in this country (El-Khawas & Astin, 1972). Here again the possibility that such figures are not comparable presents itself. Furthermore, it may well be that there are fewer nonwhites in the United Kingdom population as a whole than in the United States. Unfortunately, the United Kingdom census bureau does not issue official statistics for minority classifications (Central Statistical Office, 1970).

Age at Entry and Graduation

The mean age of beginning doctoral students was 23.58 and the mean age at graduation was 26.7 ($N_{departments} = 24$). Pressey (1965) reported a *median* age for graduation of PhD students in this country of 31. Pressey (1965) also sampled APA presidents and other renowned American psychologists and found an early age pattern close to the figure typifying the British and the Irish: APA presidents: $Mdn = 25.7$ ($N = 25$); E. L. Thorndike, 23; Kohler, 22; etc. Pressey concluded from his findings that we do a disservice to students by extending their education past the age of 26. The fact that the British are able to graduate half of their PhD psychologists at age 26 and younger suggests that Pressey's position is as realistic (feasible) as it is desirable.

It may be concluded on the bases of the present survey and comparable domestic survey data that (*a*) United Kingdom postgraduate training in psychology (at both beginning and advanced levels) is conducted on a much smaller scale than in the United States, (*b*) United Kingdom students enter doctoral training at an early age and typically graduate at a much earlier age than students in this country, and (*c*) females and nonwhite minority groups are well represented in doctoral training programs in the United Kingdom.

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Announcements

APA Representation

Paul L. Crawford, West Virginia State College, represented APA at the inauguration of Harold Maceo McNeill as President of West Virginia State College on April 19, 1974.

James C. Parsons, Alaska Methodist University, Anchorage, represented APA at the inauguration of Robert W. Hiatt as President of the University of Alaska on May 4, 1974.

Kenneth F. Schenkel, Southern Bell T&T Company, Atlanta, Georgia, represented APA at the inauguration of Marvin Banks Perry, Jr., as President of Agnes Scott College on May 18, 1974, in Decatur, Georgia.

American Board of Professional Psychology

The *American Board of Professional Psychology,* during the period January 21–March 18, 1974, awarded its diploma in the specialities of Clinical, Counseling, Industrial and Organizational, and School Psychology to the following:

CLINICAL
Carol Jean Rogalski

COUNSELING
Richard Earl Hardy

INDUSTRIAL AND ORGANIZATIONAL
George W. Henderson, Jr.

SCHOOL
Francis J. Lodato

Deaths

Helen Walker Bechtel, February 14, 1974
Olga L. Bridgman, February 6, 1974
Charlotte Bühler, February 3, 1974
Harold D. Carter, March 3, 1973
Neil W. Coppinger, February 28, 1974
Walter J. Coville, date unknown
Phyllis Culmer, March 1, 1974
Louis Delman, October 1973
Thomas J. Erwin, March 6, 1974
Marvin J. Feldman, September 29, 1973
Cecil Max Freeburne, February 19, 1974
Esther A. Gaw, date unknown
Carlyle F. Jacobsen, March 14, 1974
Donald H. Kase, date unknown
William R. Knievel, February 19, 1974
Paul J. Kruse, date unknown
Arthur J. Looby, November 9, 1973
Hallack McCord, date unknown
James T. Morton, March 8, 1974
Ralf A. Peckham, March 7, 1974
Carl E. Rankin, February 24, 1974
Margaret Rose, February 26, 1974
Robert W. Shaw, January 11, 1974
Ralph R. Wolf, Jr., January 26, 1974

Predoctoral, Doctoral, and Postdoctoral Programs

Adelphi University Postdoctoral Program in Psychotherapy: Now accepting applications for admission for fall semester 1974. The Program in the Institute of Advanced Psychological Studies is a four-year curriculum in psychoanalysis and psychotherapy encompassing personal therapy, supervision, academic courses, and seminars and research. The Program also announces a two-year specialty program in group therapy, beginning in the fall 1974. A PhD in psychology and two years of postdoctoral psychotherapy training are requisites for admission. For further information and applications for either program write to Donald S. Milman, Codirector, Postdoctoral Program in Psychotherapy, Institute of Advanced Psychological Studies, Adelphi University, Garden City, New York 11530. When requesting information, please specify which program.

Edward W. Cook School of Parapsychiatry: Announces a professional development program leading to a master's degree in counseling and psychotherapy, awarded by Antioch College. The program establishes a new profession, that of parapsychiatry. A parapsychiatrist is a professional mental health worker trained in the diagnosis and treatment of psychiatric disorders in adolescents. The parapsychiatrist works under the supervision of a consulting psychiatrist. Two full years of training are required for completion of the master's degree program: an 18-month practical and didactic curriculum and a 6-month internship. The program is oriented toward the achievement of competency in four basic areas: academic, diagnostic, treatment skills, and sociotherapeutic techniques. The academic framework is basically psychoanalytic, and the graduate will be qualified to practice psychotherapy as part of a mental health team. Interested persons should write to the Office of the Provost, Constance Bultman Wilson Center for Education and Psychiatry, Faribault, Minnesota 55021.

Elmcrest Psychiatric Institute: Now accepting applications for pre- and postdoctoral internships in clinical psychology. The Institute is an expanding inpatient mental health fa-

cility with emphasis on intensive therapeutic community. It offers supervised experience and seminar teaching of group therapy, individual therapy, psychodrama, and psychological testing. There is a special focus on family therapy and multiple family group therapy. Interns have the opportunity to explore their interests in art therapy and movement therapy. The Program is flexible and can be adapted to the needs and interests of the individual. Stipends are available. Send application and vita to Fredda S. Kelly, Elmcrest Psychiatric Institute, 25 Marlborough Street, Portland, Connecticut 06480.

Indiana University School of Medicine: Announces a postdoctoral fellowship in clinical child psychology with advanced training in diagnosis and treatment of children, adolescents, and families; school and community consultation; developmental disabilities; neuropsychology; research; and supervision. The fellowship is for one or two years. The stipend is $8,500, plus $600 per dependent, $3,600 of which is tax exempt. Applicants must have a doctorate in clinical psychology and a clinical internship. The fellowship can start at any time. For further information write to Aare Truumaa, Section of Psychology, Department of Psychiatry, Indiana University School of Medicine, Indianapolis, Indiana 46202.

National Institute for the Psychotherapies: Announces an innovative training program in psychotherapy. It is now accepting applications for its certificate program in comprehensive psychotherapy for fall 1974. The Institute, chartered by the Regents of the State of New York, offers a unique, psychodynamically based training program, which, over four years of part-time study, provides integrated, elective instruction in the major psychotherapeutic modalities, for example, analysis, behavior therapy, Gestalt. Eligibility is limited to qualified psychiatrists, psychologists, social workers, and psychiatric nurses. For further information write to Kenneth Frank, Codirector of Training, National Institute for the Psychotherapies, Suite 200, 330 West 58th Street, New York, New York 10019.

Piedmont Summer Program, Wake Forest University: Announces its three-week residential workshop for professionals who wish to expand their group leadership skills, June 23 through July 14, 1974, in Winston-Salem, North Carolina. Active participation in training sessions and coleading a weekend growth group provide an experiential focus. Graduate credit is available. Drawing upon the varied resources of psychotherapy, counseling, and the human potentials movement, study includes training in personal awareness and interpersonal skills, transactional analysis, Gestalt techniques, experiential teaching, family therapy, human sexuality, psychodrama, T-group procedures, leadership dynamics and ethics, body movement, yoga, expressive art, and other body-minded procedures. For further information write to John Woodmansee, Piedmont Summer Program, Wake Forest University, P.O. Box 6129, Winston-Salem, North Carolina 27109.

Postdoctoral Research Training, Department of Psychiatry, Montefiore Hospital and Medical Center: Fellowship positions are available through National Institute of Mental Health-supported programs that provide basic preparation for research in areas relevant to mental health and brain and behavior. The Program is available to individuals who have completed the requirements for a doctorate degree. Emphasis is on acquiring proficiency in the fundamental principles of research strategy, methodology, and techniques currently employed in laboratory and clinical studies in psychiatry. The traineeships stress active participation in research work under the supervision of faculty preceptors, supplemented by a curriculum of seminars and workshops. For further information write to Herbert Weiner, Department of Psychiatry, Montefiore Hospital and Medical Center, 111 East 210th Street, Bronx, New York 10467.

APF Journal Subscription Grants

American Psychological Foundation Journal Subscription Grants: Funds are available from the American Psychological Foundation for APA journal subscription grants to postdoctoral foreign students trained in American and Canadian universities and now residing in their native lands. These grants, limited in number, are intended for centers that are interested in developing local research and scholarship but that lack financial resources for such purposes. The subscription awards will cover the cost of approximately five APA journals. Applicants would choose any five from a complete list of APA journals.[1] Each grant will run for a five-year period. Applications can be made by any foreign university, college, or research center in the form of a letter to Harry C. Triandis, Department of Psychology, University of Illinois, Champaign, Illinois, 61820, USA, and should include assurances to the APF Committee that (*a*) the journals would be made available to undergraduate and graduate students as well as faculty, (*b*) the journals would be controlled by a librarian at an educational or research institution, and (*c*) there is a need for outside assistance of this sort.

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[1] *American Psychologist, Contemporary Psychology, Developmental Psychology, Journal of Abnormal Psychology, Journal of Applied Psychology, Journal of Comparative and Physiological Psychology, Journal of Consulting Psychology, Journal of Counseling Psychology, Journal of Educational Psychology, Journal of Experimental Psychology, Journal of Personality and Social Psychology, Professional Psychology, Psychological Abstracts, Psychological Bulletin, and Psychological Review.*

National Conventions

American Psychological Association: August 30–September 3, 1974, New Orleans; 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco

For information write to:

Carl N. Zimet
c/o Miss Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Southwestern Psychological Association: April 17–19, 1975; Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Rocky Mountain Psychological Association: 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque

For information write to:

Irwin Cohen
Veterans Administration Hospital
Mental Hygiene Clinic
1055 Clermont Street
Denver, Colorado 80220

New England Psychological Association: November 15–16, 1974; Boston, Massachusetts

For information write to:

Barbara Ross
University of Massachusetts
100 Arlington Street
Boston, Massachusetts 02116

Instrumentation Seminar on Control and Data Acquisition in Behavioral and Biofeedback Applications: June 24–25, 1974; Lehigh Valley, Pennsylvania

For information write to:

Coulbourn Instruments, Inc.
P.O. Box 2551
Lehigh Valley, Pennsylvania 18100

Conference on Humanistic Education: August 21–23, 1974; Carrollton, Georgia

For information write to:

David Ryback
Education Center
West Georgia College
Carrollton, Georgia 30117

Association for Humanistic Psychology: August 25–28, 1974; New Orleans, Louisiana

For information write to:

John Levy
Association for Humanistic Psychology
325 Ninth Street
San Francisco, California 94103

Postdoctoral Institute: August 27–29, 1974; New Orleans, Louisiana

For information write to:

Arthur J. Gallese
Department of Psychiatry
Louisiana State University
Medical School
1542 Tulane Avenue
New Orleans, Louisiana 70112

Society for Ear, Nose, and Throat Advances in Children: October 18–19, 1974; San Francisco, California

For information write to:

Virgil M. Howie
2345 Whitesburg Drive, S.E.
Huntsville, Alabama 35801

Family Institute of Philadelphia: October 24–25, 1974; King of Prussia, Pennsylvania

For information write to:

Florence Kaslow
807 Buckingham Road
Rydal, Pennsylvania 19046

Rocky Mountain Educational Research Association: October 30–November 1, 1974; Albuquerque, New Mexico

For information write to:

James G. Cooper
Educational Foundations
University of New Mexico
Albuquerque, New Mexico 87131

Association for Advancement of Behavior Therapy: November 1–3, 1974; Chicago, Illinois

For information write to:

David H. Barlow
Program Chair, AABT
305 East 45th Street
New York, New York 10017

American Society of Clinical Hypnosis: November 5–10, 1974; New Orleans, Louisiana

For information write to:

F. D. Nowlin
American Society of Clinical Hypnosis
800 Washington Avenue Southeast
Minneapolis, Minnesota 55414

Third Information and Feedback Conference: People Services: November 7–8, 1974; Toronto, Canada

For information write to:

IF Conference Committee
Counselling and Development Centre
York University
Toronto, Ontario, Canada

Conference on Behavioral Research and Technology in Higher Education: November 14–16, 1974; Atlanta, Georgia

For information write to:

Susan E. Hansen
Department of Psychology
Georgia State University
33 Gilmer Street, S.E.
Atlanta, Georgia 30303

Southwestern Partial Hospitalization Association: November 16–17, 1974; Dallas, Texas

For information write to:

Lenore Mankoff
Timberlawn Day Hospital
P.O. Box 11288
Dallas, Texas 75223

National Conference on the Use of On-Line Computers in Psychology: November 20, 1974; Boston, Massachusetts

For information write to:

N. John Castellan, Jr.
Department of Psychology
Indiana University
Bloomington, Indiana 47401

Psychonomic Society: November 21–23, 1974; Boston, Massachusetts

For information write to:

Frederick A. Mote
Department of Psychology
University of Wisconsin
Madison, Wisconsin 53706

International Conventions

Eighteenth International Congress of Applied Psychology: July 28–August 2, 1974; Montreal

For information write to:

Secretariat of the 18th International Congress of Applied Psychology, Inc.
C. P. 242
Station Youville
Montreal, Quebec, Canada

First International Symposium on Non-Verbal Aspects and Techniques of Psychotherapy: July 29–31, 1974; Vancouver, British Columbia, Canada

For information write to:

Ferdinand Knobloch
Department of Psychiatry
University of British Columbia
Vancouver 8, B.C., Canada

Eighth Congress of the International Association for Child Psychiatry and Allied Professions: July 29–August 2, 1974; Philadelphia, Pennsylvania

For information write to:

Albert J. Solnit
Yale University
333 Cedar Street
New Haven, Connecticut 06510

Fifth International Conference on Humanistic Psychology and Second Indian Conference on Humanistic Psychology: August 1–4, 1974; Vishnakapatnam, India

For information write to:

Carmi Harari
285 Central Park West
New York, New York 10024

Second International Conference of the International Association for Cross-Cultural Psychology: August 6–10, 1974; Kingston, Canada

For information write to:

J. W. Berry
Psychology Department
Queen's University
Kingston, Ontario, Canada

Fourth International Conference on Social Science and Medicine: August 12–16, 1974; Elsinore, Denmark

For information write to:

P. J. M. McEwan
Centre for Social Research
University of Sussex
Falmer, Brighton
Sussex, BN1 9RF, England

Seventeenth International Convention of the Parapsychological Association: August 22–24, 1974; Jamaica, New York

For information write to:

J. G. Pratt
Division of Parapsychology
Box 152, University of Virgina Medical Center
Charlottesville, Virginia 22901

Thirty-Second Annual Conference of the International Council of Psychologists, Inc.: August 29–September 4, 1974; New Orleans, Louisiana

For information write to:

Frances A. Mullen
2901 King Drive, No. 1017
Chicago, Illinois 60616

Fifth International Congress of Social Psychiatry: September 1–7, 1974; Athens, Greece

For information write to:

George Vassilious
Athenian Institute of Anthropos
Ploutonos 10, P. Faleron
Athens, Greece

International Conference on Dimensions of Anxiety and Stress: September 2–5, 1974; Athens, Greece

For information write to:

Irwin G. Sarason
Department of Psychology NI-25
University of Washington
Seattle, Washington 98195

Symposium on the Latency Child—Development and Therapeutic Issues: September 27–28, 1974; Montreal, Quebec, Canada

For information write to:

Conferences and Special Events
McGill University
3587 University Street
Montreal, Quebec H3A 2B1, Canada

First International Congress on Obesity: October 9–11, 1974; London, England

For information write to:

Kim Solly
1st International Congress on Obesity
22 Montagu Street
London, W1H 2BR, England

Sixtieth Annual Convention of the International Association of Pupil Personnel Workers: October 27–31, 1974; Atlantic City, New Jersey

For information write to;

Joan E. Gaeng
Montclair State College
Upper Montclair, New Jersey 07043

Fourth International Congress of Psychosomatic Obstetrics and Gynecology: October 27–November 2, 1974; Tel Aviv, Israel

For information write to:

Organizing Committee, Congress of Psychosomatic Obstetrics and Gynecology
P.O. Box 16271
Tel Aviv, Israel

Fifteenth Interamerican Congress of Psychology: December 14–19, 1974; Bogotá, Colombia

For information write to:

Liuz F. S. Natalicio
Secretary General
Interamerican Society of Psychology
P.O. Box 88 UTEP
El Paso, Texas 79968

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29–January 1, 1975; Nairobi, Kenya

For information write to:

S. H. Irvine
College of Education
Brock University
St. Catharines, Ontario, Canada

International Conference on Psychological Stress and Adjustment in Time of War and Peace: January 6–10, 1975; Tel Aviv, Israel

For information write to:

Norman Milgram
Department of Psychology
Tel Aviv University
P.O. Box 16271
Tel Aviv, Israel

Tenth International Congress of Gerontology (and Geriatrics): June 22–27, 1975; Jerusalem, Israel

For information write to:

The Congress of Gerontology
P.O. Box 16271
Tel Aviv, Israel

Announcing the

1974 APA Membership Register

Contents:

■ Alphabetized listing

- 37,000 APA members (including the newly elected 1974 members)
- Updated mailing addresses
- Current membership status and divisional affiliations for 1974

■ Divisional membership lists,

including the 2 new divisions: The Psychology of Women *and* Population Psychology

■ Updated Rosters of the ABPP and ABPH diplomates

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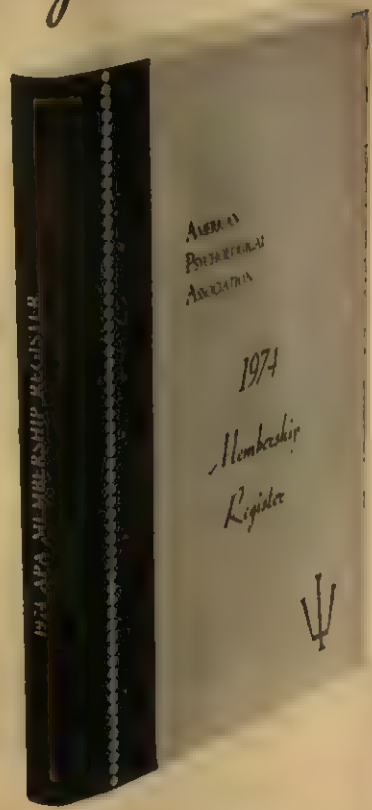

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The American Psychological Association, founded in 1892 and incorporated in 1925, is the major psychological organization in the United States. The purpose of the APA is to advance psychology as a science, and as a means of promoting human welfare. It attempts to accomplish these objectives by holding annual meetings, publishing psychological journals, and working toward improved standards for psychological training and service.

In addition to the *American Psychologist*, the Association publishes 13 other journals: *Contemporary Psychology, Developmental Psychology, Journal of Abnormal Psychology, Journal of Applied Psychology, Journal of Comparative and Physiological Psychology, Journal of Consulting and Clinical Psychology, Journal of Counseling Psychology, Journal of Educational Psychology, Journal of Experimental Psychology, Journal of Personality and Social Psychology, Professional Psychology, Psychological Bulletin*, and *Psychological Review*. The APA also publishes *Psychological Abstracts*, the *APA Monitor*, a monthly *Employment Bulletin*, the *Biographical Directory*, the *Convention Program*, and the *Catalog of Selected Documents in Psychology*.

And Still the Dryads Linger

ROBERT L. EBEL *Michigan State University*[1]

The title of this article was borrowed in part from that of another presidential address which was given more than 40 years ago. The speaker was Paul R. Heyl, a scientist and philosopher on the staff of the U.S. Bureau of Standards. The occasion was a meeting of the Philosophical Society of Washington, D.C., of which Heyl was president. He called his address "The Lingering Dryad" (Heyl, 1930). Here is some of what he said at the start of it:

> The dryad was a tree nymph. Every tree had its protecting spirit who was born with the tree, lived in or near it in intimate association, watching over its growth, and who died when the tree fell. The dryad was thus a personification of the life of the tree, and the connection between nymph and tree was far more intimate than was the case with the deities dominating sea or wind. Because of this peculiarly intimate relation the tree possessed life which the sea did not, though Neptune inhabited its depths, and which the wind did not, though set in motion by Aeolus.
>
> The men of old, it seems, drew very much the same distinction that we do when we speak of living and nonliving substances. Water, they observed, never grew old or died, but a tree was obviously a living thing, almost one of us, growing, reproducing its kind, and eventually dying. And as the ancients had difficulty in forming an idea of life without an animating personality there arose naturally the concept of the inseparable tree nymph.
>
> Human thinking from the first has been frankly anthropomorphic. Only in modern times has there been any notable effort to cast out anthropomorphism from our philosophy, and this struggle has not yet resulted in victory. Even we of today, with hereditary habits of thought heavy upon us, find the concept of impersonal, physical causes drab and unsatisfying, and we spell Nature with a capital N. The dryad lingers [pp. 205–206].

Thus spoke Paul Heyl in his presidential address 40 years ago. He went on to discuss the debate in biology between the mechanists and the vitalists, between those who put their faith in natural causes and those who believed that supernatural influences must have been involved in some way in the creation of life from nonliving matter, and later in giving some living forms the powers of reflective thought and the guidance of conscience. He traced the steady advance of the mechanists, the steady retreat of the vitalists. First to go were the ancient gods of the physical world, Aeolus, Neptune, Poseidon, and the rest, along with their junior partners, the fairies, elves, demons, and dryads. All of these are now consigned to the realms of mythology by most of us. Then, a century and a half ago, the vitalists lost an important battle. Friedrich Woehler heated the inorganic compound ammonium cyanate and transformed it into the organic compound urea. The supposed barrier between living and nonliving matter, between organic and inorganic chemistry, was breached.

Biochemists in our day have gone much farther. They seem to be approaching the goal of creating primitive particles of living matter out of nonliving materials. Success may elude them for a while, but few scientists doubt that the goal will be attained ultimately (Fox, 1960).

Dryads of the Mind

Thus in the physical world today there is less room for supernatural influences than there used to be. There are fewer homes for dryads. But in the world of the mind, some of them still seem to lurk in the woods and thickets. Of course they are not called dryads. Those who speak with scholarly care call them hypothetical constructs. Sometimes they are called traits (Allport, 1966). More often they are not regarded as members of any class. Those who accept them as real and important do not inquire too closely what stuff they may be made of. But they are cherished because they seem to explain why different people behave in different ways. To those who enjoy mystery, the fogs of vagueness that surround them do more to augment than to diminish respect for them. Mystery, they believe, is what one ought to expect when one is dealing with ultimate essences.

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[1] This article was the presidential address presented to Division 5 at the meeting of the American Psychological Association, Honolulu, Hawaii, September 1972.

Requests for reprints should be sent to Robert L. Ebel, College of Education, Michigan State University, East Lansing, Michigan 48823.

The history of these dryads is a long one. Originally they were thought to dwell not just in the brain but throughout the whole body. There were four humors—blood, phlegm, yellow bile, and black bile—that were held responsible for the behavioral manifestations of cheerfulness, impassivity, irritability, and melancholy, respectively. A person's behavior in a particular instance, as well as his general temperament, were attributed to the relative abundance or potency of these four humors in his body. There was, of course, nothing dryadic about these four substances. They were, with one possible exception, tangible, observable substances. Dryads enter the picture in the hypothetical influence of these substances on behavior and temperament.

Later on, as the mind took up residence principally in the brain, the dryads reappeared in the guise of mental faculties: attention, perception, memory, reasoning, imagination, will power, and the like. These faculties were regarded as general powers of the mind, gifts of a favored inheritance, or fruits of a disciplined education. Phrenologists made a pseudoprofession out of attempts to discover relationships between the shape of a person's skull and the degree of over- or underdevelopment of the various mental faculties.

The humors of our ancestors have been replaced to some extent by hormones, much more numerous and more limited and specific in their relation to body processes and behavior. But we no longer have much faith in the ancient humors, in skull contours, or in mental faculties as determiners of behavior. Those particular dryads are pretty well gone, so far as most of us are concerned. But others have appeared to take their place. They have gained acceptance in some of the best circles of behavioral science. To question them is to invite an argument, but I believe they need to be questioned. When they are used simply to describe behavior they are soundly meaningful, and I have no quarrel with them. But when they are used to explain the behavior, they take on all of the questionable characteristics of the dryads of long ago.

What is meant by "explain" in this context is to account for a specific instance in terms of a general law of the type: if–then always. Such laws relate a sufficient cause to its invariable effect (Reichenbach, 1951). The "sufficient causes" called into question here as dryads of the mind are traits originating as descriptions of behavior, but then coming to be accepted as explanations of the behavior.

Here are three prominent examples of contemporary mental dryads:

Intelligence, when the term is used to *explain* rapid learning or effective behavior.

Motivation, when the term is used to *explain* persistent efforts to succeed.

Creativity, when the term is used to *explain* production of an original work.

Am I saying that there is no meaning in words such as intelligence, or motivation, or creativity; that there are no important differences among people in how easily they learn or how quickly they understand, in how hard they work to reach a particular goal, in how successfully they innovate? No indeed! I am no more inclined to make such absurd claims than I would be to deny that trees live, winds blow, or storms trouble the seas. What I do say is that to attribute these differences in learning, effort, or innovation to differences in the intelligence, motivation, or creativity of the person involved is to go well beyond any credible evidence, to create the illusion of explanation in the absence of any real explanation, and to forestall or misdirect efforts to achieve real understanding. It is to invoke the help of dryads to explain away what we do not understand.

The only evidence we have that a person is more or less intelligent is that he behaves more or less intelligently. To say that he behaves intelligently *because* he possesses intelligence is completely circular (Cureton, 1951). The only evidence we have that a person is highly motivated is that he works hard to achieve a goal. To say that he works hard *because* he is highly motivated is completely circular. The only evidence we have of a person's creativity is the quality of his creations. To say that he creates effectively *because* of his creativity is completely circular. Careful psychologists are no doubt aware of this kind of circularity and do their best to avoid it. But when hypothetical constructs are used to explain observed behavior, it is very difficult to avoid. Not all psychologists manage to do it, and many in the public who read of their hypothetical speculations are quite unaware of the danger.

In none of these cases do we have any evidence for the existence or nature of the presumed cause apart from the effect it is supposed to produce. On this score the ancients who believed in the four

humors, or the more recent phrenologists, were a step ahead of us. They were probably wrong in thinking that anger was caused by an excess of yellow bile, melancholy by an excess of black bile, or curiosity by a particular bump on the head. But at least they had presumed causes that could be observed independently of the effects they produced. With intelligence, motivation, and creativity we do not. We have no more reason to believe in them than our ancestors had to believe in dryads. In all of these cases we are dealing with purely hypothetical causes of observable phenomena.

Intelligence, motivation, and creativity, when used not to describe but to explain behavior, are hypothetical constructs. There is nothing wrong with hypothetical constructs per se. Physical scientists have made good use of many of them. Universal gravitation is one. The luminiferous ether is another. The electron is a third. The gene is a fourth. What is wrong with intelligence, motivation, and creativity is that they are bad hypothetical constructs—ad hoc, imprecise, unverifiable. Their only function is to dispose of the question "Why?" without providing any real answer to it.

Good hypothetical constructs are precisely defined. Expected functional relations between them and observable variables are specified exactly so that they can be tested experimentally. In short, the hypothetical constructs useful to the scientist imply relations between different phenomena. They are not unique, ad hoc, "explanatory" hypotheses, as intelligence, motivation, and creativity appear to be.

Intelligence as an Explanatory Construct

Consider intelligence as an explanation of rapid learning or effective behavior (McNemar, 1964). What is it? Presumably it might be a consequence of some special anatomical or physiological quality of the brain or nervous system. Since human beings differ anatomically and physiologically in other respects, it is reasonable to guess they differ in neural qualities also. But no such differences have been discovered among normal human beings. What is more serious, the concept of intelligence as a cause is not even related to any hypotheses about neuroanatomy or neurophysiology by most people who use the term. It is simply a description of behavior that many accept as an explanation of that behavior.

A number of other dryads of the mind are associated with intelligence as explanations of behavior. There are higher mental processes, cognitive skills, and cognitive strategies among others. It may be worthwhile to consider each of these briefly.

Part of the difficulty with the term *higher mental processes* is in seeing the difference between higher and lower mental processes. Is recall a lower mental process? When I leave the office and try to recall where my car was parked the last time I left it, in Lot D, H, M, or P, and where in the lot, it seems that I am using about all the mental processes, high and low, that I possess. Further, because we do not really know what a mental process is, may it not be premature to try to differentiate higher and lower mental processes?

To Charles H. Judd (1936) and some of his associates and successors, particularly at the University of Chicago, the essence of education was cultivation of the higher mental processes. Gagné (1968) emphasized the development of cognitive skills and cognitive strategies. In both cases there is an effort to separate the process of thinking from the content of the thoughts and to give priority to the process over the content in specifying the objectives of education. It seems to me that such separation is impossible (Ebel, 1972a, 1972b). As I consider what goes on in schools and colleges, I find few instances in which processes of thinking are taught separately from the content to be thought about. Reviewing my own education, it seems that I always learned process and content together: in reading, arithmetic, and grammar; in Latin, geometry, and physics; in measurement, statistics, and philosophy. That is why in my own teaching I do not try mainly to develop abstract cognitive skills in my students, or to cultivate abstract higher mental processes. Instead I try to help them build solid, comprehensive structures of knowledge. Of course, these structures include knowing how to do, as well as knowing what is so. But the emphasis is squarely on knowing.

A reader who thinks critically as he reads may inquire at this point whether "structure of knowledge" is not as much of a dryad as "higher mental processes," "cognitive skills," or "cognitive strategies." Is it not like them a hypothetical construct? Indeed it is, but it differs significantly from the others in this important respect: It is more clearly and specifically definable than they usually have been. Further, and partly as a consequence of this,

[illegible]

[illegible]

[illegible]

[illegible] and [illegible] goal is [illegible] to most of the rest of us. We are [illegible] that [illegible] creativity and the [illegible] and opportunity too often and [illegible] development accounts for the [illegible] these were achieved. While it [illegible] such a contention I for one see no good reason for believing it.

If there is any identifiable quality that is present only in the [illegible] of creative [illegible] [illegible] [illegible] and [illegible] almost in those less creative [illegible] it has not been identified. Nor does it need to be [illegible]. While it is unlikely that the whole [illegible] career development can ever be [illegible] who reads the life story of a [illegible] can find many reasons why [illegible] to do what they did and why they succeeded so well in doing it.

The [illegible] that creative potential in usable [illegible] is limited to a favored few among many [illegible] are quite untenable. Every sentence I [illegible] as I compose this article is an original creation. It would be the same if I were writing a [illegible] or contributing to a conversation [illegible] for retirement or packing for a vacation. And it is the same for all of my fellow [illegible]. The problem is not that of finding unique [illegible] to be. That problem is that what we [illegible] little favor in the eyes of the world [illegible] value it enough to mention it. [illegible] it is not worth mentioning. It is on that [illegible] Shakespeare, Einstein, and the rest have [illegible].

[illegible] for more than originality is the basis [illegible] can be built. Hence the emphasis [illegible] of creativity just on uniqueness of [illegible] on [illegible] questions to any two [illegible] means questionnaire [illegible] Should we take [illegible] Should we [illegible] members access to [illegible]?

The alleged incompatibility of creativity and [illegible] has been over-advertised. It finds some [illegible] in the alleged difference between divergent and convergent thought processes [illegible]. It finds some apparent support [illegible] the low correlations reported between [illegible] and creativity test scores. If the [illegible] were more reliable the correlations would [illegible] be higher. But the two kinds of tests [illegible] different tasks which [illegible] require somewhat different [illegible]. [illegible] that [illegible] a person from being creative [illegible].

Other Deeds of the Mind

Intelligence, [illegible] and creativity are [illegible] three examples of deeds of the mind. There are many others. In the cognitive domain there seem to be a [illegible] of any [illegible] that [illegible] to be [illegible]. There are [illegible] and to analyze, to interpret and to [illegible] recognize [illegible] and to [illegible] them [illegible]. Though each [illegible] a [illegible] seems to have a [illegible]. No matter how [illegible] the [illegible] may be [illegible] who has the [illegible] [illegible] that [illegible] that [illegible]. [illegible] arrangement [illegible] deeds.

In the [illegible] of [illegible] there are more [illegible] of deeds. [illegible] [illegible] [illegible] and [illegible] [illegible] [illegible] such as [illegible]. When [illegible] [illegible] [illegible] [illegible] of the [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible].

[illegible] of [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] and

others better, utilize our human resources more effectively, or if not these, at least accept more philosophically our limitations and peculiarities and those of our fellowmen. But human behavior is also a complex phenomenon, something that is likely to be difficult to understand. We need to recognize that words describing behavior, words like *intelligent*, or *motivated*, or *creative*, cannot be transformed by the wave of a linguistic wand into explanations of that behavior. We need to recognize that explanation of behavior requires more than suggestion of a hypothetical cause for it. The hypothetical cause must be verified as a real cause. It must be shown to have construct validity. This means, as a minimum, that it is a measurable quantitative variable that is functionally related to some variable or variables other than the behavior variable it presumably explains.

The Validation of Physical and Behavioral Constructs

Validated constructs are abundant in the physical and biological sciences. Few dryads linger there. They have been replaced by less purely hypothetical, more completely validated constructs. The dryads of phlogiston and the philosopher's stone, of caloric and the ether, of spontaneous generation and the inheritance of acquired characteristics have been replaced by the validated constructs of oxidation and nuclear reactions, of molecular motion and empty space, of reproduction and mutation.

In the behavioral sciences, on the other hand, dryads like those mentioned earlier in this article, seem to linger in larger numbers and greater security. Why is this true? Is it because the constructs that might replace dryads are more difficult to validate in the behavioral sciences than in the physical sciences? It seems clear that the number of precisely defined, readily quantifiable constructs is much smaller in the domain of behavior than it is in natural science. This difficulty may be due, in turn, to the greater complexity of behavioral phenomena. Contrast the very large number of precise functional relations among the physical constructs with the very small number of such relations among behavioral constructs. On the border between physiology and psychology, in the field of sensory psychology, a number of precise, quantitative relations have been discovered. But as one moves farther from biology toward behavior, considering problems of learning and thinking, of motivation and adjustment, of personality and social behavior precise quantitative relations become fewer and fewer. Look in a compilation of the laws of human behavior as provided in the book by Berelson and Steiner (1964). While much useful data from specific studies are reported, the "laws" themselves are disappointing. Few are precise statements of exact quantitative relations.

To validate an explanatory construct one must show that it is functionally related to some behavioral variable rather than the one it was invoked to explain. This means that it must be defined clearly enough to allow it to be measured with a high degree of reliability. It also must be defined in sufficiently basic terms so that the construct can be expected to have some general utility. Behavioral scientists do not need to be reminded that few if any explanatory constructs have been defined so as to meet both of these requirements.

Inadequately defined constructs typically yield very imprecise functional relations. Few, if any, of them can be expressed in mathematical formulas. Many are expressed as correlation coefficients, usually far from unity. Sometimes a "functional relationship" is considered to have been established on no better basis than that the correlation between the two variables cannot be attributed wholly to chance.

Low correlations between functionally related variables may be due to imprecise measurements of the variables. They may also be due to the influence on the dependent variable of many other independent variables not measured or even considered in studying the relationship. Probably both factors are responsible for the low correlations observed in most studies of functionally related behavior variables.

Can these defects be remedied? Do they exist mainly because the behavioral sciences are "younger" than the physical and biological sciences? Perhaps. On the other hand, behavioral science may be less well developed than astronomy, physics, chemistry, or biology because it presents harder problems to solve, not because it was neglected so long. From the record of history it appears that man has been concerned about problems of human behavior just as long as, and probably more seriously than, he has been concerned about problems in his natural environment. A more plausible explanation may be that the phenomena studied by the behavioral scientist involve many more variables that are much more com-

plexly related. Perhaps it is not just a matter of time until we have as impressive an array of clearly defined and broadly useful variables and precisely powerful functional relationships in the behavioral sciences as we have in the physical sciences. That day may never come (Bakan, 1972).

The most precise and useful scientific laws deal with relationships in which the number of variables involved is relatively small and the relations among them relatively simple (e.g., $e = mc^2$, $I = E/R$, $s = \frac{1}{2}at^2$, $F = ma$). These situations are quite common in basic physical phenomena. They become less common as the situations to which they are expected to apply become more complex. The variables that affect the functioning of a living cell are infinitely more numerous and more complexly related than those that affect the swings of a pendulum or the orbital motions of a planet. There are physical variables of temperature, pressure, motion, electric potentials, and electromagnetic radiations which affect the cell. There are chemical variables in the protoplasm of the cell and its inclusions, in the hormones, nutrients, and metabolic wastes in the fluids surrounding the cell. There are structural variables in the cell wall, cell body, and the nucleus; in the chromosomes, centrioles, nucleolus, Golgi bodies, and many other structural features. There are genetic variables in the arrangement of bases that carry the genetic code in nucleic acid. Is it reasonable to believe that the functioning of a cell ought to be as easy to understand and predict as the functioning of a pendulum? Since the variables that affect the behavior of a person are infinitely more numerous and complex than those that affect the functioning of a living cell, it is not difficult to see why scientific understanding of human behavior comes hard and slowly (Van Den Haag, 1959).

Why is it that the occurrence of an eclipse can be predicted 10 years hence, or 100 years hence, with much greater certainty and precision than tomorrow's weather? It is because the variables involved in the eclipse are much fewer and their functional relations much simpler. In principle, tomorrow's weather is as predictable as a twenty-first-century eclipse. It is fully determined by atmospheric circulation, which in turn depends in general on heat exchange and the rotation of the earth, and in particular on surface features of the earth.

In practice, as everyone knows, weather predictions are fraught with uncertainty. The most plausible explanation for the uncertainty is that there are just too many variables involved, and their functional relations are too complex, too nonlinear (Kovach, 1960), too sensitive to minute changes. Given better data on the status of the earth's atmosphere today and more complete and accurate knowledge of the physics of the atmosphere and the influence of the local terrain, better weather forecasts could be made. But modest increases in the accuracy of the forecasts cost increasingly large amounts of money and effort. There comes a point when the increase in accuracy may not be worth what it is likely to cost.

What seems to be true of laws dealing with weather phenomena may also be true of the laws of human behavior. They may involve so many variables in such complex relationships as to be very difficult to discover. They may have such limited ranges of application and be so ephemerally valid as to be hardly worth the effort of discovery. For human beings learn, and with this learning comes changes in their behavior. What is true of the individual is also true of the race. Conditions of life change and bring with them changes in needs, values, and patterns of behavior. In these circumstances it is not easy to discover laws of behavior that will have enduring general validity.

Hence, we cannot expect that the valid constructs of behavioral science will dispose of dryads of the mind as quickly and decisively as those of natural science have disposed of its dryads. But this need not keep us from recognizing our dryads for what they are: partial descriptions that masquerade as causal explanations. This need not keep us from understanding how useless they are in our search for understanding of behavioral phenomena. Let us be on guard against their deceptive pretentions. Let us make behavioral science, limited and imperfect as it is, inhospitable to them. They can only weaken it. Let them linger, if they must, outside of our houses of intellect.

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How Do You Know?

FRED ATTNEAVE *University of Oregon*[1]

Epistemology is traditionally classified as one of the fields of philosophy, and some psychologists have been content to leave it to the philosophers. In my opinion, the problem of how we know is an absolutely basic concern of psychology, but whether you consider my remarks to be psychology or philosophy I really do not care, as long as you are willing to grant me that the problem is a fundamental one.

Naively, it seems to us that the outside world, the world around us, is a given; it is just there. I look out and see you sitting in front of me; around you I see walls that enclose the room and stop me from seeing farther. But my world, the world I live in, does not seem to stop at the walls; beyond them, in the same continuous space, there are cities, roads, rivers, oceans, all of which have some determinate loci in my picture of the world. We all feel as if our experiencing of the world around us were quite direct. However, the apparent immediacy of this experience *has* to be more or less illusory because we know that every bit of our information about external things is coming in through our sense organs, or has come in through our sense organs at some time in the past. All of it, to the best of our knowledge, is mediated by receptor activity and is relayed to the brain in the form of Morse code signals, as it were, so that what we experience as the "real world," and locate outside ourselves, cannot possibly be anything better than a *representation* of the external world. (Epistemologists can argue about whether it is even that, but I am willing to take for granted the existence of a physical world that is being represented.) The afferent nerve impulses that link the representation to the reality are extraordinarily dissimilar to either, however formally considered. This is the point of Brunswik's (1952) *lens analogy:* information about the world is represented in a very diffuse way at the receptor level, but is brought to a "focus" within the nervous system where objects and events are presumably represented in a form more "like" the outside world than they are at the receptor surface.

The statement that the world as we know it is a representation is, I think, a truism—there is really no way it can be wrong. It has some fairly interesting corollaries, however, that you may or may not have contemplated. One of them is that I have got to be at least as complex as the world as I know it, and psychologists who are attracted by simplistic theories might do well to ask themselves whether they are taking this fact seriously.

We can say in the first place, then, that knowing necessarily involves representation. Now what does "representation" mean? To say that one system represents another, we must at least mean that certain parts and relations between parts of one system must, via some sort of transformation, *correspond to* certain parts and relations between parts of the other system. This is not necessarily to say, however, that the correspondence has to be part for part and relation for relation. Another important possibility, which I shall discuss presently, is that what is a relation in one system may be a part, or an element, in the other. It just makes for a more complicated transformation if that is true.

Let us consider two representational systems that we know something about, and that exist objectively, outside the head where we can examine them. The first is spoken or written language. Think of the *Encyclopedia Britannica*. An encyclopedia *represents* considerable portions of the world. Note that in the case of language, the English language in which the *Encyclopedia* is written, the relations in the world being described are not preserved as relations on the printed page. Relations in the world are represented almost entirely by *things*, that is, *words*, in the language system. The word *chair* and the word *inside* are both simply elements of language, and if you do not know English, you do not know that one is a noun

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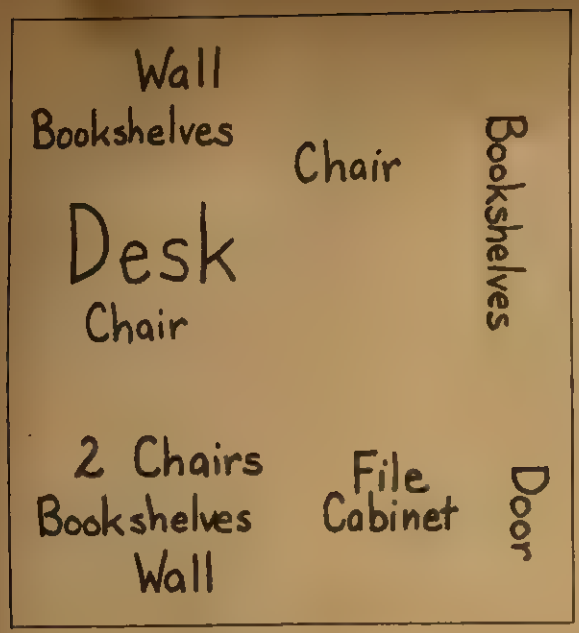

Figure 1. A hybrid (digital–analogue) representation of an office.

and the other a preposition. This substitution of things for relations is one of the most fundamental of all linguistic devices. The English language and, in fact, all other natural languages employ it quite effectively in describing large portions of what we call reality.

Another kind of representational system that we are all familiar with is the *map*. A map is quite different from language in that it is an analogue representation, whereas language is a digital representation. What this means, at least for our present purposes, is that relations among the items being mapped, items like cities, lakes, rivers, and so on, are represented by relations on the map, whereas they are not in language. In fact, in the special case of the map, space is represented by space; within certain well-understood limits, the map is just a minification of the terrain that is being mapped. It is not, of course, a completely literal minification. On the map, towns are usually represented by black circles, though they do not look like black circles from an airplane; on this microlevel the analogy is not maintained.

Digital systems of representation and analogue systems may coexist in many combinations. The *Encyclopedia Britannica contains* pictures and maps. Likewise, a map contains words to tell the names of towns, states, lakes, and rivers. Furthermore, we can have all sorts of hybrid systems, as in Figure 1, in which gross spatial relationships *between* objects are shown in an analogue manner—a maplike manner—but spatial relationships *within* objects are not shown. The objects are merely symbolized by arbitrary words.

Now, I think nobody would particularly object to my saying that both the map and the encyclopedia contain knowledge. Do we really want to say, though, that the *Encyclopedia Britannica knows* the various facts that it contains? I suspect you would be uncomfortable with that proposition, and, just to exacerbate your discomfort a little, let me ask you to imagine a person who knows nothing about the English language, let us say a Chinese, who is nevertheless gifted with such uncommon eidetic imagery that he can look through the encyclopedia and retain a precise visual image of every page, which he can call up at will and, given enough time, even reproduce. Are we willing to say that he *knows* what is in the *Encyclopedia Britannica?* Again, this does not seem right, and the reason it does not is that "knowing" at least connotes some kind of utility, some kind of consequences for behavior. Let us sidestep the discussion of "meaning" that suggests itself at this point and merely ask, very explicitly: Why is representing the world a good thing biologically—that is, for survival? It is not too hard to give a commonsense answer to this question, and I think the commonsense answer is quite right. If it is not obvious that all knowledge is important for survival, it should be at least moderately obvious that *knowing how* is biologically important. It appears to me that *knowing how* is indeed both the beginning and the end of knowledge.

The basis for *knowing how* can be represented in a somewhat oversimplified way as an SRS linkage, like this:

$$S_1, R \rightarrow S_2 .$$

If situation S_1 obtains at a given time, and I do R, then situation S_2 results. If I *know* this, I know how to change situation S_1 into situation S_2. The beginning of knowledge, I think, is to be found in the fact that we live in a lawful world, in which propositions of this SRS type have some continuing validity from one day to the next. If we did not live in a world in which lawful relationships of this form existed, learning would simply not be worthwhile to the organism at all—nor, for that matter, would innate reflexes or tropisms have any value. It is only by doing things that change one situation into a more favorable situation that the organism

can possibly affect his chances of survival by his own behavior. Likewise, the end of knowledge, that is, the utility of knowledge, lies in knowing how to change things for the better and to avoid changing them for the worse. This SRS notation is shorthand for something that is a lot more complex, of course. If we wanted to get a little more fancy in one fairly obvious way, we should formulate this in terms of a conditional probability matrix, because S_2 may not always follow S_1 and R; it may merely be a good bet; also, S_2 may happen regardless of R, and so on. Without pursuing this probabilistic aspect, let me just point out that the organism will be better off to the degree that he can slice up the world into chunks such that situations described in terms of those chunks yield more determinate SRS relationships than if the world were partitioned otherwise. In other words, good organization in the representational system is organization that makes for relatively determinate SRS linkages; poor organization is that which makes for more indeterminate linkages.

Although I am proposing that *knowing how* is the beginning and end of knowledge, I am not proposing that it is necessarily the middle. The middle may take quite a different form. In fact, there may be considerable advantage in depersonalizing the R portion of the SRS linkage. To take one such step: The R may be, not what *I* do, but what *one* does, what some generalized person might do in that situation. Thus, if I see another person perform the act with a determinate result, I may imitate him; I may say that if he can do it, I can. There are further steps of depersonalization in which the representation can simply take the form of causal linkages between events. If I understand how the world works, if I effectively represent what Tolman and Brunswik (1935) called "the causal texture of the environment," then the opportunity exists for me to intervene in the causal network and make things come out my way.

I think the utility of knowledge has to be explained in terms such as these. But let me add that we have many items of knowledge, the utility of which we would be hard put to demonstrate individually. Accumulating knowledge, at least at the human level, is a lot like saving pieces of string, for which the classical justification is that "It may come in handy some day." So it is with knowledge.

My argument, then, is not that representation is *reducible* to SRS linkages but merely that it must embody information from which SRS linkages can be derived conveniently when the occasion demands. Phenomenally, the set of representations we are talking about is completely identifiable with "the external world as we know it." But the scientific problem is to characterize this system in more objective terms, looking at it from the outside, that is, *as* a system. How can we *represent* the representational system?

There is one thing that the system is certainly *not:* It is not just a lot of stimulus objects (or surrogates thereof) tied together by simple, uniform associative bonds. Objects in the world as we know it are indeed associated, but they are associated *by particular relations*, and relations have to be differentiated one from another. Relations have quite as much fine structure as objects and classes of objects have. The situation that is described by the sentence "John gave the ball to Mary" cannot be represented by undifferentiated associative linkages between John, Mary, and ball. The particular act of giving, of exchange of possession, is involved here, and it takes a particular direction—the ball passes *from* John *to* Mary. The relations that we wish to represent vary tremendously one from another. For example, the relationship that is described by the word *give* is of a completely different *family* from the relationship that is described by the words *is a*—as in "John is a boy," or "John is a dog," as the case might be. (If I say "John is a dog," that is essentially a statement of how I can transfer knowledge. Whatever I know about dogs, I can apply to John.) If only one kind of relationship between the elements of a representational system is possible—that is, a simple associative bond—the only way such a system can be made to represent the numerous and diverse relations it has to cope with is by transforming these relations into *things*, or elements—just as they are transformed into words in natural language—in which case the associative bond will have a status comparable to that of serial order in language.

Now, should we take one further step and suppose that language *is* the representational system, for man? This position has been favored by a good many behaviorists in the past, who have supposed that anything they could comfortably call knowledge does not come into being until the human level, when language becomes available to subserve that knowledge.

It is not difficult to refute this hypothesis, at least in its most naive form. For example, Bartlett (1932) showed some years ago that memory for stories is not in terms of the words that make up the stories, but in terms of the ideas. Words, in other words, are attached to representational structures that are more fundamental than language. These structures can be given various names. We can call them concepts; we can call them logogens, as Morton (1969) did: In common parlance, they are the *ideas* to which the words are attached. We are all clearly aware that the word *tree* is not the same as a real tree—even Premack's (1970) chimpanzee Sarah knows this difference. But now let me remind you, once again, that what you know as a real tree is not the real tree out there at all, but your representation of a tree, so there has to be some level of representation that is not verbal. Furthermore, I do not believe for a moment that concepts come into being only when language is acquired. It is much more plausible to suppose that subhuman animals—certainly the higher mammals—have concepts and concept structures that are not ridiculously different from ours, and that when these concept structures reach a certain level of evolution, they are ready to have language grafted onto them. I think a comparative psychology of semantics is very much a possibility and that something of the sort is coming into being at this time, particularly with the work of people like Gardner and Gardner (1971) and Premack (1970), who are teaching language to chimpanzees. It seems moderately clear from their work that lower animals do have concepts just sitting there waiting for words to be attached to them. Premack emphasized this point and gave some evidence for it. The Gardners found that their chimpanzee Washoe would say "dog" either to a real dog or to a picture of a dog, or to the sound of a dog barking. Herrnstein and Loveland (1964) got similar results, even with pigeons. They found that pigeons would give a particular response to human figures generally and would even give the same response to parts of human beings, such as hands. So it seems by no means unreasonable to suppose that lower animals, in many cases, may have already evolved languages of their own—languages peculiar to their own nervous systems. Covert languages, or languagelike processes, may be entirely possible in the absence of language responses. Let me suggest, then, that some of the structures that we find in natural languages may be a guide, albeit a feeble one, to the nature of languagelike processes at the logogen or concept level. One can certainly imagine that there are unitary events in the nervous system that correspond to objects, or classes of objects; that there is a "dog" neuron somewhere and a "table" neuron somewhere else. These would be essentially what Konorski (1967) suggested as *gnostic units*. But there may likewise be unitary processes, even in the case of the lower animals, that are the precursors of relational words like *inside, before, give,* and so on. Relations, like objects, may be represented in digital terms as in fact they are in language.

There are many psychological functions, however, that give every indication of entailing *analogue* representation, that is, representation in which relations are represented by relations. To take an example that is by no means the most obvious: People seem to represent numbers in an analogue fashion. Moyer and Landauer (1967) found that if a subject was asked to tell which of two numbers was greater, his reaction time was shorter the greater the difference between the two numbers. This is what you would expect from an analogue representation, but it is exactly the opposite of what you would expect if, for example, the subject were counting from one number to the other. More generally, I think that any psychophysical continuum to which metric considerations apply, that is, any continuum that is potentially scalable, is essentially an analogue system, or subsystem. The very fact that a subject is able to squeeze a hand dynamometer as hard as a light is bright (Stevens, Mack, & Stevens, 1960) is exceedingly difficult to understand at all, except in terms of analogue processes. Some people have tried to conceive psychophysical continua as the axes of a huge multidimensional space in which objects may be represented as points. I think that in general this is the wrong approach. What is more typically the case is that the psychological continuum functions as a medium for the representation of relations—a medium *in which* relations occur (see Attneave & Olson, 1971).

Perhaps the most important case of all in which analogue representation seems to be necessary is that of the system for representing physical space—the tridimensional space around us. If anything seems immediate to us, in our perception of the world, it is this space in which objects are *located* and in which we move about. The three dimen-

sions of physical space are related to one another in quite a unique way. They are *isotropic:* the same metric applies to all three. You can use a yardstick to measure the height of something, or the width, or to measure in any oblique direction you like. Moreover, a yardstick *looks* just about a yard long regardless of its distance away and the angle at which it is viewed. Physical entities are all represented as having determinate locations in this space, and this is true of imagined space as well as of perceived space. The animal *knows where* the water hole is.

Now, it is true that we have linguistic terms for spatial relationships: *above, behind, in front of, inside, outside,* and so on. It nevertheless seems exceedingly likely to me that these words refer to relations in an analogue model of space. This assumption of a tridimensional analogue model of physical space has some important theoretical advantages. For example, the Gestalt psychologists (e.g., Koffka, 1935) and Ernst Mach (1886/1959) before them argued that in perceiving objects we represent them in the simplest way that is consistent with the constraints of the input, the stimulation. This is the Gestalt principle of *Prägnanz,* and there is a great deal to recommend it. Several years ago, Robert Frost and I (Attneave, 1972; Attneave & Frost, 1969) did some experiments, and the results were quite consistent with the *Prägnanz* account of space perception. Thus, we were led to ask: What does one have to assume about the nature of a system that is capable of *finding* a simplest representation? It appeared to us that the minimum assumptions involved some kind of analogue tridimensional model in which objects *could* be represented either more or less simply and in which more complicated representations could be smoothly transformed into simple ones (i.e., by continuous, as opposed to discontinuous, processes) under the guidance of some feedback signal from a primarily digital descriptive system.

An arrangement of this sort is suggested in Figure 2. On the left, we have a tridimensional modeling medium, in which any representation consistent with the stimulus constraints *might* be constructed. This representation is *described* in the center box in Figure 2, and if it changes from one moment to the next, any resulting change in the complexity of the description is then fed back as a hot–cold signal into the tridimensional system, thereby guiding it into a *simplest* representation as

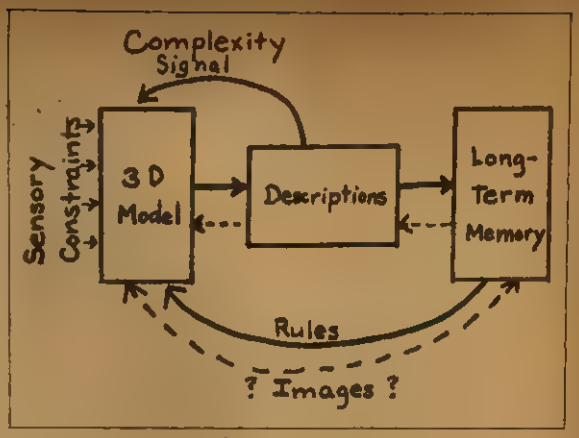

Figure 2. Outline of a system for achieving economical representations.

a stable state. This is essentially a hill-climbing machine, and the analogue medium provides a smooth terrain, so to speak, for the hill climber to operate on.

An analogue stage like this also makes a great deal of sense in terms of the identification or categorizing of objects, because the descriptive machinery is taking its descriptions, or its defining features of objects, not from the flat picture on the retina but from a model of the tridimensional world. It is describing solid objects rather than plane projections of the objects.

Evidence for the analogue modeling of physical space comes from quite diverse sources. A few years ago I became intrigued with an old study by H. H. Corbin (1942) on apparent movement, in which he found that the time interval between two flashing lights necessary to produce apparent movement varies with the distance between the lights (as Korte's third law of apparent movement says it should) but that the distance in question is not the retinal distance, not the visual angle between the lights, so much as their *physical* separation. He dissociated the two by presenting the lights on a slanting board. Gene Block and I (Attneave & Block, 1972) have recently done a series of similar experiments in which we have fairly thoroughly verified this finding. In fact, we found that the time interval necessary for apparent movement varies with the phenomenal or perceived distance between the light stimuli even when this distance is varied by purely illusory means, that is, by placing the lights at different apparent depths in a picture. What this shows, or what we believe it shows, is that apparent movement must be occurring in a tridimensional representational medium in

which the isotropy of physical dimensions is fairly well preserved.[2]

However, the most compelling evidence I know for the existence of a tridimensional modeling medium in the head comes from the work of Roger Shepard and his students. Shepard and Metzler (1971), in a study that many of you know, presented the subject with two pictures of tridimensional objects and asked: "Can one of these be rotated into the other? Are they the same except for orientation?" In the case of positive responses, that is, when one could be rotated into the other, the reaction time turned out to be a very precise linear function of the angular difference between the objects. Furthermore, it made no difference whether the rotation was in the frontal plane or in depth: The reaction time function was almost identical for the two types of rotation. This and other studies from Shepard's laboratory, which I do not have time to review, show beyond any reasonable doubt that when one rotates a mental image from one aspect to another, the representation of the object is in fact going through all of the intermediate aspects in a continuous manner. I have no idea how anybody could possibly account for these results without postulating an analogue representational medium.

This brings up the business of imagination and images, and I would like to suggest that this tridimensional modeling medium can be used not merely to represent the current input but also to represent images that are taken from memory, that is, that imaginary scenes can be reconstructed in space and that the organism can then proceed to use this as a work space in which he tries out things and sees what happens. He can engage in vicarious manipulation; he can engage in vicarious locomotion. He can try out the results of particular forms of behavior before he commits himself to them in practice. This highly developed facility for handling spatial information may be used in various ways. Consider, for example, the popularity of graphs, in psychology and other sciences, in which nonspatial continua are mapped onto spatial coordinates in order to make functional relationships more easily apprehended.

Now, if images in this space can be generated from memory, the question immediately arises, In what form do they exist in memory? We might suppose, of course, that they are *stored* in an imagelike, or picturelike, form. Alternatively, we could suppose that they are stored as languagelike descriptions, which are reversible in the sense that images can be reconstructed from them. Pribram (1971) has a third alternative, or perhaps an in-between alternative: He believes that images are stored in the form of neural holograms of some sort. This is a very real possibility, but I am more inclined to favor the notion of reversible descriptions. In a remarkable article published a few years ago, the Dutch psychologist Leeuwenberg (1971) developed a powerful descriptive language in terms of which highly complex tridimensional visual forms can be represented. These descriptions are about as economical as they can be; that is, they take into account virtually all of the internal regularities of the forms being described to eliminate redundancy. Moreover, they are reversible: They can be used as instructions for rebuilding the form. Leeuwenberg believes, as I do, in the necessity of a tridimensional analogue representational medium from which such descriptions can be taken and into which images can be "projected" from descriptions.

In any case, if we suppose the existence of languagelike representational structures, the elements of which have a wordlike status, whether in memory or in consciousness, we need to consider how and where these structures get their *meaning*. How does the nervous system understand its own language? One place to look for the meaning would certainly seem to be in the imagery that the description can generate. In other words, what has the function of a *logogen*, looked at one way, may also have the function of an *iconogen*, looked at the other. But there is another aspect of meaning that I think is even more essential. I can imagine going for a walk and encountering a dog, or a bird, or a wildcat in the woods, but how the scenario progresses beyond that point is highly dependent on *which one* of the three I imagine meeting. The rules of the game are by no means the same for dogs, wildcats, and birds; the SRS linkages that are plausible are extremely different

[2] I have just learned that the Japanese psychologist Jiei Ogasawara (1936) did a study of this type that antedated Corbin's by several years. His results were quite consistent with those mentioned in the text: The apparent movement threshold was highly dependent on apparent separation when viewing distance was varied with retinal separation held constant. I am grateful to Tadasu Oyama for bringing Ogasawara's article to my attention and for providing me with a copy and an English summary of it.

in the three cases. The real point or utility of identifying anything is in order to access the rules by which it interacts with other things and, more particularly, with us. We may be able to suppose that the rules of geometry, and perhaps even some very simple principles of causation, are embedded in the spatial representational medium, but a great many other rules are certainly dependent on *what* it is that is represented in that medium.

However, these two aspects of meaning that I have just suggested—that is, connection with imagery on the one hand, and access to rules on the other—are not as distinctly different as they may seem at first. I am thinking particularly of deGroot's [8] work with chess masters who are able to look at some chess position from a real game for five seconds and afterwards reconstruct it perfectly. It becomes quite evident from their reports that they are not just assigning particular pieces to particular squares on the board, but rather remembering *functional relationships* between pieces—what piece is threatening another, what piece is guarding another piece against a threat, and so on. In other words, the rules of the game turn out to be quite essential to the way the position is remembered and reconstructed.

It should be obvious at this point that I do not have a beautiful, elegant, comprehensive answer to the question that I posed in the title of this presentation. But let me leave you with this suggestion. Let me ask you to imagine—use your spatial representational system to imagine—a 2×2 table. On one axis put "digital, languagelike processes" versus "analogue, maplike processes," and on the other axis put "consciousness" versus "memory." Now, if we can ever figure out what belongs in each of these four cells, and how each of the four is related to the other three, then I think we shall possibly know what knowing is.

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[8] deGroot, A. D. "Perception and Memory in Chess: An Experimental Study of the Heuristics of the Professional Eye." In preparation, 1974.

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Toward a Convergence in Hypnosis Research

NICHOLAS P. SPANOS *Medfield State Hospital, Medfield, Massachusetts*
THEODORE X. BARBER *Medfield Foundation, Medfield, Massachusetts* [1]

Within the last two decades, methodological and theoretical controversies in the area of hypnosis research have gradually consolidated into two alternative paradigms (Chaves, 1968; Spanos, 1970; Spanos & Chaves, 1969, 1970; Tellegen, 1970). The older paradigm, which we shall call the *state* viewpoint, assumes that the hypothetical construct *hypnotic state* or *trance state* is a useful conceptual tool for organizing the available data and for generating fruitful research in this area. The alternative paradigm, which we shall call the *nonstate* viewpoint, rejects this assumption and argues that the data can be more parsimoniously conceptualized in terms of constructs that are already an integral part of contemporary social psychology.

The research generated by each of these alternative paradigms has spawned a number of useful developments. A wealth of new information has been compiled concerning the antecedents and correlates of response to hypnotic suggestions (Fromm & Shor, 1972; Gordon, 1967; Moss, 1965).[2] Also, new methods have been developed for studying the hypnotic subject's experiences, and numerous theoretical issues have been recast in operational terms and, thereby, have been brought into sharper focus (Coe, 1973).

Inevitably, the proponents of a particular scientific paradigm tend to stress the differences rather than the similarities between their conceptualization and the alternative point of view. The tendency of investigators to maximize such differences is understandable. Furthermore, it appears to be useful. It forces the investigator to search for and continually examine implicit assumptions underlying his own paradigm and the rival paradigm that might otherwise remain unrecognized. Unfortunately, the tendency to stress differences rather than similarities between paradigms may also engender a number of negative consequences. First, it may lead investigators into self-righteous polemics which are of little value in advancing knowledge. Second, and more important, it may obscure significant areas of agreement between proponents of alternative paradigms and thereby foster a distorted view of the research area as a whole.

We believe that the theoretical controversies in this area have overshadowed a significant degree of agreement concerning the factors that mediate response to hypnotic suggestions. More specifically, we believe that major theoreticians in the area of hypnosis, regardless of paradigm affiliation, are converging on the conclusion that responding to suggestions involves at least two interrelated factors. The first can be conceptualized as a willingness on the part of the subject to cooperate with the experimenter in fulfilling the aims of the suggestions. The second can be described as a shift in cognitive orientation from an objective or pragmatic perspective to one of involvement in suggestion-related imaginings. The construct *involvement in suggestion-related imaginings* encompasses at least two simultaneously occurring cogni-

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Requests for reprints should be sent to T. X. Barber, Medfield Foundation, Medfield, Massachusetts 02052.

[2] Although hypnotic behavior is multifaceted (Barber & Calverley, 1969b), its most important and most thoroughly studied component consists of responses to "hypnotic suggestions." Hypnotic suggestions can be subdivided into two sets: (*a*) suggestions that constitute the "hypnotic induction procedure" (typically consisting of repeated suggestions that the subject is becoming relaxed, drowsy, sleepy, and is entering a hypnotic state) and (*b*) suggestions that typically are given after the hypnotic induction procedure, such as suggestions for hand levitation, arm rigidity, analgesia, hallucination, age regression, amnesia, posthypnotic behavior, etc. Since we shall be discussing primarily the factors that determine responsiveness to suggestions, we should emphasize at the outset that theories of hypnosis have been primarily theories of responsiveness to suggestions; that is, they have been concerned primarily with explaining responsiveness to the two sets of hypnotic suggestions described above.

tive processes: (*a*) sustaining and elaborating imaginings that are consistent with the aims of the suggestions and (*b*) disregarding information that is inconsistent with the aims of the suggestions. These two cognitive processes are not separate or independent; instead, they are two sides of one unitary psychological process. An individual cannot sustain and elaborate a pattern of imaginings while also focusing on information that contradicts his imaginings. On the contrary, the process of sustaining imaginings necessarily implies the complementary process of ignoring or disregarding inconsistent and irrelevant information.

In this article we shall examine the theoretical formulations proffered by major proponents of the two alternative paradigms with an eye toward delineating the areas of conceptual agreement. We shall also examine the empirical evidence to see whether it supports these areas of agreement.

Willingness to Cooperate in Fulfilling the Aims of the Suggestions

This section is divided into two parts. The first part shows that major investigators in this area, regardless of their paradigm affiliation, appear to be in basic agreement concerning the importance of the subject's motivations and attitudes in determining response to hypnotic suggestions. The second part of the section summarizes the empirical evidence that relates to this point.

THEORETICAL FORMULATIONS

It appears that major investigators, regardless of theoretical orientation, agree that hypnotic behavior is motivated or goal directed and that the good hypnotic subject is an individual who construes the hypnotic situation as one in which he is willing to cooperate in fulfilling the aims of the suggestions. This point was made succinctly by White (1941), who contended that hypnotic behavior involves "meaningful, goal-directed striving, its most general goal being to behave like a hypnotized person as this is continuously defined by the operator and understood by the subject [p. 503]." Along similar lines, Gill and Brenman (1959) asserted that the hypnotist is successful only when he converts the subject "into a person whose major overt characteristic is his willingness, indeed his eagerness, to see, hear, think, and do what he is told to by the hypnotist [p. 10]."

Although many, if not all, investigators appear to recognize the importance of motivation for hypnotic performance, there is little agreement concerning the factors that produce favorable motivation. For example, psychoanalytically oriented writers view the motivation for hypnotic performance as deriving from dependency and from masochistic and erotic needs (Ferenczi, 1950; Freud, 1953; Kubie & Margolin, 1944; Schilder, 1956). For example, Ferenczi (1950) suggested that the motivating force behind hypnotic performance stemmed from the reactivation of the Oedipus complex, with the hypnotist and subject standing in a parent–child relationship to one another. More recently, Shor (1962, 1970) advanced a related thesis. He suggested that hypnotic responding involves three separate psychological processes. One of these, which he labeled *depth of archaic involvement*, derives from the intense needs of some subjects to cooperate with and please the hypnotist. As Shor (1962) pointed out, the notion of archaic involvement has much in common with psychoanalytic formulations of hypnosis that attribute the motivational basis of hypnotic performance to a "transference" relationship which purportedly develops between the hypnotist and the subject (cf. Gill & Brenman, 1959).

Investigators who do not accept psychoanalytic theories have also emphasized the importance of motivation in hypnotic performance, but they differ from the psychoanalytic writers in that they stress the situational sources of the subject's motivation. Both state theorists (Orne, 1959, 1966; Pattie, 1956) and nonstate theorists (Barber, 1961; Meeker & Barber, 1971; Sarbin & Coe, 1972) have noted the strong social demands for conformity inherent in the hypnotic situation. Sarbin and Coe (1972) indicated that the degree to which the subject enacts hypnotic behavior will, in part, be determined by his expectations of what the hypnotic role involves and by the extent of his self-role congruence. Extent of self-role congruence is a motivational construct referring to the degree to which the subject views the components of the hypnotic role as congruent with and acceptable to his conception of himself. Barber (1969, 1970a) has stressed heavily the role of motivational variables in hypnotic performance and has presented an extensive series of experimental studies indicating that individual differences in hypnotic performance are due, in part, to differences among subjects in their attitudes, motivations, and expectancies toward the hypnotic situation. In brief, despite differences in

pinpointing the factors that produce favorable motivation, one theoretical generalization that cuts across the state and nonstate paradigms may be stated as follows. The subject's willingness to cooperate in carrying out the aims of the suggestions is an important precondition for hypnotic performance. Let us now summarize the empirical research that supports this generalization.

EMPIRICAL DATA

As we pointed out earlier in this article, psychoanalytically based formulations of hypnosis usually stress the importance of long-standing, transsituational need dispositions as the motivating force behind hypnotic responsiveness. This line of thought has led numerous investigators to search for correlations between responsiveness to hypnotic suggestions (hypnotic suggestibility) and generalized motivational dispositions such as dependency, masochistic needs, need for affiliation, and the like. This research, which has been reviewed elsewhere (Barber, 1964), has yielded either negative or inconsistent results. The available data fail to support the notion of a relationship between hypnotic suggestibility and generalized, transsituational motivational dispositions.

Attempts to relate situational-specific attitudes and motives to hypnotic suggestibility have attained a greater degree of success. Investigators who differ in theoretical persuasion have consistently reported that positive attitudes or motivations toward the hypnotic situation are correlated with hypnotic suggestibility (Andersen, 1963; Barber & Calverley, 1966; Coe, 1964; Coe & Sarbin, 1966; Diamond, Gregory, Lenney, Steadman, & Talone, 1972; J. R. Hilgard, 1970, London, Cooper, & Johnson, 1962; Meier & Hilgard, 1964; Shor, Orne, & O'Connell, 1966).[8]

Studies that experimentally manipulated the subjects' attitudes and motives toward responding to suggestions achieved results that are congruent with the correlational studies cited above. For example, a series of studies summarized by Barber (1969, 1970a) indicated that subjects given brief instructions aimed at enhancing their motivation to perform well exhibit a level of suggestibility that is (*a*) higher than that achieved by control subjects (who are not given motivational instructions) and (*b*) about as high as the level achieved by subjects who have been exposed to a standardized hypnotic induction procedure that focuses on repeated suggestions of relaxation, drowsiness, and sleep. Several studies (Barber & Calverley, 1964a, 1964b; Cronin, Spanos, & Barber, 1971; Diamond, 1972; Kinney, 1969; Spanos & McPeake, 1973b) have also shown that the inculcation of positive attitudes and motivations can enhance hypnotic suggestibility, while the inculcation of negative attitudes and motivations can adversely affect suggestibility.

Taken together, the studies cited above indicate that subjects who view hypnosis as positive and worthwhile and who look forward to participating and cooperating are more likely to respond to hypnotic suggestions than subjects who view the hypnotic situation from a negative perspective. However, these studies also indicate that a substantial proportion of subjects with positive attitudes and a willingness to cooperate do not exhibit high levels of hypnotic suggestibility. These empirical results support a generalization that cuts across the state versus nonstate paradigms: A willingness to cooperate constitutes an important but not sufficient condition for hypnotic performance.

Involvement in Suggestion-Related Imagining

All theorists appear to agree that extramotivational factors need to be postulated in order to attain a comprehensive account of hypnotic performance. White (1941), for example, argued that hypnotic behavior will be manifested only when goal-directed strivings occur in the context of other psychological processes which he subsumed under the rubric of *an altered state of consciousness*. Gill and Brenman (1959) indicated that the subject's willingness to cooperate merely sets the stage for the occurrence of other psychological processes which they conceptualized in terms of the development of a regressed, quasi-stable ego subsystem that mediates hypnotic performance. Orne (1959, 1966, 1971) emphasized that a high level of motivation by itself cannot account for the subjective aspects of hypnotic performance. As pointed out above, Shor

[8] Similar findings have also been obtained with respect to subjects' expectations concerning their hypnotic performance. Subjects who expect to respond well to hypnotic suggestions exhibit higher levels of responsiveness than subjects who do not expect to respond well (Barber & Calverley, 1966, 1969b; Dermen & London, 1965; Gregory & Diamond, 1973; Meier & Hilgard, 1964; Shor, 1971; Unestahl, 1969). Although, from our viewpoint, expectancies play an important role in hypnotic responsiveness, this factor has not been emphasized by state and nonstate theorists to the same extent as attitudes and motivations, and thus we are not giving it primary emphasis here.

(1962, 1970) argued that appropriate motivation is only one of three factors that are necessary for hypnotic performance.

The nonstate theorists have also rejected a purely motivational theory. Sarbin and Coe (1972) stressed that hypnotic performance requires a set of imaginative skills within an appropriate motivational context. Similarly, Barber (1970b) and Barber, Spanos, and Chaves (in press) presented data indicating that, in addition to possessing positive motivations, attitudes, and expectancies, good hypnotic subjects think and imagine with those things that are suggested and, in fact, become "involved" in imagining.

The extramotivational factors have been variously conceptualized as *trance, altered state of consciousness, regressed ego subsystem, role-relevant skills, thinking and imagining with the suggestions,* and *imaginative involvement.* These varied conceptualizations have been defined and operationally delineated with different degrees of precision. However, all of these formulations share in the belief that constructs other than positive motivation or cooperation must be employed if hypnotic behavior is to be explained.

A very important consideration here is that, despite the use of different terminologies, there appears to be substantial agreement among theorists concerning the nature of the extramotivational factors that mediate hypnotic behavior. Both state and nonstate theorists appear to be converging on the conclusion that responsiveness to hypnotic suggestions involves a shift in set or orientation away from the pragmatic one that governs our everyday transactions to one that involves imagining. This shift in orientation appears to involve at least two interrelated components: (*a*) a tendency to carry out and also to elaborate imaginings consistent with the suggestions that are administered, together with (*b*) a tendency to simultaneously ignore or reinterpret information that contradicts the imaginings. We shall first present evidence indicating that both state and nonstate theorists are converging on this conclusion, and then we shall summarize the empirical work that supports it.

THEORETICAL FORMULATIONS

The importance of suggestion-related imaginings and the disregarding of contradictory information have been recognized and afforded a central position in the theoretical formulations of nonstate theorists. Sarbin (1950), for example, viewed the good hypnotic subject's ability to carry out suggestion-related imagining while simultaneously disregarding inconsistent information as a role-relevant skill. In a classic paper (Sarbin, 1950) that influenced the formulations of both state and nonstate theorists, he argued that adoption of the hypnotic role involves a shift in perspective from an alert, objective frame of reference to a relatively uncritical acceptance of what the hypnotist says. More recently, Sarbin and Coe (1972) reiterated and expanded this position. They contended that, when the good hypnotic subject is responding to suggestions, he is not concerned about evaluating the veridicality of the imaginative experiences generated by the suggestions. Instead, the good subject is actively imagining those things that are suggested and such active imagining precludes the possibility of simultaneously focusing on information that is inconsistent with the suggestions.

Sarbin and his associates (Sarbin, 1950; Sarbin & Coe, 1972; Sarbin & Juhasz, 1970) have been particularly concerned with delineating the concept of imagining. They have criticized earlier conceptualizations that viewed imaginings as *conditioned sensations* or as quasi-perceptual *pictures in the mind.* As an alternative, they have suggested that imaginings can be more fruitfully conceptualized as active, albeit covert, behavioral processes more akin to creative play acting or muted role taking than to a passive "looking at" quasi-objects in a hypothetical mind-space. For these investigators, imaginative activity involves a large component of "as if" behavior. When an individual is imagining, he is, to some extent, acting *as if* certain hypothetical events are, in fact, occurring. These investigators have argued that imagining can be usefully conceptualized as falling along a dimension of involvement where the degree of involvement refers to the intensity with which the process is carried out. Sarbin and Coe (1972) further pointed out that the tendency to become involved in imaginings can be conceptualized as a skill or ability possessed by individuals to different degrees. Successful enactment of the hypnotic role calls for one aspect of this skill, namely, the ability to become involved in imaginings that are generated by verbal suggestions.

Barber and his associates (Barber, 1970b; Barber & DeMoor, 1972; Barber & Ham, 1974; Barber, Spanos, & Chaves, in press) have stressed the role of "thinking and imagining with the suggestions" as a mediator of response to suggestions given in hypnotic situations and also in nonhypnotic situa-

tions. Barber (1970b) pointed out that responding to suggestions involves a shift in set that is analogous to the shift that occurs when a person becomes involved in watching a movie or reading a novel. He views the subject who is responsive to suggestions as actively imagining the themes that are suggested while simultaneously failing to attend to contradictory information. Thus, Barber and DeMoor (1972) stated:

> When the suggestibility-enhancing factors have given rise to positive attitudes, motivations, and expectancies, the subject is ready to think with and vividly imagine those things that are suggested. The more a subject thinks with and vividly imagines the suggested effects, the less he tells himself that the suggested events cannot or will not occur, that is, the less he covertly contradicts the suggestions. We see responsiveness to test-suggestions as directly related to thinking with and vividly imagining those things that are suggested and inversely related to thinking against (covertly contradicting) the suggestions [pp. 123–124].

Barber and his associates (Barber, 1970b; Barber & Ham, 1974; Barber, Spanos, & Chaves, in press) applied the concept of *thinking and imagining with those things that are suggested* to a wide variety of hypnotic behaviors such as suggested age regression, hallucination, analgesia, and amnesia. This viewpoint has been further developed by Spanos (1971, 1973), who has delineated one aspect of the process of thinking and imagining with the suggestions that is labeled *goal-directed fantasy*. Goal-directed fantasy involves imagining a situation that, if it actually occurred, would tend to produce the behavior called for by the suggestion. For example, a subject who was administered an arm levitation suggestion would be engaging in goal-directed fantasy if he imagined a pulley, a helium-filled balloon, or a magnet raising his arm. Spanos (1973) indicated that "good" subjects pass suggestions when they become highly involved in their goal-directed fantasies and, thereby, temporarily fail to evaluate the veridicality of their suggested experiences. On the other hand, "poor" subjects are either unwilling or unable to become involved in goal-directed fantasy. Instead, they continually evaluate and thereby contradict the veridicality of the suggestions.

A number of major theorists who adhere to the state paradigm (e.g., Arnold, Shor, Hilgard, and Gill) also view the cognitive processes subsumed under the rubric of *involved imaginings* as important in explaining hypnotic behavior. Let us look at the formulations of each of these investigators in turn.

Among earlier state theorists, Magda Arnold (1946) was the most explicit in pinpointing the importance of *intense imagining* and the interrelated process of inattention to contradictory information. For example, she wrote:

> Hypnosis essentially consists in concentrating and therefore intensifying the subject's imaginative processes. Directed toward one focus, the situation described by the experimenter, the subject will be able to imagine this situation much more intensely than he is able to do ordinarily, without the usual internal and external distractions . . . . If the hypnotized subject is told that his arm is stiff and he will not be able to bend it, he immediately conceives of his arm as being stiff, and no contradictory thought will interfere. As long as the arm is imagined as stiff the arm *feels* stiff, and with this feeling comes an actual contraction of muscles; thus, as long as this feeling of rigidity lasts, the arm really cannot be moved [p. 116].

Arnold (1946) also provided a description of the processes occurring in the subject who does not become hypnotized. Her description emphasized the unwillingness or inability of the nonhypnotized subject to "lay aside" irrelevant thoughts and to allow himself to imagine those things that are suggested.

More recent attempts to conceptualize hypnotic experiences within the context of the state paradigm have been made by Shor (1959, 1962, 1970). In his latest theoretical paper, Shor (1970) compared the good hypnotic subject to an individual who becomes highly involved in reading a novel. When an individual is in hypnosis and also when he is absorbed in reading a novel, he employes information from an external source to generate and elaborate imaginings that temporarily become the main focus of his attention. Shor also recognized the importance of the subject's ignoring information that contradicts his imaginings. He contended that the individual's usual transactions with the environment are guided by a set of rules or implicit assumptions about the world that constitute a framework for categorizing and interpreting ongoing experience. This framework, which Shor labeled *the generalized reality orientation*, allows the individual to develop a stable, consistent, and socially shared picture of reality. Shor went on to argue that at various times the generalized reality orientation may fade into the background of awareness and, thereby, become relatively nonfunctional. Thus, for Shor, hypnosis is conceptualized as sustained imaginings that occur at a time when the generalized reality orientation has faded into the background of awareness.

The theoretical formulations proffered by Ernest R. Hilgard (1965) and Josephine R. Hilgard (1970) have much in common with Shor's thinking. Two interrelated themes recur throughout the writ-

ings of the Hilgards and are particularly prominent in the work of J. R. Hilgard. The first theme is that the good hypnotic subject is especially capable of carrying out a pattern of fantasy that is initiated by some external source such as by a movie, by music, or by the hypnotist's suggestions. The second theme is that the good hypnotic subject becomes involved in his imaginative or fantasy productions. One component of involved imagining is inattention to both external information and also internal information (i.e., thoughts) that are inconsistent with the imaginings. Thus, J. R. Hilgard (1970) wrote:

> To the extent that absorption in an activity has resulted in attention to it, some selective inattention must also be involved. It might be argued, therefore, that a subject who is able through imaginative attention to reinstate vivid experiences from the past must be setting aside other stimuli that claim his attention from the real world about him [p. 86].

In line with Sarbin (1950), J. R. Hilgard (1970) delineated a number of characteristics held in common by the good hypnotic subject and the dramatic actor. She wrote that in the case of both the dramatic actor and the good hypnotic subject:

> 1. There is a temporary departure from reality orientation, to the creation of an *as if* reality. 2. The actor [in the same way as the good hypnotic subject] is not merely passive but actively receptive to the demands of the role; he is involved in it and works at it. . . . The absorption is intense, interest is concentrated and empathic, so that emotions are stirred up [p. 53].

The above quotation indicates that J. R. Hilgard, in a way similar to Shor and the nonstate theorists such as Sarbin and Barber, considers two interrelated cognitive processes—suggestion-related active imagining and inattention to information that is inconsistent with the imaginings—to be central to an understanding of response to hypnotic suggestions. Other state theorists have made reference to these two processes but have not spelled out their implications for hypnotic responsiveness as precisely as Arnold, Shor, or J. R. Hilgard. White (1941), for example, suggested, but did not fully develop, the notion that hypnotic responsiveness involves a shift away from the alert frame of reference that the individual uses to sustain his conception of reality. According to White, the poor hypnotic subject is unable or unwilling to give up this frame of reference, and, consequently, he is continually engaged in contradicting the reality of the suggestions he is given.

The simultaneous processes of sustained imagining and inattention to contradictory information are usually implied rather than directly described in psychoanalytic formulations of hypnotic behavior. Thus, psychoanalytic investigators (Gill, 1972; Gill & Brenman, 1961; Kubie, 1972; Kubie & Margolin, 1944) indicate that the induction of hypnosis involves a temporary holding in abeyance of the subject's usual reality orientation, a partial dissolution of the sense of self, and a subsequent tendency to treat the hypnotist's suggestions as if they were veridical descriptions. Recently, Gill (1972) stated that the notions of regression in the service of the ego and development of an ego subsystem, which he used earlier to explain hypnotic behavior (Gill & Brenman, 1959) are consistent with and, in fact, incorporate what J. R. Hilgard (1970) means by involved imagining.

The above discussion indicates that the cognitive processes posited by many of the major nonstate and state theorists have much in common. Investigators operating from both paradigms have stated or implied that subjects respond overtly and experientially to suggestions when they engage in involved, suggestion-related imagining. Such involved imagining is present when the subjects carry out, sustain, and, at times, elaborate on imaginings that are consistent with the aims of the suggestions, while concurrently failing to attend to or acknowledge information that contradicts the suggested effects.

Despite the foregoing important commonalities, the formulations of nonstate and state theorists contain a major difference: The two viewpoints differ in the way they view the relationship between the construct involved suggestion-related imagining and the construct hypnotic state or trance state. The nonstate theorists have rejected the utility of the notion of hypnotic state or trance state. For the nonstate investigators such as Sarbin and Barber, involved suggestion-related imagining (or its synonyms such as thinking and imagining with the themes of the suggestions) functions as an *alternative* to the traditional trance state formulation of hypnotic behavior. These investigators have replaced the trance state notion with what they believe is a more parsimonious and theoretically useful construct.

The state theorists, on the other hand, usually fail to clarify the relationship between these constructs. It is not always clear from their writings whether the construct hypnotic state or trance state is (*a*) a synonym for the construct involved suggestion-related imagining, (*b*) an antecedent condition that must exist before involved imagining can

occur, or (*c*) a superordinate construct that includes involved imagining together with other unspecified processes. Ambiguities of this kind have made it difficult to interpret the formulations of some of the state theorists. J. R. Hilgard, for example, has clearly stated that individuals who become involved in imagining outside of the hypnotic situation, such as when reading a novel or when listening to music, tend to be good hypnotic subjects. She has strongly implied, but never stated explicitly, that good hypnotic subjects are those individuals who become involved in their suggestion-related imaginings during the hypnotic session. At various points in her book she also uses terms such as *hypnotized, under hypnosis,* and *deeper hypnosis* when referring to the psychological processes occurring in good hypnotic subjects. At no point, however, does she deal clearly with the relationship between terms such as *imaginative involvement* and *hypnosis* or *hypnotic state.* Thus, it is not clear from her writings whether the "hypnotized" subject is to be considered as an individual who (*a*) is simply involved in imaginings that are related to the suggestions he is given or (*b*) is involved in his imaginings *and, in addition,* is in a special state (hypnosis) the characteristics of which remain unspecified.

We believe that many of the problems in this area could be overcome if all state theorists, including those who have not yet utilized a construct like involved imagining in their formulations, would attempt to specify clearly what they mean by hypnotic state or trance and how they see the relationship between this construct and the construct of involved imagining. If state theorists mean to say that the "hypnotized" subject is involved in his imaginings and is also in a special state (hypnosis) which has special characteristics that are over and above involved imagining, then they should clearly specify the nature of these special characteristics. On the other hand, if state theorists see the construct hypnotic state or trance as synonymous with the construct involved imagining, then much confusion would be avoided if they simply used the construct involved imagining and dropped the construct hypnotic state or trance which has many misleading implications that have accumulated during the past 200 years. As we have noted elsewhere (Barber, 1970b), if the construct hypnotic trance is now to be used synonymously with such constructs as involved imagining, then this

> will change the meaning of the term "hypnotic trance"; the term will no longer refer to a special state basically different from ordinary states of consciousness . . . . Since the construct "hypnotic trance" (or "hypnosis" or "hypnotic state") has always referred to some kind of basic, qualitative change in the organism and has accreted many associated connotations (including connotations of somnambulism or sleepwalking) attempts to give it a new meaning and new connotations will lead to confusion rather than clarity . . . [p. 65].

We will return to the state and nonstate paradigms in the concluding section of this article. Before we do so, however, let us summarize the empirical evidence which shows that involvement in suggestion-related imagining is closely related to hypnotic performance.

EMPIRICAL DATA

Attempts to assess the role of involved imagining in hypnotic performance have followed three general research approaches. The first approach assesses the degree to which individuals become involved in their imaginings outside of the hypnotic situation, for instance, while reading a novel, and then correlates these measures of involvement with hypnotic suggestibility. The second approach compares the responsiveness to suggestions of subjects who have been provided with a strategy for becoming involved in imagining with the responsiveness of subjects who have not been provided with such a strategy. Finally, the third approach assesses the degree of involvement in imagining displayed by good and poor subjects when they are exposed to suggestions. We shall discuss the research pertaining to each of these approaches in turn.

Nonhypnotic involvements and hypnotic suggestibility. A number of specific strategies have been used to assess the degree of involvement in imaginings that occur outside of the hypnotic situation. These strategies include (*a*) the use of self-report scales that assess the extent to which the subject feels that he becomes involved in such activities as daydreaming, watching a movie, reading a novel, and the like (As, 1962; Barber & Glass, 1962; Shor, Orne, & O'Connell, 1962); (*b*) semistructured interviews aimed at assessing the subject's degree of involvement in everyday imaginative activities (J. R. Hilgard, 1970); and (*c*) performance measures, such as skill in dramatic acting, that are believed to reflect degrees of imaginative involvement (Coe & Sarbin, 1966). With one exception (Barber & Calverley, 1964c), all studies in this area found a positive correlation between these various indexes of imaginative involvement and hypnotic suggestibility (Andersen, 1963; As,

1962; As, O'Hara, & Munger, 1962; Atkinson, 1971; Barber & Glass, 1962; Coe, 1964; Coe & Sarbin, 1966; J. R. Hilgard, 1970; Lee-Teng, 1965; Sarbin & Lim, 1963; Shor, Orne, & O'Connell, 1962; Spanos & McPeake, 1973a; Tellegen & Atkinson, in press). These positive results are especially impressive when it is kept in mind that imaginative involvement is the only "personality measure" that has consistently yielded positive correlations with hypnotic suggestibility.

Provision of strategies for imagining. A number of reports (Barber, Spanos, & Chaves, in press; Spanos, 1971, 1973; Spanos & Barber, 1972; Spanos, DeMoor, & Barber, 1973) have pointed out that suggestions differ in the degree that they specify a pattern of imagining or strategy which the subject can use to help him in experiencing the suggested effects. Some suggestions are highly explicit in specifying a helpful imaginary situation whereas others simply indicate the subjective effects to be experienced without providing a strategy that can help to produce them. For instance, suggestions of anesthesia of a hand that instruct the subject to imagine Novocain being injected into the hand provide an imaginative strategy for experiencing anesthesia, whereas suggestions that simply state that the hand is becoming insensitive do not provide a strategy. A number of studies have either assessed or manipulated the extent to which a strategy for imagining is provided by various types of suggestions, for example, suggestions for arm levitation, analgesia, catalepsy, and hallucination (Barber & Calverley, 1969a; Barber & Hahn, 1962; Chaves & Barber, in press; Coe, Allan, Krug, & Wurzmann, 1972; Evans & Paul, 1970; Johnson, 1973; Spanos & Barber, 1972; Spanos, Barber, & Lang, in press). The results of these studies can be summarized as follows: Suggestions that provide the subject with an explicit strategy for imagining—that ask him to direct his imagining along specific channels that are congruent with the aims of the suggestions—are more likely to be experienced than suggestions that do not provide such a strategy.

The findings of another series of studies (Comins, Fullam, & Barber, 1973; Diamond, 1972; Sachs, 1969, 1971; Sachs & Anderson, 1967) indicate that suggestibility can be enhanced by training subjects to become involved in suggestion-related imaginings. The training procedures included (*a*) teaching subjects through the use of modeling procedures and direct instructions to carry out imaginings consistent with the subjective experiences implied by the suggestions and (*b*) teaching them to exclude competing thoughts and competing sensory input from the focus of their attention. For example, Diamond (1972) assessed the effects of having prospective hypnotic subjects watch a model undergo hypnosis. He found that the best results were obtained when the model not only performed the overt behaviors that were suggested but also described in detail how and what he was imagining while he was responding to the suggestions.

Imaginative involvement while responding to suggestions. A series of recent studies assessed the relation between the degree of imaginative involvement generated by a suggestion and the extent to which the suggested effects were experienced. Spanos (1971) presented data indicating the subjects who failed suggestions for arm levitation, limb rigidity, selective amnesia, etc., were unable to sustain involvement in goal-directed imagining. In a related study (Spanos, 1973), positive correlations were obtained between the extent that the subjects reported involvement in their suggestion-related imaginings and (*a*) the likelihood that they would pass the suggestion, (*b*) the extent that they experienced their response to the suggestion as occurring involuntarily, (*c*) the extent that their imaginings were experienced as real events, and (*d*) the likelihood that they would respond successfully to more difficult suggestions.

Congruent findings have been reported by Spanos, Ham, and Barber (1973). These investigators administered a suggestion for visual hallucination to 74 subjects who had been exposed either to task motivational instructions or to a hypnotic induction procedure. The one task-motivated subject and the two hypnotic subjects who stated that they saw the suggested object and that they believed, for a few moments, that it was real, also reported sustained imagining with a concomitant failure to attend to contradictory information.

In brief, studies assessing individual differences in imaginative involvement generated by test suggestions have consistently found a positive relationship between the degree of such involvement and responsiveness to suggestions. Of course, it is important to keep in mind that these studies are primarily correlational in nature, and, therefore, causal relationships cannot be justifiably inferred from their results. Nonetheless, the results are consistent with the hypothesis that responsiveness to suggestions is, in part, a function of degree of involvement in suggestion-related imaginings.

Implications

This article has attempted to highlight the existence of significant areas of theoretical convergence that transcend the state and nonstate paradigms in hypnosis research. Specifically, many important contemporary investigators in this area seem to agree with the following generalizations: (*a*) The subjects' willingness to cooperate in carrying out the aims of the suggestions is an important but not a sufficient condition for a high level of hypnotic responsiveness. (*b*) Subjects respond overtly and experientially to suggestions when they become involved in imaginings that are consistent with the aims of the suggestions. These generalizations were shown to be supported by empirical studies carried out by investigators differing in theoretical orientation. At this point it is appropriate to consider the ramifications of the aforementioned theoretical convergence for the state versus nonstate controversy in hypnosis research.

Historically, the notion of a hypnotic state or trance developed on the basis of several superficial and misleading similarities between the behavior of mesmerized subjects and the behavior of sleeping individuals and sleepwalkers (Barber, 1973). As E. R. Hilgard (1969) noted, even today, "Hypnosis is commonly considered to be a 'state' perhaps resembling the state in which the sleepwalker finds himself, hence the term 'somnambulist' as applied to the deeply hypnotized person [p. 71]." The quasi-physiological notion of hypnotic state or trance gained theoretical ascendancy at a time when purely psychological theories were too primitive to encompass the complexities of hypnotic behavior, and when the prevailing "magnetic" theory of these phenomena was being increasingly challenged by the gains in knowledge made by nineteenth-century science (cf. Ellenberger, 1970; Sarbin, 1962). At that time, the notion of trance also became associated with highly exaggerated claims concerning the wonderous "feats" attributed to mesmerized subjects.

The nonstate view of hypnotic behavior grew gradually out of a movement aimed at curtailing the excessive claims made by some state theorists. This viewpoint became associated first with the notion that hypnotic behaviors did not involve highly unusual or transcendent phenomena and, second, with the notion that it was unnecessary and misleading to posit a special or qualitatively different state in order to explain the experiences of the good hypnotic subject.

At the present time there appears to be relatively little disagreement between state and nonstate theorists concerning the characteristics and limits of hypnotic behavior. For example, no contemporary theorist in hypnosis research argues for the existence of a unique relationship between hypnotic phenomena and sleep (Evans, 1972). Many present-day state theorists (e.g., Orne, 1971, 1972; E. R. Hilgard, 1965) are almost as critical of exaggerated claims concerning transcendent hypnotic phenomena as are nonstate theorists (e.g., Barber, 1969, 1970a; Barber & Ham, 1974; Sarbin & Coe, 1972; Sarbin & Slagle, 1972). Also, all contemporary nonstate theorists agree with state theorists that hypnotic phenomena involve genuine changes in the subject's experience that cannot be explained away in terms of faking or sham behavior (Barber, 1970b; Sarbin & Coe, 1972; Spanos, 1971, 1973; Spanos, Ham, & Barber, 1973). As we have seen in this article, there are state and nonstate theorists who seem to be converging in their conceptualizations of the cognitive processes that mediate hypnotic performance. These similarities lead to the following question: What are the important differences at the present time between state and nonstate theorists? We believe that the differences that now exist are primarily differences in approach to the subject matter and in the kinds of research questions that are pursued.

The construct trance or hypnotic state, despite its inherent vagueness and lack of amenability to operational definition, continues to dominate state conceptualizations of hypnosis. This construct seems to refer to a state that differs, not simply quantitatively, but in some basic, qualitative way, from waking states and from states of sleep. As Bowers (1966) noted, "Most [present-day] investigators interested in hypnosis believe that there is an hypnotic state which fundamentally differs from the waking state [p. 42]." This belief makes it much more likely that it will be state theorists who will conduct studies aimed at establishing a physiological basis for the hypothesized fundamental alteration. The state theorists are also more likely than the nonstate theorists to pursue research that might indicate that hypnotic performance involves unique or highly unusual changes in perceptual functioning or in cognitive functioning, such as trance logic (Orne, 1959). Although studies of this type have generally produced negative or inconclusive findings (Barber, 1969, 1970a, 1973; Barber & Ham, 1974; Hilgard, 1972; Johnson, 1972; Johnson, Maher, & Barber, 1972; Sarbin & Slagle,

1972), they have on occasion yielded some provocative results (Graham, 1969).

On the other hand, the guiding assumption of the nonstate theorists—that the good hypnotic subject is not fundamentally different from the normal individual who is cooperating in a social situation in which he is asked to experience suggested effects—will lead these investigators to continue their studies of situational and social-psychological antecedents of hypnotic performance. The nonstate investigators may be expected to probe further into the effects of such variables as how the situation is defined to the subject, what attempts are made to remove fears and misconception, and how the suggestions are worded (Barber & DeMoor, 1972; Spanos, 1973). The basic assumption of the nonstate theorists will also continue to influence their conceptualization of hypnotic performance as a set of socially influenced cognitive skills or abilities. However, despite these differences in the research proclivities of state and nonstate investigators, theoretical convergences of the type outlined in this article indicate that a good deal of the future research carried out by proponents of both paradigms will dovetail in focusing on the role of imaginative processes in hypnotic performance.

The sympathies of the present authors are, of course, associated with the nonstate paradigm. We believe that this approach offers the most useful way of conceptualizing hypnotic phenomena. Nonetheless, there is a great deal about hypnotic phenomena that remains to be learned. Diversified theoretical and methodological approaches in this area can help to generate research that will eventually lead to a more comprehensive understanding.

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Two Dimensions of Tests

Psychometric and Edumetric

RONALD P. CARVER *American Institutes for Research, Washington, D.C.*[1]

A recent development in testing involves the design and construction of criterion-referenced tests. *Criterion-referenced tests* are designed "to yield measurements that are directly interpretable in terms of specified performance standards [Glaser & Nitko, 1971, p. 653]." Traditionally developed tests, that is, those designed to measure a person in relation to a normative group, have been labeled *norm-referenced tests* (see Popham, 1971). The distinction between the new criterion-referenced test and the traditional norm-referenced test is important. However, the distinction is also misleading. It suggests that a newly developed test must be either norm referenced or criterion referenced in mutually exclusive fashion. This is misleading because a criterion-referenced test may be referenced to a normative group, and a norm-referenced test, to a criterion.

Not only is there a semantic problem underlying the new movement in testing but there is a substantive problem as well. The problem revolves around the measurement of individual differences versus the measurement of amount learned. Cronbach (1971) pointed out that "the testing movement in psychology started with the Darwinian emphasis on differences among individuals, and all theoretical work behind test scores has attempted to conceptualize differences in abilities or traits [p. 446]." McClelland (1973) argued, quite properly, that schools should be testing for competence rather than for ability. In other words, schools should use criterion-referenced tests rather than norm-referenced tests. McClelland appears to have generalized beyond schools, however, when he said that "tests should be designed to reflect changes in what the individual has learned [p. 8]." Not every test should be designed to maximize sensitivity to growth in experience or knowledge. Some tests should measure individual differences in aptitude for growth.

The thrust of the present article is that there is room for both those tests that focus on stable between-individual differences and those that measure progressive within-individual gains. Each of these focuses can be considered a separate dimension of all tests. All tests, to a certain extent, reflect both between-individual differences and within-individual growth. Because of their design and development, however, most tests will do a better job in one area than the other.

The tests that have focused on measuring between-individual differences have been called psychometric tests. Therefore, this dimension of tests will be called the *psychometric dimension*. A test may be evaluated in terms of its psychometric properties, that is, the extent to which it reflects the stable between-individual differences that traditionally have been of primary interest to psychological testing. The other dimension of tests will be called the *edumetric dimension* (see Carver, 1972, 1973). A test may be evaluated in terms of its edumetric properties, that is, the extent to which it reflects the within-individual growth that traditionally has been of primary interest to educational testing. Teacher-made tests, for example, usually focus more on the edumetric dimension rather than on the psychometric dimension.

A failure to keep the psychometric and edumetric dimensions conceptually separate has resulted in a certain amount of confusion when evaluating tests. For example, the American Psychological Association's (1966) *Standards for Educational and Psychological Tests and Manuals* states that "reports of reliability studies should ordinarily be expressed in the test manual in terms of variances for error

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Requests for reprints should be sent to Ronald P. Carver, American Institutes for Research, 3301 New Mexico Avenue, N.W., Washington, D.C. 20016.

components (or their square roots) or standard errors of measurement, or product-moment reliability coefficients [p. 29]." This focus on variances and correlation coefficients is of high relevance to the psychometric properties of a test but is completely irrelevant to the edumetric properties of a test. A test may be perfectly reliable from an edumetric standpoint while at the same time it is perfectly unreliable from a psychometric standpoint. The *Standards* does not make this kind of discrimination, however, but reflects a tendency to evaluate all tests from a psychometric standpoint. There is little recognition of the fact that it is quite possible for a test to be extremely good from the edumetric standpoint and extremely bad from the psychometric standpoint.

The remainder of this article is concerned with how to evaluate a test along both the psychometric and edumetric dimensions.

Purpose

PSYCHOMETRIC

If the primary purpose of a test is to measure individual differences, for example, a general aptitude, ability, or trait, then the test should be primarily evaluated using psychometric principles. However, the test may be evaluated from an edumetric standpoint as well.

EDUMETRIC

When the primary purpose of the test is to measure the gain or growth of individuals, for example, the measurement of knowledge, skill, or achievement, the test should be primarily evaluated using edumetric principles. The test may also be evaluated from a psychometric standpoint.

EXAMPLE

The primary purpose of a mathematics aptitude test may be to reflect stable between-individual differences that are useful for predicting achievement in a variety of mathematical tasks. The purpose of this type of test might be to predict, for example, the relative length of time required for eighth graders to learn how to calculate the square root of any number. The primary purpose of this test would be psychometric. However, if a test were developed for determining whether an individual had learned how to calculate a square root, then the purpose would be primarily edumetric.

Item Selection

PSYCHOMETRIC

The most efficient item from a psychometric standpoint is one with a passing proportion of .50 (i.e., $p = .50$). Items that all students answer correctly or items that no students answer correctly are discarded or revised. The psychometric efficiency of a test is maximized when $p = .50$ because at that point the variance in test scores is largest. Small variances automatically lower psychometric reliability and validity estimates. Another way to select good items from a psychometric standpoint is to use item–test correlations. By correlating scores on each item of a test with the total test score, the items with the highest correlations are selected because they are the best items for discriminating among individuals. Those items that everyone tends to get right or everyone tends to get wrong will automatically have low item–test correlations and will therefore be discarded.

EDUMETRIC

The most efficient item from an edumetric standpoint is one that is maximally sensitive to growth or gain. The best edumetric item is one that shows the most increase in proportion passing from a pre- to a posttreatment condition. One way to select the best edumetric type of items is to administer them in a setting in which the items should reflect change or growth and then select the items that reflect the most gain. The best edumetric items would have p values approaching .00 prior to the treatment condition and p values approaching 1.00 subsequent to the treatment condition.

EXAMPLE

The mathematics aptitude test, mentioned earlier, might be developed by administering a set of items to a group of eighth-grade students and eliminating those items that almost all students answer either correctly or incorrectly. The square-root test might be developed by administering a set of items to one group of students who had never received square-root instruction and to another group of students who had received such instruction. Those items that show little gain would be discarded.

[illegible]

[illegible]

[illegible]

[illegible] an edumetric standard [illegible] the square-root test [illegible] acceptable from a psychometric standpoint.

Score Interpretation

PSYCHOMETRIC

Raw scores on a test are usually not interpreted psychometrically because score itself means nothing unless it is compared to an average score either in a deviation from a mean or by the percentile of a normative group. Thus psychometric scores may be z scores, T scores, stanines, percentiles or any variation of these types of scores. [illegible] are developed to provide comparisons between individuals.

EDUMETRIC

Raw scores on a test may be interpreted edumetrically. The edumetric score has meaning in relation to a criterion, objective, or scale that is independent of individual differences. Edumetric scores may also be converted into psychometric scores to provide additional meaning. For example, an edumetric score may be converted into a percentile using a norm table to provide an estimate of the number of people in a normative group who would score this particular edumetric score under similar circumstances.

EXAMPLE

The raw score on a mathematics aptitude test should not be interpreted unless it was compared to the average score of a group. However, knowing if the square-root test could not be interpreted directly because it would mean something [illegible] with respect to the task of taking a square root. The interpretation of this score would not [illegible] the average score of a group.

Sample Evaluation

A sample evaluation is presented here to demonstrate the usefulness of the psychometric and edumetric dimensions for evaluating tests. The test [illegible] evaluated is the Sequential Tests of Educational Progress (STEP) Reading Test published in [illegible] by the Educational Testing Service. This test was chosen because it has been widely used (e.g., [illegible] et al. [illegible]) and because it is typical of a large number of tests that [illegible] the [illegible] cies that could be eliminated if the psychometric and edumetric dimensions were used to evaluate the tests.

PURPOSE

The stated purpose of the STEP test is to measure the ability to understand direct statements to interpret and summarize passages, and to draw inferences with respect to direct and paraphrased presentation. This purpose appears to emphasize the edumetric dimension more than the psychometric dimension. There is nothing in the purpose to suggest that the test is more relevant to reflecting individual differences with respect to these skills than it is to reflecting gain, growth, or achievement in these skills. This interpretation of the primary purpose of the test is reinforced by the fact that the test is part of an "achievement test series" and the test is designed to be used in schools. Therefore, the measurement of individual gain or growth in reading would be expected to be more important than individual differences in reading aptitude. Therefore it appears reasonable to expect the test manual to lean more toward the edumetric dimension than the psychometric dimension.

ITEM SELECTION

The edumetric dimension was not focused on in the item selection process. Instead, it was stated that the goal was a test with a modal item difficulty of 60% correct. The 60% correct value is really the standard $p = .50$ when a correction for guessing is applied. Therefore, the primary emphasis in item selection was on the psychometric dimension rather than the edumetric dimension. If the emphasis had been on the edumetric dimension, then the item selection procedure would have focused on selecting those items that reflected the most gain or growth. The test manual fail[illegible] that the "item difficulty as indicated by measurement theory" was not the only guideline for selecting items. A short description of these other guidelines suggested that they were edumetric. No some edumetric criteria were probably applied, although in an apparently [illegible]. It thus appears that item selection techniques were used that would tend to assure the development of a reliable and valid psychometric test. It is questionable, however, how good the test would be from an edumetric standpoint.

VALIDITY

At the time that the test was published, there was no validity data collected. It was stated, however, that the publisher expected to conduct validity studies, relating test scores to suitable criterion measures. This suggests that validity would be estimated in the traditional psychometric manner. There is no hint that a validity evaluation would regard the degree to which the test reflected gain or growth, in spite of the fact that the main purpose appears to be edumetric.

RELIABILITY

Only the psychometric type of reliability was estimated; that is, Kuder-Richardson 20 reliability coefficients were reported. The KR-20 is useful for estimating psychometric reliability because it focuses on item variances. If item variances approach zero, the psychometric reliability of the test approaches zero. The standard error of measurement was given, but this reliability estimate is also of the psychometric variety. If the test were to be evaluated from an edumetric standpoint, it would help if there were an estimate of the degree to which the same score on the test could be expected to be made by the same individual under similar circumstances. For example, the average difference between scores on Form A and Form B could be given. It was impossible to estimate the edumetric reliability of this test in the above manner because both forms of the test were never given to the same individuals. The data suggest that the test is psychometrically reliable. Its degree of edumetric reliability is indeterminant.

SCORE INTERPRETATION

The test manual stresses that raw scores are never interpreted. This suggests at the outset that the scores are psychometrically oriented rather than edumetrically oriented. The raw scores are first translated into what are called *converted* scores. The mean converted scores increase with each higher grade between Grades 4 and 14, and this suggests that the converted scores may have good edumetric properties. These scores are subsequently used to determine percentile bands. If the converted scores had been developed to provide an edumetric type of scores, then data regarding the edumetric reliability and validity of these converted scores should have been presented. The manual for the test never even suggests that one score may be compared with another score to determine the amount of gain or growth. Therefore, it must be concluded that the test provides scores that are of questionable usefulness from an edumetric standpoint.

CRITIQUE OF THE TEST

From an edumetric standpoint, the STEP Reading Test is of an uncertain quality because neither the item selection process, the reliability estimation process, the validity evaluation, nor the score interpretation focuses on the edumetric properties of the test. It may appear to be strange that a major test publisher would attempt to solve an edumetric type of measurement problem using a test designed by psychometric principles. This is not surprising considering that almost all standardized tests have been designed using psychometric principles irrespective of the stated purpose. When psychometric principles are used to develop an achievement test, the result is usually not an achievement test at all but an aptitude test (see Anderson, 1972). Achievement tests would be more reliable and valid if they were developed with a focus on their edumetric characteristics.

Discussion

The measurement of gain or change has been a continual nemesis to psychologists in general and psychometricians in particular (Harris, 1963). Bereiter (1963) stated that it is only with regard to problems in measuring change that he has ever heard colleagues admit to having abandoned major research objectives solely because the statistical problems seemed insurmountable. More recently, Cronbach and Furby (1970) suggested that "investigators who ask questions regarding gain scores should ordinarily be better advised to frame their questions in other ways [p. 80]." Because gain or change is an aspect of science that fascinates most scientists, it is surprising to find that Cronbach and Furby argued that "gain scores are rarely useful, no matter how they may be adjusted or refined [p. 68]." These problems with measuring change or gain always involve correlational, that is, psychometric, models. For example, Cronbach and Furby defined the reliability of gain or difference scores as "the correlation of the score with an independently observed difference [p. 70]." The gain

scores on a perfectly *reliable* edumetric test that everyone failed prior to treatment and everyone passed subsequent to treatment would have to correlate zero with any other variable, thereby rendering the gains as perfectly *unreliable* in Cronbach and Furby's psychometric model. It seems reasonable to agree with Cronbach and Furby that gain scores on a *psychometrically developed test* would rarely be useful no matter how they may be adjusted or refined. However, it is absurd to suggest that gain scores on an *edumetrically developed test* are rarely useful.

On reflection, it is hardly surprising that psychometricians have had so much difficulty with gain scores on tests. If a test is designed according to the best psychometric principles, then it would be somewhat serendipitous if the test scores reflected gain very well. If test scores that measure gain well are desired, then the test should be designed, developed, and evaluated with a focus on edumetric principles. Furthermore, the psychometrician who desires to measure and study gain must recognize that psychometric statistics, such as variances and correlations, are likely to be inappropriate. Use of edumetric principles should yield greater success.

The distinction between the psychometric dimension and the edumetric dimension has always been understood intuitively by experimentalists. For example, in prose-learning research, the researcher may develop 12 multiple-choice questions to determine how much was learned from a short reading passage. The reliability, that is, the psychometric reliability, of such a short test would of necessity be extremely low. However, this type of reliability is never even considered by the experimentalist because he is only concerned with how sensitive the test is to the change or gain that accrues from his experimental treatment. The experimentalist is only concerned with the edumetric validity of the test. The main reason the psychometrician has had problems with measuring gain or change is that the gain or change means that some treatment condition is involved and therefore the psychometrician is entering the domain of the experimentalist. The psychometrician has tried to bring his psychometric test principles with him when he entered the experimentalist's domain, but these principles are inherently inappropriate.

Many psychologists have fostered the use of psychometric test principles in education. Because education generally involves a treatment effect, it is easy to see how the intuitive edumetric approach of the experimentalist would be inherently more appropriate for education than the individual difference approach of the psychometrician. The danger involved in using the psychometric approach in education is that psychometrically developed tests will be used to measure the effects of education, and education may appear to be ineffective because the tests were not designed or developed to be sensitive to growth. One example of this danger involves the Equality of Educational Opportunity Report (Coleman et al., 1966) commissioned by Congress. This research used psychometrically developed tests and psychometric analyses and found that between-individual differences contribute more to the variance of test scores than between-school differences. This result has been interpreted as having implications for federal policy with respect to school funding (Mosteller & Moynihan, 1972) because schools appear to have little impact on achievement. It has been pointed out that psychometrically developed tests have been designed to produce just this type of a result, so the result should not be surprising nor should it be used to influence federal funding policy.[2]

Standardized achievement tests are used to evaluate innovative treatment programs in education, and many school systems use these tests to evaluate teachers and schools. These tests are usually psychometrically sound, but they may not be highly sensitive to differences in educational treatment; that is, they may be good aptitude tests but poor achievement tests. Therefore, it is quite possible that the differences between good education and poor education will erroneously appear to be small simply because the tests have been designed to be more sensitive to aptitude differences than to treatment differences.

The increasing development of criterion-referenced tests indicates that the focus in testing is turning more and more to the edumetric dimension. However, it is not generally recognized that a criterion-referenced test may have sound or unsound psychometric properties and that a norm-referenced test may have sound or unsound edumetric properties.

Psychometrics has a long, documented history, an active society, a professional journal, an overwhelm-

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[2] R. P. Carver. "The Coleman Report: Using Inappropriately Designed Achievement Tests." Unpublished manuscript, 1974.

ing influence on testing, and a multitude of statistical techniques and procedures. Edumetrics is little more than an idea. However, there is apt to be little progress in the measurement of gain, change, growth, or achievement unless the edumetric aspects are considered.

Conclusion

When a measure of individual differences is desired, a test that was developed to be maximally sensitive to individual differences should be used. Similarly, when a measure of within-individual gain or growth is desired, a test should be used that was developed to be maximally sensitive to within-individual gain or growth. Most tests are sensitive to both individual differences and to gain or growth, but because of certain design and development techniques, one of these two features is usually focused on. These two primary dimensions of tests have been called psychometric and edumetric.

A test may be evaluated with respect to both the psychometric and edumetric dimensions. Because the psychometric dimension has been focused on traditionally, many standardized tests are used to measure gain or growth without being developed or evaluated from an edumetric standpoint. The danger of this approach is that the psychometrically developed tests may not be sensitive to gain when in fact there is gain.

Miller (1969) encouraged psychologists to give psychology away to other disciplines. It appears that psychologists have been giving away sound psychometric tests to education for many years when education really needed sound edumetric tests. The edumetric dimension of tests has been sorely neglected by psychologists involved in testing. Experimentalists in psychology have known intuitively that psychometric principles are irrelevant and that edumetric principles are needed when measuring amount learned. It is hoped that future tests will be developed and evaluated with an appreciation of both dimensions so that both researchers and practitioners will have better tests with which to solve measurement problems.

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Status of Women in the Psychological Community in the Southeast

A Case Study

ELLEN KIMMEL *University of South Florida* [1]

Psychologists are called on increasingly to solve society's problems. One of the most salient of these problems concerns the "minority" status of women. Equal opportunity of education and employment is a familiar slogan and has now even become law.[2] The problem lies, of course, in implementation of the law—in spirit as well as letter. Eliminating discriminatory admission, hiring, or promotion practices will satisfy the law technically. But the *spirit* of new legislation will not be realized without attitudinal changes encouraging women to utilize new opportunities. Equality for women has been sought on practical as well as moral grounds. Full utilization of the talents of all members of a community is necessary for its health and well-being. This is a legitimate goal for community psychology. Also, because the modification of attitudes is unquestionably "psychology," it is essential that psychological expertise be directed to this issue.

Many segments of the community seek psychological assistance. In this instance, the academic segment of the community was early to seek help, as academic women began to examine their status. Women academic psychologists were contacted for assistance in collecting data on the apparent sexual inequality in employment. As these women became more "aware" of sexual discrimination in all academic areas, including psychology, they began to examine the larger community of psychology itself. To what extent do women participate in the field of psychology in this country? Before psychologists can guide the community at large toward sexual attitudinal change, surely they must first put their own house in order.

In October 1970, APA's Council of Representatives established the Task Force on the Status of Women in Psychology. Its major effort was to recommend actions based on inequalities already documented nationally by other groups. The 1971 and 1972 reports of Astin's committee made a significant contribution by suggesting ways to combat institutionalized sexism. But local applicability of these recommendations was unclear. Did regional differences in the nature of the problem warrant different approaches to solving it? Even if not, wouldn't the use of people most closely associated with a problem be necessary for its effective resolution?

Assuming an affirmative answer to the latter question and with insufficient information to refute the former, the Executive Committee of the Southeastern Psychological Association (SEPA) appointed its own Commission on the Status of Women in the spring of 1972.[3] The Commission was charged to describe the status of women in psychology in the Southeast, to develop an affirmative action plan, and to present a report at the annual meeting in 1973.

Budget precluded a face-to-face meeting of the group's members. Consequently, each agreed to provide a different piece of the puzzle of how and to what extent women contribute to psychology in the

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[1] This and the following articles were presented in modified form in a symposium at the meeting of the Southeastern Psychological Association, New Orleans, Louisiana, April 1973. The authors gratefully acknowledge the help of Jane S. Howland.

Requests for reprints of the entire case study should be sent to Ellen Kimmel, University of South Florida, FAO 270, Tampa, Florida 33620.

[2] Refer to the October 24, 1972, issue of the *Chronicle of Higher Education* for a summary of the five major legislative acts affecting women passed since 1963.

[3] Commission members were Ellen Kimmel (Chair), Annette Brodsky, LaMyra Davis, Joan Joesting, Beth Piacente (graduate student), Virginia Pendergrass, Joan Sophie (graduate student), and Judith Worell. Raymond Schrader served as liaison from the Executive Committee of SEPA.

Southeast and to SEPA. We looked at women in the role of faculty, administrators, private practitioners, researchers, graduate students, and SEPA members, through surveys of female and male psychologists, members and nonmembers of SEPA, PhD students, department chairs, and, in one case, a controlled experiment on undergraduates of both sexes.

Our definition of the "Southeast" as a region included states with significant SEPA membership or containing members of SEPA's Association of Department Chairmen. Alabama, Arkansas, Florida, Georgia, Louisiana, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia thus comprised our Southeast.

What follows is a summary of the major findings of these studies. Some of our results replicate regionally what others have found nationally. Some of our findings are unique. In advance, it can be said that women psychologists in the Southeast constitute a "minority" group. Not surprisingly, women are underutilized in many ways. On the other hand, we have some suggestions that things may be improving.

Women in SEPA

Approximately 19% (263) of SEPA's members in 1972 were female, while 23.5% of APA's members in 1971 were female. Fourteen percent (23) of SEPA's Association of Department Chairmen are female, but only one woman (2%) chaired a southeastern graduate psychology department. Only eight identifiable (by name) females head any of the 333 graduate psychology departments in the United States.[4] No member of SEPA's 1973 Executive Committee was female, and there has been only one woman among SEPA's 20 presidents. Female participation at SEPA's 1973 meetings was proportional to their number in SEPA, while 15.5% of APA's participants in the 1970 meetings were women. More women's requests to be on the 1973 SEPA program were denied due to "poor quality" (53% of those rejected) than men's (45% of those rejected).

[4] Survey by APA Educational Affairs Office, September 1972.

Women in Academe

JOAN JOESTING *Livingstone College* [1]

An abundance of studies document the status of women in academe (e.g., Astin & Bayer, 1972; Oltman, 1970), but none has particularly focused on the Southeast. To accomplish this, a questionnaire was sent to the chairs of a sample of 49 psychology departments in the Southeast. Data were sought about women faculty, graduate students, etc., and about the size of the academic institution and department.

[1] More information may be obtained from Joan Joesting, Livingstone College, Salisbury, North Carolina 28144.

Method

All 25 PhD-granting departments in the region were included, and 2 additional non-PhD departments were chosen from each state, 1 from a public and 1 from a private four-year institution. Since the institutions with non-PhD departments also had smaller student populations, they will be referred to as "small" institutions, and the PhD-granting sample as "large" institutions.[2] The questionnaire was mailed in January 1973 and a follow-up sent in February 1973.

[2] This sample was developed by a staff assistant (Jane Howland) using the 1973 *Report of Credit Given by Educational Institutions Directory*, American Association of Collegiate Registrars and Administration Officers.

TABLE 1

A Comparison of Number, Rank, and Salary of Female and Male Faculty with Doctorates at Large and Small Institutions

Rank	Female (F)				Male (M)				Difference in favor of female/male (in dollars)	Difference in favor of large institutions (in dollars)
			Salary (9 mos., in dollars)				Salary (9 mos., in dollars)			
	N	%	$\bar{X}$	Range	*N*	%	$\bar{X}$	Range		
Large institutions										
Professor	11	6	19,350	16,700–27,500	141	94	20,412	16,250–24,000	1,062 M	5,150 F 2,527 M
Associate	7	7	16,640	12,500–19,040	100	93	15,221	13,916–16,583	1,419 F	2,278 F 2,417 M
Assistant	18	12	12,387	11,250–13,550	128	88	12,432	11,415–13,552	45 F	1,344 F 586 M
Instructor and other	12	46			14	54				
Small institutions										
Professor	4	22	14,200	11,500–16,250	14	78	17,885	12,500–19,438	3,685 M	—
Associate	4	25	14,362	12,400–15,350	12	75	12,804	11,600–14,900	1,558 F	—
Assistant	8	26	11,043	9,000–11,800	23	74	11,846	10,000–12,450	803 M	—

Results and Discussion

Twenty-two of the 25 large institutions and 14 of the 24 small institutions responded. Table 1 gives the number, rank, and salary of female and male faculty with the doctorate for large and small institutions. Women comprised 9% (36) of the 420 psychology faculty with professorial rank and a doctorate in the large institutions and 29% of the 55 faculty in the small institutions. Women in large institutions held 6% (11) of the professorships, 7% (7) of the associate professorships, 12% (18) of the assistant professorships, and 21% (12) of all other PhD faculty appointments. At large institutions, 6 females of professorial rank did not have the doctorate (5 were tenured), while no males with academic rank lacked a PhD. Six female and 5 male non-PhDs were instructors, but because of the small *N*, their salaries will not be discussed. In small institutions, 73% of the faculty with professorial rank held PhDs (16 females, 49 males), and women with PhDs were 22% of the full professors, 25% of the associate professors, and 26% of the assistant professors.

Men with full professorships earned an average of $1,062 more than women having that rank. However, the average salary of female associate professors was $1,419 higher than that of male associate professors. Female assistant professors earned more than male assistant professors. In small institutions, male professors (with PhDs) earned $3,685 more than female professors, while women associate professors earned $1,558 more than men in that category. Male assistant professors earned $803 more than female assistant professors. Since the *N*s are small, these differences may not be reliable. Comparison of salaries of the PhDs with the same rank and sex in large versus small institutions showed differences in favor of the large institutions for each rank, with the greatest difference at the rank of full professor for both sexes. Females have wider salary ranges at the larger institutions than males, but not in the small institutions. It may be that salary differences favoring men are smaller in the large institutions because larger institutions have had more intense and earlier affirmative action activities than smaller, particularly private, institutions. Some larger institutions who responded have completed university-wide salary equity studies to equalize salaries for women and men.

In small institutions, there were 9 female and 10 male faculty without doctorates. Their salaries

were only slightly lower (within rank) than their PhD counterparts. Women assistant professors (5) earned $1,037 less than men (3). Only 1 person without a PhD (a woman) held the rank of associate professor.

Twenty-five percent of the women and 63% of the men in large institutions had tenure, as compared to 59% of the women and 44% of the men in smaller institutions. Since few women in large institutions were in the upper ranks, it was not surprising that fewer had earned tenure. It is possible that women in small schools are given tenure (and rank) instead of more money. Or, women may have fewer opportunities to move to a better (bigger) institution.

Concerning special recruitment of women faculty, there were some interesting differences between large and small institutions. Eighty-two percent of the large, but only 7% of the small, institutions responded yes to the question, "Has your *department* made any special efforts to recruit women psychology faculty?" Eighty-two percent of the large and only 22% of the small answered yes to the question, "To your knowledge is your *institution* actively trying to hire more women faculty?" One department (large) reported that its institution actively discouraged the hiring of women faculty. Many departments advertised their interest in women and minorities, or sent recruitment letters to this effect. Others listed vacancies with the Association for Women in Psychology. One department head wrote, "We try to find a woman first . . . then look for a man if we can't find a girl [*sic*]." Large departments appeared to be sensitive to the need for hiring more women and are trying to do so. One small institution reported that it was willing to hire either a male or a female and, in fact, did interview women the last time it recruited. Fifty percent of the large and only 7% of the small institutions reported antinepotism regulations, a major obstacle to the hiring of women faculty. Nine percent of the large and 14% of the small did not answer this question at all, and one institution (small) responded ambiguously. There probably still are *unwritten* antinepotism rules. Those remaining departments officially proscribe persons related in the first degree of consanguinity from working either in the same administrative unit and/or directly supervising the other. One department chair indicated that such regulations led to the loss of "four superb faculty persons that we otherwise would have hired."

Three questions concerned graduate students. The large institutions reported that 41% (93) of their MA students and 37% (523) of their PhD students were women. Small institutions indicated that 47% (151) of their MA students were female. Sixty-one percent of the large, but only 36% of the small, institutions permitted graduate students to register part time. The APA Task Force (1973) survey found that 54% of graduate departments permitted part-time registration. The Southeastern Psychological Association (SEPA) departments reported that there were few apparent differences between the attrition rates of women and men students. One institution claimed higher attrition rates for females, but did not send the data. There was no feeling a priori that women are less likely than men to complete graduate training. Since no "hard" data were presented in response to our request, it is assumed that departments have none. It is unfortunate that records of applications and admissions are infrequently maintained for any period of time.

None of the large institutions and only one of the small institutions have an undergraduate major in women's studies, although 32% of the large and 14% of the small institutions have women's studies programs. Thirty-two percent of the large and 14% of the small institutions offer a psychology of women course. At large institutions, female faculty, more frequently than male, taught these courses. At small institutions, male faculty were more likely to teach the women's studies courses, probably because few female faculty are available. One respondent said, "We may, when we hire a female or two." Also, 22% of the large and 29% of the small departments indicated that they were planning to offer such a course in the future. One department with no such plan said, "No, the reason is simple: There *just* isn't that *damn much difference* between men and women . . . ! Such courses drive wedges between us where understanding, harmony, and equity are needed!!!!!!" However, APA's Task Force on the Status of Women (APA, 1973) recommended that "colleges and universities should offer (a) general education courses and programs that inquire into the psychology of women and the forces affecting their status and (b) an opportunity for in-depth study, at both undergraduate and graduate levels, of the psychology of women [p. 613]." Thirty percent (25) of the large departments in APA's survey reported offering a course on the psychology of women in the 1971–1972 aca-

demic year or before. Since 33% already offered and 29% of SEPA's sample planned to offer a psychology of women course, the Southeast is in step on this issue.

Conclusion

Women psychology faculty in the Southeast are few in number in large as well as small institutions, more so in the larger institutions. A comparison of our data with that of APA's (1973) survey showed slightly less representation of women on faculties in the Southeast than nationally. Our study indicates that salaries of women full professors are substantially lower than those of men, but that the reverse is true for associate professors. No differences in salary were found for assistant professors. There was a suggestion of greater differences in pay between women and men at small institutions. Salaries in general are higher in all ranks for both men and women in the larger institutions. With women seldom holding higher ranks in large institutions, tenure was also more difficult for them to earn. In small institutions, women were better able to achieve both high rank and tenure.

As in the APA study, our data showed that a substantial number of MA students are women, but women are still underrepresented among doctoral students. We found no evidence supporting the notion that women show higher attrition rates than men in graduate school. Departments verbalized no impressions that "women are a bad risk." Southeastern graduate departments permit part-time study as frequently as departments nationally, but there is room for improvement for both.

The status of women was related to the size of the department and institution. Women are employed as faculty members in small institutions roughly in proportion to the percentage of doctorates women earned in psychology. This is not true in large institutions. Overall, accordingly, women remain underrepresented in academe. Women in small schools have a better chance to attain tenure and promotion than women in large schools, although their salary status in relation to men's is worse. Finally, the substantial number of psychology of women courses and women's studies programs, especially in large schools, shows an awareness of the need to include women in the curriculum.

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Women as Graduate Students

ANNETTE BRODSKY *University of Alabama* [1]

The survey reported here explored areas of potential discrimination that are not evident from the usual statistical reports. Thus, many items were selected in open discussion with several of the women graduate students at the University of Alabama, Tuscaloosa. Emphasis was placed on gathering information from students themselves, about such things as their assignments, interpersonal relationships, and the encouragement they receive. The question was asked, "How much are these 'soft' signs of discrimination felt by graduate students and in what ways?"

[1] Detailed information may be obtained from Annette Brodsky, Department of Psychology, University of Alabama, Box 2968, University, Alabama 35486.

Method

A 45-question survey was sent to 24 of the 25 (one was omitted unintentionally) departments in the southeastern United States that offer PhD programs in psychology. Each was requested to distribute 10 questionnaires in the following manner:

> Make an alphabetical listing of all currently enrolled female graduate students and divide the total number of them by five. For example, if you have fifteen female graduate students currently enrolled, divide by five and then select every third name. Then use the same procedure for the male graduate students. If any of the students who are randomly selected are away from campus (such as those on internships) please forward their form promptly.

Thus, five female and five male doctoral students were selected in each department, with a potential total of 240 student replies. Of those, 150 questionnaires, from 77 males and 73 females, had been received two months after the questionnaire was distributed.

We had no control over the compliance of the department chairs (all males) in distributing the questionnaire. To the usual problems associated with return rates, we must add one more source of possible bias: An uncooperative chair meant the loss of 10 respondents. However, replies contained postmarks from all the states that comprise the southeastern region, except Mississippi which has no PhD program in psychology.

Results and Discussion

The sexes were remarkably similar in age (females: range 21–39 years, mean age 25.3 years; males: range 21–41 years, mean age 26.4 years); Graduate Record Examination (GRE) scores (female median, 1,208; male median, 1,245); and undergraduate grade point average (GPA) (female, 3.26; male, 3.17). Psychology doctoral students in the Southeast tend to come from the Southeast (females, 56%; males, 39%). As Table 1 shows, programs were selected mainly for financial aid, department characteristics, and faculty characteristics. More women (19%) were influenced by a spouse or loved one than men (1%).

Only one item concerning motivation to enter psychology strongly suggested a sex difference. Thirty-two percent of the males checked income potential but only 12% of the females. Also, men slightly more often than women checked prestige of the field, freedom of hours, and freedom from supervision.

In terms of careers within psychology, there were few sex differences. Ten percent of the males said they were seeking administrative jobs, compared to 1% of the females. Women who planned to enter clinical fields saw themselves more likely to work in educational or institutional settings, while more

TABLE 1

Reported Basis of Selection of Current Program

Reason	% females	% males
Reputation of the institution	16	18
Reputation of the department	27	35
Knowledge of certain faculty	22	31
Personal acquaintance on faculty	10	21
Personal acquaintance in program	10	14
Proximity to home or job	29	22
Spouse or loved one's association with institution or program	19	1
Geographical location (climate)	19	26
Offer of financial aid	34	43
Best acceptance received	37	48

males (29%) than females (18%) planned to go into private practice. (See Pendergrass's results, pp. 533–535, this issue.)

Table 2 shows that parents and spouses were sources of encouragement to enter psychology for about 25% of both sexes. Friends were more of an influence for females. The most frequent source of encouragement for both sexes was male teachers (males, 40%; females, 33%). However, female teachers were reported as an influence by 33% of the females, but by only 16% of the males. If same-sex role models are important to college students, the small number of female professors may result in fewer females applying to graduate programs. Thirty-nine percent of the males and 31% of the females entered with no specific source of encouragement.

When students were asked what they expected their major duties to be in 10 years, females ranked clinical work highest (60%), then teaching (49%), research (34%), service (20%), and administration (12%). Males, however, selected clinical,

TABLE 2

Sources of Encouragement to Attend Graduate School

Source	% females	% males
Father	22	21
Mother	27	29
Spouse	23	26
Other relative	5	10
Same sex teacher	30	40
Opposite sex teacher	33	16
Male friend	19	12
Female friend	20	6
Other	5	8
No one	31	39

TABLE 3

Sex Discrimination Felt by Graduate Students in Four Areas of Graduate School Experience

Area of concern	% females	% males
Academic grades	8	21
Attitudes of faculty	40	12
Research assignments (or assistantships)	14	3
Course assignments	5	1

teaching, and research activities with equal frequency (44%), and then administration (29%).

In the area of family responsibilities there were some expected sex differences. Only 27% of the 150 respondents were parents: Twice as many men, 18%, as women, 9%, had children. This agrees with other findings on parenthood and graduate school enrollment, namely, that the presence of young children at home is negatively correlated with graduate enrollment for women (Feldman, 1972). Sex differences were striking in the percentage of total nonfinancial responsibilities for the child: males, 41%; females, 64%. Thus, while psychology PhD students are not often parents, the responsibility weighs more heavily on graduate student mothers than fathers. This is related to the additional finding that more male graduate students were married (65%) than female graduate students (42%). This is true nationally also (Feldman, 1972). Because the males and females did not differ in age, it may be that females who marry and raise children do not attempt to enter graduate school in the first place. Because married women PhD psychologists have children at the national rate (Fields, 1972), it is likely that female graduate students simply delay marriage and families.

Sex differences regarding child care were unclear. Both males and females used their spouses (males much more so) as baby sitters and felt that lack of time to spend with the children because of graduate student duties was an important problem. Cost of child care was less of an issue for females than males, but the numbers were too low to draw any conclusions. Women reported guilt more often for not participating in *professional* opportunities. Men reported the reverse—guilt for not participating in *family* activities. Sex role stereotypes in the ranking of responsibilities between the sexes are still apparent in that women placed family and men placed professional obligations as primary.

Our data did not indicate discrimination against women in areas of graduate student experience where it is often suspected. Males and females equally reported receiving fellowships and research and teaching assistantships. Assistantship duties were also similar for the two groups. More males reported full teaching responsibility, while females were more often teaching assistants.

Table 3 shows that 21% of the males reported feeling sexually discriminated against on grades, while only 8% of the females did. Females felt more discrimination in research assignments and assistantships (14% females versus 3% males). Most striking was the fact that 40% of the females but only 12% of the males reported feeling sex discrimination in the attitudes of the faculty.

Table 4 shows that men and women reported equal encouragement to attend meetings and publish research and that females were more often encouraged to present papers than males. On the other hand, slightly more males than females reported an invitation to accompany a faculty member on a professional trip. Fifty-two percent of the males but only 41% of the females were offered authorship for research participation. Males were more often encouraged to meet important scholars outside the department (34%:19%) and to teach a course, or supervise students (36%:23%).

There was no apparent difference in social treatment between the sexes. Two thirds of the graduate students of both sexes were invited to at least one party by a faculty member. The current group of female graduate students in psychology departments in the Southeast seemed to feel as secure about their presence as their male counterparts.

Conclusion

There are far more similarities than differences between women and men graduate students in the

TABLE 4

Types of Encouragement from Faculty

Type of encouragement	% females	% males
Attend professional meetings	37	40
Write for publication	37	36
Present a paper	33	25
Invited to accompany on consulting trip or convention	19	35
Offered authorship	41	52
Meet scholars outside the department	19	34
Teach a course or supervise students	23	36

Southeast. Glaring discrimination is *not* perceived to be present by women psychology graduate students as a group. The absence of married women and/or older women, especially those with children, and their relative absence in certain selective, high-prestige student positions were somewhat suspect since the age, GPA, and GREs of the women were so similar to those of the men. The absence of women's administrative aspirations was discouraging, particularly because they indicated feeling secure in their status with regard to their peers. Since women are more likely to have a discontinuous school pattern due to child rearing (Hembrough, 1966), one would expect a higher percentage of women than men in the sample to be in the over-30 age range. However, 8 women and 16 men were 30 or over. The modal age for both groups was 23, but by the age of 28, women had far less representation (9 women over 28 as opposed to 21 men). Also, almost all of the respondents were full time, which suggests discouragement if not outright prohibition of part-time training, although 61% of the department chairs (answering their own survey) indicated that part-time graduate study was permitted.

The similarities between the sexes in such things as financial aid, areas within psychology pursued, assignments, and general encouragement from faculty lead to the general conclusion that women graduate students in the Southeast feel that they receive equal treatment to men. While they may feel as secure about their professional presence as the males do, they perceive personal attitudes from the faculty which indicate that women in psychology graduate programs in the Southeast are not yet fully accepted as individuals.

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Women as Experimenters

BETH STEARNS PIACENTE *University of South Florida* [1]

A great deal of evidence has accrued during recent years to support the feminist allegation that both overt and covert prejudice against women seeking, or functioning within, professional roles exists not only among the general lay public but at the professional level as well. For example, Goldberg (1968) gave female college students identical articles in a variety of disciplines (e.g., law, education, city planning) to evaluate, but varied the sex of the author. Articles were judged to be less valuable and their authors less competent when the author was female.

Findings seemingly inconsistent with these results have been reported by Pheterson (1969), who replicated the Goldberg (1968) procedure with the exception that the subjects were middle-aged, "uneducated" women. Those subjects judged the articles allegedly written by women as nearly equal to those allegedly written by men. A further study conducted by Pheterson, Kiesler, and Goldberg (1971) may shed some light on the apparent dis-

[1] This study is based on a thesis submitted by the author in partial fulfillment of the Master of Arts degree at the University of South Florida. More information may be obtained from Beth S. Piacente, Department of Psychology, SOC 326, University of South Florida, Tampa, Florida 33620.

crepancy between the Pheterson (1969) study, which found women to be judged of equal if not greater competence than men, and the Goldberg (1968) study, which found greater perceived competence in men than women. Pheterson et al. asked women college students to judge a set of paintings. The sex of the artist and the independently judged quality of the production (whether the painting was described as an entry or a winner in an art contest) were systematically varied. It was found that when the subjects were told that the paintings were simply entries, paintings by male artists were rated better than those by female artists. However, no sex-related differences were found in the ratings when subjects were told the paintings were winners. In the Goldberg (1968) study there was much greater ambiguity as to the competence of the women and thus, in the absence of further information about the women they were judging, subjects in the Goldberg study assumed the women to be less competent than the men.

Two generalizations are suggested from the literature. First, the professional efforts of females will tend to be judged less favorably than those of males, provided some ambiguity exists regarding the competence of the individuals judged. Second, based on the observations of such researchers as Silverman, Shulman, and Wiesenthal (1972) or Rosenthal (1967), it appears that when women function (or are perceived to function) with relatively unambiguous competency they do so at the "cost" of losing some measure of their femininity.

The purpose of the present study was to evaluate experimentally some implications of those two generalizations. The evaluative perceptions of undergraduate psychology majors regarding the performance of female and male experimenters were studied in a situation where competency was systematically manipulated.

Method

SUBJECTS

The subjects were 214 females and males in the introductory psychology course at the University of South Florida.

EXPERIMENTAL DESIGN

The design included four between-subject variables, varied factorially, and one hierarchical variable. The between-subject variables were (*a*) sex of the videotaped experimenter; (*b*) manner of the videotaped experimenter (competent or incompetent); (*c*) sex of the experimenter showing the videotapes; and (*d*) sex of the subjects. The hierarchical variable was the *persona* (e.g., the performance, appearance) of the four confederates who played the roles of the videotaped experimenter. Two female and two male confederates each made one videotape in which they acted competently and a second videotape in which they acted incompetently. The confederates were unaware of the major hypotheses of the study.

DEPENDENT MEASURE

The dependent measure was a 20-adjective (listed in Table 1) semantic differential scale. The instructions were the same as those given by Osgood et al. (1957).

PROCEDURE

The subjects were shown the videotapes in groups of 10 to 25. They were told that the videotape was of undergraduates who were being trained as experimenters and that the experimenter wanted the impressions of a group of people who would be similar to the subjects in the actual experiment. It was further explained that the experimenter seen in the videotape was not aware that she/he had been videotaped through a one-way mirror. The videotape was then shown. After viewing the tapes, the semantic differential was administered with instructions to rate the experimenter on each scale. Approximately one week after the completion of the study all subjects were debriefed.

VIDEOTAPES

The videotapes were approximately 15 minutes in duration. The experiment conducted in the tape was supposedly designed to measure subjects on physiological responses to stress, namely, instructions to administer high levels of shock to a laboratory white rat. A complete description of the apparatus, procedure, and behaviors displayed by the experimenters can be found in Penner, Hawkins, Dertke, Spector, and Stone (1973). In the competent condition, the experimenters acted in a confident, self-assured manner, made no mistakes, and remained calm even when, on the seventeenth trial, the rat seemingly was seriously injured. In the incompetent condition the experimenters started the experimental session late; had an incorrect list of the subjects' names; did not provide an adequate number of chairs for the subjects; had difficulty attaching the physiological sensing electrodes; read the instructions haltingly, suggesting that they were unfamiliar with them; were unable to operate the equipment on the practice trials; and became extremely agitated and distressed when the rat was apparently injured.

Results

A varimax rotated factor analysis (Mulaik, 1972) was performed on the 20 bipolar adjectives, collapsing across between-subject and hierarchical variables.[2] Those adjectives that loaded together on the same factor were combined and the subjects' responses to these adjectives were summed in order to yield a single score. These scores were then

[2] Two additional factor analyses were conducted collapsing across all variables except the manner of the experimenter. The factor structure and loadings were essentially the same for all three analyses.

TABLE 1

Summary of Significant Sources of Variance for Twenty Semantic Differential Bipolar Adjectives

Adjectives	Sources of variance
Active–Passive	ACD
Attractive–Unattractive	A
Complex–Simple	—
Competent–Incompetent	C
Confident–Nervous	C
Efficient–Inefficient	C, AE
Excitable–Calm	A, C, AC
Experienced–Inexperienced	C
Fast–Slow	C, BCDE(A)
Good–Bad	A, C
Hard–Soft	C, AC, ACE
Impulsive–Deliberate	C, ACD
Masculine–Feminine	A, C, AC, CD
Positive–Negative	C
Professional–Novice	C, BC(A)
Serious–Humorous	C
Severe–Lenient	C, E, AC
Strong–Weak	C, AC, DE
Successful–Unsuccessful	C, B(A)
Wise–Foolish	C, BD(A)

Note. All F ratios shown were significant at $p < .025$ or beyond. A = sex of videotaped experimenter; B = "persona" of videotaped experimenter; C = manner of videotaped experimenter; D = sex of experimenter showing videotapes; E = sex of subjects.

subjected to a five-way analysis of variance described above.

In order to simplify the analyses, subjects were randomly eliminated until equal numbers in each cell were obtained. All analyses were based on the responses of 80 female and 80 male subjects.

In the preliminary analyses it was found that the procedure of combining those adjectives that loaded together introduced excessive error variance on the sex of the videotaped experimenter variable. That is, although some of the adjectives that had been combined discriminated between female and male videotaped experimenters, others did not discriminate. Since sex of the videotaped experimenter was a crucial variable in this study, 20 separate analyses of variance were conducted.

Since many of the semantic differential items were intercorrelated, traditional statements about the probability of committing a Type I error due to multiple independent tests of significance did not seem appropriate. However, in order to reduce the possibility of Type I errors, a relatively stringent α level was chosen (.025). Fisher's least significant difference (LSD) test, with the significance level set at .02, was used for all subsequent analyses.

Table 1 presents a summary of the 20 analyses of variance. As can be seen from this table, 36 out of the 460 possible F ratios were significant at or beyond the .025 level. In the interest of economy, only those F ratios that are germane to the hypotheses of interest will be discussed.

All of the following significant main effect F ratios had 1/128 degrees of freedom (df). Competent experimenters were seen as significantly different in the expected direction from incompetent experimenters on 14 of the 20 bipolar adjectives. The sex of the videotaped experimenter yielded significant F ratios on four adjectives. Males were seen as *better* and more *masculine* than females, while females were seen as more *excitable* and more *attractive* than males. The sex of the subjects viewing the videotapes yielded one significant main effect. Males rated all experimenters as more *severe* than did females. Individual videotaped experimenters differed only on the bipolar adjective *successful–unsuccessful.*

Significant F ratios (each with $df = 1/128$) were obtained for the following first-order interactions. Again, using the LSD test, it was found that male incompetent experimenters were seen as *harder, stronger,* and *calmer* than female incompetents. On the other hand, female competents were seen as more *severe* than any other experimenters. Incompetent females were seen as more *feminine* than were competent females. Also, it was found that males rated male videotaped experimenters as more *efficient* than female videotaped experimenters (females did not discriminate on this adjective). When subjects were run by male experimenters, they perceived the experimenter functioning under the incompetent condition as more *feminine* than under the competent condition, a difference not present when a female experimenter conducted the actual experiment.

Two second-order interactions germane to this study were found, again each with $df = 1/128$. Male subjects did not discriminate between competent and incompetent videotaped male experimenters on the hard–soft dimension, but did rate incompetent female experimenters as *softer* than competent female experimenters. Female subjects discriminated between competent and incompetent experimenters within sex, but did not discriminate within "manner" across sex on this bipolar adjective. The second interaction showed that when subjects were shown videotaped female experimenters by a female experimenter they did not dis-

criminate between competent and incompetent conditions on the bipolar adjective *impulsive–deliberate;* however, subjects did discriminate between competent and incompetent experimenters on this dimension on all other conditions.

Approximately 10% of the subjects suspected that the purpose of the study was other than that which was given; however, none of these individuals was aware of the true purpose of the study.

Conclusion

The results of this study tend to confirm the hypothesis that when there is little ambiguity as to their competence, women and men are judged to be equally competent, but when there is reason to doubt competence, women are perceived as much less competent than men—more so by men than by women. In addition, competence in women is accompanied by a loss in femininity (in the view of others) and incompetence is accompanied by an increase in perceived femininity. To the extent that these results possess external validity they may offer a partial explanation of the greater "dropout" rate among female graduate students in psychology or other fields (Creager, 1971) and the dearth of women psychologists in highly prestigious academic settings which Joesting's (pp. 520–523, this issue) and Kimmel's (pp. 536–539, this issue) studies of the Southeast and other national surveys have documented.

The results of this study may be interpreted as showing that a woman who achieves competency in her chosen field may do so at the cost of losing those characteristics that may be popularly considered desirable in heterosexual and/or social interactions. It may be that women pursuing a professional career become cognizant of this cost and decide not to pay it. Further, even if a woman decides to pursue a professional career, she may face the difficulty of being assumed incompetent until she clearly demonstrates that this is not the case. Current trends toward hiring greater numbers of women in professional positions may alleviate the difficulty of proving competence; it may not, however, alleviate the cost to a woman of being perceived as competent.

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Women as Scholars

JOAN SOPHIE *University of South Florida* [1]

Publication in scientific and professional journals is generally regarded as an index of scholarly productivity and is frequently a requirement for academic employment and promotion. Differences in publication rate between women and men could account, therefore, for their differential status in academe evidenced by the decreasing proportion of faculty members who are female at the higher aca-

[1] This study is based on a thesis submitted by the author in partial fulfillment of the Master's of Arts degree at the University of South Florida. More information may be obtained from Joan Sophie, Department of Psychology, SOC 326, University of South Florida 33620.

demic ranks (see Joesting, pp. 520–523, this issue; APA, 1973).

Astin and Bayer (1972) found the correlation between academic rank and publication to be .49. However, when other relevant variables, including publication, were partialed out, there remained a partial correlation between sex and academic rank of .17, which is significant. While publication is an important determinant of academic rank, women who publish as much as men still do not achieve comparable rank. This discrimination itself could lead to further discrepancies in productivity, because the time and facilities for conducting research, as well as the motivation to do so, would be less for those kept at a lower academic rank.

Simon, Clark, and Galway (1967) examined the effects of marital status and sex on publication and academic rank. Women were divided into three categories: unmarried, married without children, and married with children. Men could not be subdivided because almost all were married and had children. While there were many significant differences among these groups, they were similarly productive. Married women were more likely to publish at least one article than men or single women, and men were more likely to publish at least one book. Thirty-five percent of the men and only 25% of the women had received at least one grant. It may be that the difference in percentage receiving grants, for whatever reason, accounts for the differences in book writing.

Guyer and Fidell (1973), sampling PhD women and men psychologists from the 1968 APA *Directory,* found that men published an average of .61 papers per year compared to .21 for women, a difference that was significant, but that occurred only at the full professor level. When age, academic position, rank of institution, and theoretical or applied interest were partialed out, no remaining difference in rate of publication was left. They concluded, therefore, that there were no differences in publication between women and men.

The present study is similar to Guyer and Fidell's, except that it was limited to psychologists in the Southeast (members of the Southeastern Psychological Association, SEPA), included non-PhDs, and obtained information directly from them by questionnaire. The present study looked at sex and various demographic, educational, employment, and scientific communication variables as predictors of rate of publication.

Method

The names of 110 women and 110 men were randomly chosen from SEPA's 1972 membership list. They were sent a questionnaire, "Characteristics of Academic and Nonacademic Psychologists," in December 1972. Care was taken to avoid identifying the study as a status-of-women project by choice of title and contents of the cover letter, a procedure considered necessary in order to avoid bias. Five weeks later a reminder letter was sent. Sixty-five women (59%) and 58 men (53%) responded.

Questions covered four areas: demographic information, education, employment, and scientific communication. Demographic variables included age, sex, marital status, number of children, each parent's education and occupation, degree of religious emphasis during childhood, number of siblings, and birth order. Education variables were highest degree and date received, and academic honors, awards, and honor societies. Employment variables included primary work activity; place of employment; full or part time; number of years employed as a professional psychologist; rank order of various types of work activity: administration, counseling, research, teaching, etc.; and, for those who were teaching, information on courses taught and students advised. Scientific communication variables consisted of number of grants applied for and received, membership in professional and scientific societies, attendance at professional and scientific meetings, oral presentations, number and type of publications, and order of authorship.

A multiple regression program was used several times, varying the specific independent variables included each time and also varying the dependent variable, from number of journal publications to total number of publications of all kinds. In addition, separate analyses were performed for women and men, again using a number of selections of independent variables. Since the choice of dependent variable did not affect the results, only the results for number of journal publications will be discussed below.

Results and Discussion

Women and men in the sample did not differ significantly in age or in the date on which they received their highest degree (see Table 1). There were significant differences, however, in education, place of employment, marital status, and basic versus applied area of specialization. Only 51% of the women, in contrast to 81% of the men, had PhDs; more men (48%) were employed in universities than women (32%); and, finally, more men doctorates (47%) specialized in basic areas of psychology than women doctorates (37%). All of these findings are consonant with the other surveys reported here, with APA's (1973) as well as with other national surveys.

Women in our sample published significantly less than men: The mean number of journal articles published by men was 6.0 compared to 2.4 for women. However, when the effects of variables that contributed more to the regression equation than sex were partialed out, the remaining correlation

TABLE 1

General Characteristics of the Respondents

Characteristic	Females		Males	
	No.	%	No.	%
Age (mean)	41.3		40.2	
Date of degree (mean)	1963		1964	
Highest degree				
PhD	33	51*	47	81*
EdD	8	12	4	7
MA or MS	24	37	7	12
Employment				
University		32		48
Four-year college		18		16
Junior or community college		3		0
Nonacademic		47		36
Area of specialization				
Applied		63		60
Basic		37		40
Marital status				
Married	40	62*	54	93*
Single	18	28	2	3
Divorced, separated, or widow/widower	7	10	2	3

* $p < .001$.

between sex and number of publications was negligible (.07).

Variables included in this study accounted for most of the variance in publication, with predictability greater for men than for women. The major regression equations accounted for 81% of the variance in publication for men ($R = .90$) and 64% for women ($R = .80$). These equations used the independent variables of age, highest degree and date received, and all of the suitable employment and scientific communication variables. In addition, marital status and honors were included in the analysis for women, but not for men, because they had been found previously to have predictive value only for women. Other demographic variables, such as parent's occupation or religious training, were eliminated for both sexes because they had little effect when added to the educational and employment variables. The variables that contributed most to the regression equation are listed in Table 2. Although the order of importance of the different variables was not the same for women as for men, the same variables were, in general, important for both. Not surprisingly, the person who published most was likely to have a PhD, work in a university, be involved in guiding graduate students' research and in doing her or his own research, be a member of several scientific societies, make frequent oral presentations, and apply for and receive research grants. Only two, time spent writing and number of oral presentations, of the corresponding correlations listed in Table 2 are significantly different for women and men. Both were more highly correlated with publication for men than for women.

Guyer and Fidell also reported greater predictability for men than for women. Perhaps since the system of achievement in academe is designed for men, the variables of this system predict their career patterns better than they predict those of women. For women there is a conflict between the academic pattern and the life pattern they are expected to follow as women. This conflict can also be seen in the large difference in the proportions of each sex who are married and in the lesser percentage of married women who have children compared to married men. (In our sample, 75% of the married women and 85% of the married men had children.)

In order to determine the independent effects of the demographic and education variables, a separate multiple regression analysis was performed using just these variables together with place of employment. The results of this analysis are shown in Table 3. These variables accounted for 48% of the variance in publication for women ($R = .70$)

TABLE 2

Correlations between Number of Journal Publications and Related Variables

Variable	Zero-order r	
	Females[a]	Males[b]
Highest degree	.44*	.33*
Time spent guiding research	.50***	.42**
Time spent writing	.37**	.65***
Time spent on research	.37**	.59***
No. graduate students advised	.35**	.56***
No. grants received	.32**	.51***
No. grant applications	.32**	.49***
No. oral presentations	.26*	.58***
No. graduate courses taught	.41***	.24
No. professional/scientific societies	.25*	.32*
Date degree was received	.25*	.22
Time spent consulting	−.21	−.07
Age	.10	−.07
No. scientific meetings	−.09	.18
Time spent studying	−.08	−.20

[a] Total regression coefficient for females: $R = .80$, $R^2 = .64$.
[b] Total regression coefficient for males: $R = .90$, $R^2 = .81$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

TABLE 3

Effect of Demographic and Education Variables and Place of Employment on Prediction of Publication

Variable	Zero-order r	
	Females[a]	Males[b]
Where employed	.46***	.41**
Highest degree	.41[c]***	.33*
Date of degree	.25*	.22
Marital status	.10[c]	.09
Academic honors	.27*	−.12

[a] Total regression coefficient for females: $R = .70$, $R^2 = .48$.
[b] Total regression coefficient for males: $R = .64$, $R^2 = .40$.
[c] These variables contributed significantly to the regression equation.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

and 40% for men ($R = .64$). Marital status contributed significantly to the multiple regression equation for women, in that single women were more likely to publish than married women, although the zero-order correlation for marital status was not significant. Marital status did not contribute to the regression equation for men since men were so homogeneous on this variable.

The number of academic honors received correlated significantly with number of journal publications for women, but not for men. This difference in correlation was significant. For women, even with highest degree partialed out, the remaining correlation between honors and publication is .25, which is significant ($t = 2.064$, $df = 62$, $p < .05$). These results are compatible with those reported by Simon et al. (1967), although the measure used was different. They compared the number of fellowships received and membership in honor societies, and found that in the social science area, 8% of the women, compared to 4% of the men, had received at least one fellowship, while 35% of the women and only 14% of the men were members of an honorary society. A motivational factor may be responsible for these results: Only the most highly motivated women would achieve honors or publish, which in turn may have promoted greater self-confidence to develop their careers and publish more, despite role conflicts and discrimination.

Conclusion

Men published significantly more than women. However, the large difference in education between women and men accounted for most of the differences in publication. Because education was the crucial variable, the importance of developing new approaches to graduate education in psychology to encourage the full participation of women becomes even more apparent. The institution of employment and one's position in that institution were also shown to be of great importance in predicting publication in this study. Thus, discrimination against women in academe, which forces them into less favorable employment, reduces their opportunities for scholarly accomplishment, which in turn makes them less able to compete for more desirable positions.

Those women who managed to achieve the same education and employment as men also published as much as their male counterparts, despite the difficulties of role conflicts, lack of professional socialization or sponsors, and inadequate access to informal communication discussed by White (1970).

The possibility exists that differences in publishing were due to simple discrimination by journal editors (predominantly male) against female authors. Outright rejection of a manuscript because a female name appears on it is difficult to imagine. However, more subtle, unconscious discrimination may occur in that women may choose topics and/or present their research in ways that men may consider unimportant and/or inappropriate. Sociologist Pauline Bart (undated) found that most of her male colleagues disapproved of her choice of depression in middle-aged women as a topic of research. She also had great difficulty in publishing the results of her research. To the extent that women in psychology, for whatever reasons, do publish less than men, our discipline suffers the loss of their full potential contribution.

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(*Case Study continued on page 533*)

Women as Clinicians in Private Practice

VIRGINIA E. PENDERGRASS *Miami, Florida* [1]

Studies of the role of women in the professions report discriminatory practices in graduate education (Astin, 1972; Women's Bureau, 1969); perception of professional competence (Pheterson, Kiesler, & Goldberg, 1971); and employment in universities (Fidell, 1970). Female psychotherapists, however, might be expected to hold a preferred status, because helpfulness, nurturance, warmth, and concern, traditionally attributed to females (Kagan, 1964), would be assets.

In this survey, psychologists in major southeastern cities were polled concerning personal characteristics and private practice activities. The objective was to describe clinicians in private practice, with special emphasis on sex differences.

Method

SURVEY POPULATION

Questionnaires were mailed to all psychologists listed in the yellow pages of cities in states represented in the Southeastern Psychological Association (SEPA), unless the listing clearly indicated nonclinical status. Thirty listings were identifiable as female, and 157 were identifiable as male. Sixty were unidentifiable as to sex, but were assumed to be distributed in proportion to the identifiable sample. The estimated total number of females contacted, therefore, was 40 (16.2%) and the estimated number of males was 207 (83.8%). This percentage of females approximates the percentage of PhDs in psychology awarded to women in 1960 (Astin, 1972). The private practice of psychology, therefore, did not appear to be a "woman's profession," in that inordinate numbers of women are not attracted to it. Of questionnaires mailed, 44.9% were returned, with approximately equal proportions of females and males responding.

QUESTIONNAIRES

Questionnaires asked for the following data:

1. Personal data, including age, sex, extent of practice, degree, publications, awards, and honors;
2. Characteristics of practice, such as extent of practice, income, sources or referrals, type of clientele, motivation for entering practice, and original career goals. Only responses most often ranked first are reported.

[1] More information may be obtained from Virginia E. Pendergrass, No. 111, 1950 Brickell Avenue, Miami, Florida 33129.

Results and Discussion

EXTENT OF PRIVATE PRACTICE

Fifty percent of the women and 73.4% of the men were in full-time practice. Men are, thus, more heavily involved in private practice as a profession than women. It has been found that competitive, aggressive aspects of work may be feared or rejected by women (Horner, 1969) and these aspects of private practice may be unappealing to women. Another explanation of this finding is that the greater responsibility women may have for children and home may limit career commitment.

Since the sample of women was small, and since women in part-time practice represented a significant part of the sample, part-time and full-time practitioners were pooled for further comparisons.

PERSONAL CHARACTERISTICS AND ACHIEVEMENTS

Average ages for women and men ($M = 45.9$ and 44.9, respectively) and length of time in practice ($M = 10.37$ years for men, $M = 11.44$ years for women) were similar. All but one woman and two men had PhDs. Approximately equal proportions of women and men had received achievement awards (25.0% of women and 20.4% of men) and 18% of both groups had Diplomate status. Women had an average of 2.63 and men 5.76 publications, a difference attributable to a few men in academic or industrial settings who produced large numbers of publications.

AVERAGE INCOME LEVEL

Table 1 shows that the differences in reported earnings for women and men in the first years were small, favoring men, and that men showed consistent economic growth while women did not. Overall average incomes were \$10,000–\$15,000 (mean rank = 3.5) for women and \$15,000–\$25,000 (mean rank = 4.7) for men. No woman made more than \$50,000, while 12 men did. These differences may be due to women therapists holding part-time jobs in conjunction with part-time practice, which may be low paying and offer few promo-

TABLE 1

Average Rank of Income for Female and Male Therapists by Years in Practice

Sex	Years in practice			
	0–3	4–10	11+	Overall
Females (N = 16)	3.4	4.0	3.2	3.5
Males (N = 94)	3.9	4.6	5.1	4.7

Note. Categories, in dollar amounts, are 1–1.9 = under $8,000; 2–2.9 = $8,000–$10,000; 3–3.9 = $10,000–$15,000; 4–4.9 = $15,000–$25,000; 5–5.9 = $25,000–$50,000.

tions, or it may be that, when women experience difficulties in building clientele and sources of referrals over many years, they become less competitive and adjust their performance and fees downward. Finally, it is possible that women attach less importance to income than men and engage in activities in their practice that do not pay off economically. Brodsky's finding (see pp. 523–526, this issue) that male students checked income potential as a reason for entering psychology more often than females supports this hypothesis. However, involuntary salary limitations on women would demonstrate significant discrimination.

SOURCES OF REFERRALS

Both females and males received most referrals from medical or osteopathic physicians (46.7% and 44.6%, respectively), former clients (20.0% and 27.3%), and government agencies (13.3% and 3.0%). Since women received referrals from the same sources as men, and since both listed medical and osteopathic physicians as primary (a profession dominated by men), women do not appear to suffer discrimination from other professionals.

TABLE 2

Percentages of Female and Male Respondents Ranking Types of Clientele as Primary Clientele

Type of clientele	Sex	
	% female (N = 16)	% male (N = 87)
Individual females	37.5	41.4
Children	6.3	20.7
Individual males	.0	12.6
Marital counseling	18.8	10.3
Family therapy	18.3	2.3

TABLE 3

Percentages of Female and Male Respondents Ranking Career Goals as Primary Original Goal

Career goal	Sex	
	% female (N = 16)	% male (N = 94)
Clinical, private practice	25.0	28.7
Clinical, institution	12.5	20.4
Academic, emphasis on teaching	31.3	17.0

PRIVATE PRACTICE CLIENTELE

Both sexes reported seeing individual adult females most often as clients (see Table 2). Males more frequently saw children or individual adult males, while women therapists more often saw couples or families. The expected stereotype of women therapists seeing children primarily was not found. Perhaps the role of the therapist in her/his own home may lead the therapist to diagnosis disorder in children differently. Women therapists may seek explanations of children's difficulties in total family interactions or in marital strife, while male therapists may deal with the child as an individual. Or parents may go to women therapists for sympathetic arbitration of marital and family difficulties, but take individual problem children to male therapists for discipline.

Only male therapists listed adult males as primary clientele. Unfortunately, if female clients

TABLE 4

Percentages of Female and Male Respondents Ranking Motives as Primary Motivation for Entering Private Practice

Motive	Sex	
	% female (N = 16)	% male (N = 93)
Freedom of hours	31.3	4.3
Do best work in this area	18.8	30.1
Could not get other attractive work	12.5	3.2
Service	12.5	7.5
High income	6.2	21.5
Do not like to work for others	6.2	14.0

also preferred therapists of the same sex, such a preference would remain unexpressed because there are so few female therapists.

ORIGINAL CAREER GOAL

Table 3 shows the items most frequently ranked first as an original career goal. Males ranked private practice in clinical psychology and clinical practice in an institution most often, and an academic career emphasizing teaching next. Most women, however, listed academic career with an emphasis on teaching as their original career choice, followed by private clinical practice and clinical work in an institution. The fact that large percentages of both sexes selected choices other than private practice as an original career goal could be because many clinicians either enter private practice when they are blocked in other endeavors, or private practice as an appealing profession is not apparent to the graduating student and is discovered later on.

MOTIVE FOR ENTERING PRIVATE PRACTICE

The item ranked first most often by males as a reason for entering private practice was ability to do best work in this area, while females chose private practice because of freedom of hours (see Table 4). This discrepancy may reflect women's larger home responsibilities, or women may not feel the same commitment to private practice as men. The latter fits with the finding that private practice was not an original career choice for many women. Males gave high income potential as the second most frequent response, and dislike of working for others was third. For women, other important considerations were doing best work in this area, unavailability of other attractive work, and desire to be of service.

Emphasis by males in this study on skill, achievement, and independence conforms to the traditional masculine stereotype (Kagan, 1964). Involvement of females in clinical practice appeared here to be due mainly to default, because they mentioned "freedom of hours" and "could not find other attractive work" as major reasons for going into private practice. Women, unlike men, also ranked high the desire to be of service, which fits with the traditional female role.

Conclusion

Relatively few women were found to be in the private practice of psychology in cities in the Southeast. Data from the few who responded revealed several differences between them and their male colleagues, although these differences may not be reliable due to the small *N*. Women less often than men practiced full time, published, or showed consistent economic growth. Women devoted a greater proportion of their time than men to marital and family counseling, and less to working with children. No woman therapist reported adult males as primary clientele. Fewer women than men reported private practice as their original career goal. Finally, women seemed to be motivated to enter private practice because it afforded flexible working hours and a chance to be of service and because of lack of opportunity to find other attractive work. In contrast, men appeared to engage in private practice because it was the area in which they did their best work, had a high income potential, and afforded them independence. The sexes were alike in length of time in practice, training, awards, Diplomate status, sources of referrals, serving a large number of adult female clients, and, finally, in not holding private practice as their original career goal. The expectation that the female therapist might hold a preferred status in this area of psychology was *not* supported in this study.

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(*Case Study continued on page 536*)

Women as Job Changers

ELLEN KIMMEL *University of South Florida* [1]

The purpose of the study reported here was to determine whether the statement "Women are a bad risk" is fact or myth with respect to women psychologists. One of the most frequent reasons for not recruiting or hiring women in general is that they are less likely than men to remain in their positions (Keyserling, 1968; U.S. Department of Labor, 1969). Yet studies comparing the mobility of women and men in many types of occupations yield conflicting results, the majority showing small differences. For professional workers, job-changing rates are relatively low, with some indication that the rates are somewhat higher for men than women (U.S. Department of Labor, 1969, 1972). The present survey focused on frequency of job change for men and women in psychology.

Method

DESCRIPTION OF SAMPLE

The sample consisted of 150 female and 150 male members of the Southeastern Psychological Association (SEPA) taken randomly from the 242 females and 1,076 males on the 1972 membership list.

SURVEY INSTRUMENT

The one-page questionnaire asked for (*a*) date of PhD or terminal degree, (*b*) area of specialization, and (*c*) a chronological listing of all jobs held since the terminal degree. Space was provided for all reasons for each change in employment. A covering letter on official SEPA stationary signed by the president asked each participant to provide the information requested. Neither the sex nor the name of the respondent was requested, the former because of the fear that individuals would be sensitized on the sex issue, and the latter to encourage honest and complete answers as to why they changed their jobs. Females were sent forms with elite type and males were sent forms with pica type.

DISTRIBUTION OF SURVEY

The survey was mailed out in the first week of November 1972. Because the response was good, it was not necessary to send a follow-up letter. Seventy-two females and 74 males responded.

Results and Discussion

Table 1 summarizes the primary data for males and females. The women's data are further broken down by terminal degree. This was not done for men because all but two had earned the doctorate. The areas of specialization within psychology were categorized into two general headings, "basic" or "applied." As might be expected, a greater number of the women (51, or 71%) received their training in an applied area than men (44, or 60%). There were no discernible differences between the sexes in areas of speciality within basic or applied fields. Table 1 also shows that women reported a total of 189 positions and men a total of 179. Jobs were categorized into three major types: private and public agencies (e.g., community mental health institutions, public school systems, or private corporations); self-employment (e.g., clinicians in private practice or industrial psychologists); and academic institutions (public or private universities, colleges or community colleges). Eighty-nine (47%) of the jobs held by women were with agencies, while 54 (30%) of the men's jobs were with agencies. Self-employment was negligible. Ninety-two (48%) of the females' jobs and 122 (68%) of the males' jobs were in academic institutions. Thus, the women were more often employed in agencies or self-employed than the men, especially women with MAs. The types of academic institutions in which women and men worked are also shown in Table 1. There were significant differences ($\chi^2 = 10.18$, $p < .02$) between the sexes, in that men more often had positions in universities than women, and women more often had jobs in small community colleges. This corroborates Joesting's finding (see pp. 520–523, this issue).

A mobility score was computed for each respondent by dividing the total number of jobs listed into the total number of years since the terminal degree. This results in a measure of job tenure in years per job. The scores ranged from .2 to 25.0. Table 1 shows that the females had a mean of 4.3 and the males a mean of 4.4 years per job. This difference was not significant ($t \cong 0$, $df = 144$). Women with MAs had larger average mobility

[1] More information may be obtained from Ellen Kimmel, University of South Florida, FAO 295, Tampa, Florida 33620.

TABLE 1

Demographic and Mobility Data on Female and Male Psychologists

Questionnaire item	Females						Males	
	MA (N = 16)		PhD (N = 56)		Total (N = 72)		Total (N = 74)	
	n	%	n	%	n	%	n	%
Total no. jobs held	43		146		189		179	
Type employer								
Agency	31	67	58	38	89	47	54	30
Self	2	4	6	4	8	5	2	2
Academic institution**	10	21	82	57	[92]	48	[122]	68
Private university					6		21	
State university					53		78	
Private college					25		17	
State college					4		3	
Community college					4		3	
Average mobility score in years—overall	5.2		4.1		4.3		4.4	
New degree: 9 years or less	—		—		2.6 (N = 45)		2.8 (N = 42)	
Old degree: 10 years or more	—		—		7.3 (N = 27)		6.6 (N = 32)	
Reasons given for change								
Professional advancement*	10	36	32	36	42	36	53	50
Job unsatisfactory	4	14	12	13	16	14	23	22
Job disappearance	5	18	12	13	17	15	11	10
Spouse related	3	11	15	17	18	15	1	1
Termination	2	7	7	8	9	8	7	6
Personal	4	14	8	9	12	10	4	3
Personality conflict	0	0	3	3	3	3	6	5

* $p < .05$.
** $p < .02$.

scores (were less mobile—5.2) than doctorates of either sex.

Table 2 compares the distributions of the actual number of jobs held by women and men. Although women held a greater number of jobs than men, the distributions of the two sexes did not differ ($\chi^2 < 1.0$). It is interesting to note that 18 females and 24 males were still in their first jobs, some for many years.

The question of whether mobility of psychologists is related to where they are employed required combining the data of both sexes, because the difference between the scores of women and men was small within each category of employment. Mobility of psychologists employed in agencies (3.4 years per job) was greater than that of persons in academic institutions (4.8 years).

Since mobility is known to be related to age (U.S. Department of Labor, 1969), the respondents were divided into groups within sex according to the age of terminal degree. This measure had to be used because the actual age of the respondent was unknown. Ten years was arbitrarily taken to be the breaking point for defining "new" versus "old" degrees. The average mobility score was

TABLE 2

Distribution of Number of Jobs Held by Females and Males

Number of jobs	Females	Males
1	18	24
2	25	24
3	10	12
4	8	8
5	5	2
6	6	2
7	—	1
8	—	—
9	—	—
10	—	1
Total	72	74

computed separately for females and males with degrees 9 years or less and 10 years or more. Forty-five females and 42 males were in the "new" degree group (refer back to Table 1), with mobility scores of 2.6 and 2.8 years, respectively. Twenty-seven females and 32 males were in the old group, with mobility scores of 7.3 and 6.6, respectively.

Additional interesting information was obtained, incidental to the main question of mobility, that related to patterns of career advancement. With respect to the highest position achieved in their careers, the most notable difference between women and men was in the number who attained the rank of professor—8 women as opposed to 27 men. This is in keeping with national figures which showed that while only 50% of the women with doctorates for 20 years achieved the rank of full professor, the figure is 90% for men (Astin, 1969). Also, fewer women (13) held the rank of assistant professor than men (23). Fewer women than men reported administrative positions in academic institutions. Since a greater number of women held nonacademic positions than men, it was pleasing, but not surprising, to discover that a greater number of women than men held administrative positions in agencies.

The 222 reasons for changing jobs were analyzed into seven categories, shown at the bottom of Table 1. (The number of reasons was smaller than the total number of jobs described, because the first job reported by each respondent did not involve a change.) The first category, Professional Advancement, contained any reason describing a move resulting in an increment in salary, rank, prestige of the institution, and/or improved opportunities for professional growth. Only 36% of the reasons women gave, but 50% of the reasons men gave, were placed here. This difference was significant ($\chi^2 = 4.78$, $df = 1$, $p < .05$). National surveys of mobility (e.g., Brown, 1967) have shown that the majority of faculty moves result in advancement of some sort in the careers of the job changers. Thus, the data for men, but not women, resembled that obtained in national surveys. The other six major types of reasons are given in Table 1 in order of frequency of occurrence. Under Job Unsatisfactory, such reasons as "bored stiff," lack of opportunity to teach, unhappiness with the nature of the work, etc., were included. More men than women gave these sorts of reasons. Job Disappearance referred to grants that ended, fellowships that expired, or adjunct monies being reduced. Here, more women than men were affected. Spouse-Related reasons included spouses changing jobs or disliking the area and consequently relocating. Such events forced the partner to change jobs. Eighteen women and only 1 man reported reasons of this sort. While this difference is quite large, 18 is only a small proportion (15%) of the 117 total reasons given by women for changing employment. The category Termination is self-explanatory, and no differences were apparent between women and men here. Personal reasons involved such things as poor health, dislike of climate or geographical area, divorce, or pregnancy. Ten percent of the females' and 3% of the males' reasons were placed in this category. The last category, Personality Conflict, is self-explanatory.

Women MAs were surprisingly similar to women PhDs in the reasons for changing employers. Women with better credentials, who were thus better able to change jobs to improve their status, did not report moving to advance professionally any more often than those presumably in a less favorable position to do so.

Conclusion

This study found that, despite some significant differences between women and men psychologists in the Southeast in training, type of employer, level of success achieved, and reasons for changing jobs, there was no significant difference in their job mobility. How then did the apparent myth that "women are a bad risk" start and what has maintained it? One possibility suggested by this study is that, though women are no more mobile than men, the reasons why they change jobs may be considered to be less valid, that is, less socially acceptable, thereby placing a kind of stigma on the move. The loss of a female psychologist from a department because of a spouse- or family-related event may lead to the development of a negative attitude about women as employees which would not occur from losing a male psychologist who moves to advance himself professionally. Thus, the same phenomenon (loss of an employee–colleague) is viewed in two quite different ways. Epstein's (1971) analysis of the obstacles women encounter as professionals supports this idea. To the extent that women deviate from the norm of what a "good" professional does (e.g., changes jobs for professional advancement), they are seen as less committed and otherwise negatively regarded by their male peers. Regardless of how the myth got started, what must be emphasized

at this point is that it was not supported by the data collected in this study. Chairs and directors in psychology may hire women with the same degree of confidence with which they hire men with respect to the expected time that women will remain in their jobs.

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SEPA's Affirmative Action

It may be concluded that women in psychology in the Southeast, except, in certain respects, women doctoral students, hold a "minority" status. In light of the evidence pointing to this conclusion, some affirmative action activities that are being conducted by SEPA and the Status of Women group are described here. Having studied the problem empirically, members of the Commission felt it was appropriate to act and asked to be reappointed and given a small budget in order to do so. First, the Commission asked the members of SEPA to endorse a formal resolution calling for the improvement of the status of women in the organization itself.[1]

Second, at the request of the Commission, SEPA's Executive Committee appointed Judith Worell to chair a committee to select the winner of a Student Research Paper Award ($100 prize). Students of either sex may compete. The topic of the paper must be related to the purpose of APA's Division 35 (Psychology of Women), namely, to "promote the research and study of women, including both the biological and sociocultural determinants of behavior and to encourage the integration of this information about women with the current psychology of knowledge and beliefs in order to apply the gained knowledge to the society and its institutions." Program space was reserved at the 1974 annual meeting of SEPA for the reading of the winning paper.

Third, the most ambitious project now underway is the Commission's Visiting Woman Program. The proposal for this was first introduced by its originator, LaMyra Davis, at SEPA's 1973 annual meeting. Brochures describing the program have been sent to approximately 500 psychology departments along with information copies to the presidents of their colleges or universities. The brochures contain information (names, addresses, titles of colloquia) about women psychologists who have vol-

[1] The resolution, which was passed by SEPA in April 1974, reads as follows:

Resolve that the Southeastern Psychological Association support affirmative action for women in psychology in the Southeast in the next year by making special efforts to recruit women members, to include women on committees and in positions of leadership in SEPA, and to nominate or otherwise promote women in the Southeast for positions of responsibility in psychology at the national level.

unteered (for expenses only) to serve as a woman visitor. Each department has been encouraged to participate by inviting at least one volunteer to its campus to deliver a colloquium address to students and faculty and to talk informally with women (only) students at some other time in the visit. The purpose of the program is to provide additional role models for women students, where few are currently available (see the Joesting and Kimmel articles this issue), who will encourage talented women to enter or continue in the pursuit of a career in the field of psychology. The Commission also hopes for an atmosphere effect within departments that "shows they care" by electing to engage in this affirmative action program. Such a spin-off benefit might elevate women's motivation and, consequently, performance.

Women who visit will collect data for the Commission and, along with students and department chairs, contribute to the evaluation of the short-term impact of the visit. A formal report on the Visiting Woman Program was presented to SEPA. Other anticipated benefits of the Visiting Woman Program include such things as boosts in the careers of the women who make the visit (few women currently receive invitations to deliver colloquia), stimulation of new ideas about psychology and/or affirmative action, and development of new relationships between the women faculty and students within the department which was visited.[2]

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[2] For additional information about the project, contact its coordinator, Dorothy Nevill, University of Florida, Gainesville, Florida 32605.

Research and Relevance

A Survey of Graduate Students and Faculty in Psychology

MARK W. LIPSEY *Claremont Graduate School* [1]

> The perspicacious listener can hear the more-than-faint rumblings of approaching breakthrough [McGuire, 1969, p. 38].
>
> A revolution of some sort is developing in psychology [Schultz, 1970, p. 394].
>
> There seems little doubt that experimental psychology is ripe for a revolution . . . [Palermo, 1971, p. 155].
>
> Psychology is in a state of crisis [Deese, 1972, p. 1].

Something is happening in psychology. This not especially original observation echoes the conclusions of such diverse psychologists as those cited above and others too numerous to mention. But in spite of the fact that many psychologists agree that *something* is happening, there is remarkably little evidence available to indicate just *what* is happening. Results of a survey of faculty and graduate students conducted during 1970–1971 by a group of graduate students at Johns Hopkins University make, we think, at least a modest contribution to understanding the changing face of contemporary psychology and provide some basis for speculating about its future. The items in that questionnaire dealt primarily with issues in psychology, graduate education, professional journals, and general communication. All of the full-time graduate students enrolled in a representative sample of 106 graduate departments of psychology received the survey, and it was returned by 2,340 graduate students [2] in 95 of the sampled departments—a return rate estimated at 50%.[3] A parallel form of the survey was also sent to a random sample of 500 faculty members in graduate departments selected from the APA *Directory*.[4] The faculty returned 368 (74%) of those questionnaires.

Issues in Psychology

The major issues in psychology could be established, at least in broad outline, from the responses to an open-ended item asking, "What do you feel is the single most important issue confronting psychology today?" The write-in responses to that item were coded into a number of general categories, each with finer subdivisions.[5] A summary of the results of that process appears in Table 1. Though considerable diversity was present, as might be expected from such a vague, open-ended item, three broad areas of concern stood out. These have been labeled *relevance, research and theory,* and *therapy and humanism.*

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[1] This article is a summary version of a considerably larger report (Lipsey, 1973) which is available from the Journal Supplement Abstract Service, APA.

The student survey was conducted under contract to the Office of Communications, APA, and supported by National Science Foundation Grants 772 and 901. Coding and analysis of the faculty survey were supported by National Institutes of Health Grant 1 RO3 MH19648-01 MSM.

Requests for reprints should be sent to Mark W. Lipsey, Department of Psychology, Claremont Graduate School, Claremont, California 91711.

[2] The number for the analyses reported here was 2,140; 200 questionnaires were omitted from those analyses.

[3] For further information regarding the study and its design, sampling, bias, etc., see SoGSiP Study Group (1971) and Lipsey (1973).

[4] The faculty survey was done by William Garvey of the Johns Hopkins University Center for Research in Scientific Communication. The faculty and student surveys were analyzed together to allow faculty–student comparisons.

[5] The original coder agreed with himself 80% of the time on the broad categorization and 60% of the time on the subdivisions when recoding a sample of the responses.

TABLE 1

The Most Important Issue Confronting Psychology

Response category	% naming issue		Mean rating of psychology's awareness of issue[a]		Mean rating of psychology's dealing with issue[b]	
	Students	Faculty	Students	Faculty	Students	Faculty
Relevance	50	47	3.6	3.3	5.4	5.3
General relevance	15	13				
Direct application	12	5				
Specific problems	11	9				
With qualification	5	12				
Identity; goals	7	8				
Research and theory	12	19	3.5	2.9	5.0	4.9
Understanding behavior	4	7				
Better theory	4	6				
Improved research	2	3				
Other	2	3				
Therapy and humanism	11	3	3.6	3.6	5.2	5.3
Individual fulfillment	3	1				
Mental health	3	1				
Ethics	2	1				
Other	3	1				
Other	27	31	3.7	3.6	5.4	5.1
Nontheoretical matters	7	10				
All other responses	10	13				
No response	10	8				

Note. N for students = 2,140; N for faculty = 368.
[a] Mean on a 7-point scale from well aware (1) to not aware (7).
[b] Mean on a 7-point scale from very well (1) to not at all (7).

RELEVANCE

About half of the graduate students and nearly the same proportion of faculty identified some aspect of relevance as the most important issue confronting contemporary psychology. Most of the responses cited relevance in a general way—relevance to social problems, the "real world," and so forth. Many, however, stressed not just relevance but the *direct* application of research findings; this group included 12% of the students, though only 5% of the faculty. Closely related was the number of students (11%) and faculty (9%) who listed the need to develop and apply psychological research to *specific* problems, for example, race, environment, militarism. In addition, a number of graduate students (5%) and even more faculty (12%) identified relevance as the most important issue, while explicitly withholding their own endorsement or otherwise expressing caution or apprehension over the cry for relevance. Feelings of concern and confusion about just what psychology should be doing were more broadly represented in the category labeled *identity; goals*, which included about 7% of the students and 8% of the faculty.

The questionnaire also included some yes–no items specifically dealing with academic psychology's role in the solution of social problems. Ninety-two percent of the students and 83% of the faculty responded, "Yes . . . academic psychology *should* be concerned with contemporary social problems." Ninety percent of the students, however, and 79% of the faculty said they did *not* think that academic psychology was making a significant contribution to the needed solutions. There was a rather striking gap between the "is" and the "ought" for academic psychology in the minds of graduate students and faculty. The fault could lie, we reasoned, either with academic psychology, which might not be perceived as producing useful knowledge, or with those in the society at large who were not making full use of the available knowledge. Fifty-one percent of the students and 52% of the faculty felt that "academic psychology does not yet have much knowledge relevant to social problems . . ." but, optimistically, "might develop such knowledge if it tried." Thirty-seven percent of the graduates and 25% of the faculty

TABLE 2

Factor Loadings for Thirteen "Issue" Items

Item	Factor 1 (social concern)		Factor 2 (experimentalism)	
	Student	Faculty	Student	Faculty
The knowledge and methods of experimental psychology have developed to the point where fundamental problems related to behavior are potentially solvable.	.09	.07	.48	.55
An increasing awareness of man's capacity for self-actualization could be psychology's greatest contribution to society.	.44	.51	−.19	−.20
Laboratory experimentation, however slow and painful, is still the most adequate tool psychology possesses for increasing its useful knowledge.	−.36	−.39	.50	.54
Psychology may be able to study value judgments, but it must be careful not to make them.	−.20	−.42	.06	.06
For all its efforts to study learning, psychology has not provided much useful knowledge to the classroom teacher.	.00	−.05	−.38	−.51
The operational definitions of the laboratory experiment are rarely related in a meaningful way to what happens in ordinary situations.	.19	.24	−.58	−.58
Psychology should be leading the search for new means of promoting human welfare.	.69	.74	−.01	−.08
Psychology needs to replace much laboratory research with careful studies of ordinary behavior in natural settings.	.42	.43	−.43	−.52
Psychologists study people as objects when they should be studying them as people.	.39	.59	−.47	−.43
Research psychologists should not be held responsible for the use society makes of their findings.	−.35	−.39	.10	.07
Psychological research mimics only the surface features of research in the physical sciences.	.10	.17	−.41	−.44
Psychologists have an obligation to demonstrate the worth of their endeavors to the society which supports them.	.36	.39	−.05	−.06
Psychologists may be obligated to communicate what they know, but they have no special obligation to solve social problems.	−.68	−.76	−.05	.05

Note. Minres factor analysis with varimax rotation. Five items were excluded from this table; they showed no relationship to the two major factors. Separate factor analyses were done on the student and on the faculty data.

thought the problem was that "academic psychology has produced much relevant knowledge, but it is not being fully applied to real world problems." Clearly, then, there was a great deal of dissatisfaction with what psychology was doing about social problems, and most students and faculty felt that the blame belonged within psychology itself.

RESEARCH AND THEORY

Responses judged as dealing with the general theme of research and theory were given with second-most frequency by both students (12%) and faculty (19%) as "the most important issue" confronting psychology. The most frequent type of comment identified psychology's central task as understanding behavior or human nature. Two other high-frequency responses in the research and theory category were the need for better theory, or a unifying theoretical structure, and the need for improved research methods and skills. There seemed to be in these responses a feeling that the appropriate action for psychology would be to get on with the research that would enable it to meet its original goal, the understanding of behavior, and, perhaps, a recognition that the goal must be reemphasized because psychology's performance to date has not been very dramatic.

THERAPY AND HUMANISM

Eleven percent of the graduate students, but only 3% of the faculty, named issues judged in the therapy and humanism category as the most important issue confronting psychology. The student–faculty difference was undoubtedly due to the greater proportion of graduate students interested in clinical psychology than graduate faculty. The students in this category most frequently mentioned contribution to individual fulfillment or, as one said, "how to provide the conditions for human development." The faculty who gave responses in the therapy and humanism category were most likely to express general concern for the human condition and psychology's conception of the nature of the human organism. Of about equal fre-

quency among students and faculty were responses focusing on the ethics of the research and the behavior of psychologists.

Experimentalism and Social Concern

In addition to the open-ended issue item, the survey questionnaire included a variety of statements regarding psychological methodology and social relevance to which students and faculty could respond on 7-point scales ranging from agree (1) to disagree (7). These items explored in a little more detail two of the major areas of concern that were identified in the open-ended responses of the most important issue confronting psychology. Those two issues, the discipline's appropriate role in the "promotion of human welfare" and the serviceability of the experimental tradition, were reflected in two of the factors that emerged from factor analysis of the items presented in Table 2.

Factor 1, labeled *social concern,* combined concern for promoting human welfare, a sense of special obligation to solve social problems, belief in man's capacity for self-fulfillment, need to demonstrate psychology's value to society, and a feeling of responsibility for the use society makes of psychological findings. Associated with Factor 2, labeled *experimentalism,* were an affirmation of confidence in the problem-solving potential of experimental psychology and laboratory methodology, support of operationism, satisfaction with psychology's past efforts to study learning, and disagreement that the discipline only mimics the physical sciences.

In order to examine more closely the students and faculty who took one or another of the four possible positions on these two issues, each person in the sample was assigned two factor scores [6]—one to represent his position on the social concern factor and another to represent his position on the experimentalism factor. Since 4 was the midpoint on the 7-point agree–disagree scale that accompanied each item, the factor score that would correspond to a uniform response of 4 on all of the items represented in the factor was designated as the neutral point on each factor. Using the two factor scores and the designated neutral points, each individual was classified into one of the four quadrants formed by the orthogonal factors.

It was possible to give a very general description of the inhabitants of each quadrant by examining the proportion of students and faculty in each, their predominant areas of interest in psychology, and their response to an item asking which person in psychology or a related area had produced the work they most respected. A special word needs to be said about this last item. The diversity of responses was so great that we found it convenient to collapse them into 12 categories of researchers who seemed to us to do more or less similar work. These 12 categories were thus inexact, but suggestive, we think, of different perspectives on psychological research. Figure 1 identifies the composition of each quadrant in terms of the general items.

In the pro-experimental, anti-social-concern quadrant there was a predominance of students and faculty with interests in such areas as comparative psychology, learning, and physiological psychology. They mentioned favorably the work of psychologists such as Estes, Pavlov, and Neal Miller. The attitudes that put them in that quadrant, of course, were supportive of experimental psychology and reflected the "basic" scientist's unconcern for immediately applicable research results—a pattern that marked them as basic researchers—and we have applied that label to their quadrant. These basic researchers comprised the second largest quadrant among faculty members with 28%, but among the students it was the least popular position; and the difference between the student proportion and the faculty proportion was larger there than for any other quadrant.

The mirror images of the basic researchers, in terms of their attitudes, were the anti-experimental, pro-social-concern citizens of the second quadrant. That quadrant was composed primarily of clinical psychologists along with some social-personality and applied psychologists. Their strong pro-social-concern stance and their heavy citation of such psychologists as Carl Rogers, as well as Freud and Allport, have led us to label this quadrant *humanist-activists.* The second largest discrepancy between proportion of faculty and proportion of students in a quadrant occurred here, but this time the direction was reversed. Students were nearly twice as likely as faculty to hold the attitudes of the humanist-activists, and that remained true even when the predominant clinical psychologists were separated out.

[6] The factor scores were calculated by Bartlett's procedure. The factor analysis of student and faculty responses agreed so closely that it was possible to use the same set of weights for calculating both the student and the faculty factor scores.

PRO-SOCIAL CONCERN

	Humanist-activists		Social engineers	
	Faculty	Students	Faculty	Students
% of total	18%	35%	34%	43%
% without clinical	15%	31%	32%	43%
Areas over-represented	Clinical 35% Applied 35% Educational 33% Comparative 33%	Personality 56% Clinical 45% Social 45%	Educational 50% Developmental 50% Personality 45% Clinical 43%	Applied 60%
Psychologists heavily cited	Rogers 60% Freud 57% Festinger 31% Allport 26%	Rogers 59% Freud 54% Allport 52% Piaget 52%	Allport 58% Skinner 46% Freud 43%	Skinner 58%

PRO-EXPERIMENTAL

ANTI-EXPERIMENTAL

	Mugs		Basic researchers	
	Faculty	Students	Faculty	Students
% of total	20%	12%	28%	9%
% without clinical	21%	14%	32%	12%
Areas over-represented	Quantitative 47% Perception 30% Cognitive 28% Physiological 26%	Cognitive 24% Perception 23% Comparative 22% Quantitative 21% Physiological 20%	Comparative 50% Learning 48% Physiological 43% Perception 40% Cognitive 38% Quantitative 35%	Physiological 22% Comparative 20% Learning 16% Experimental 16% Perception 15%
Psychologists heavily cited	Meehl 50% G. Miller 40% Piaget 33% N. Miller 27%	N. Miller 21% G. Miller 18% Festinger 18%	Estes 62% Pavlov 57% N. Miller 40% Festinger 38% Gibson 36%	Estes 30% N. Miller 22% Pavlov 21% Skinner 17%

ANTI-SOCIAL CONCERN

Figure 1. Size and general composition of the factor quadrants. (The relationship between each of the items in this table and the four quadrants was statistically significant at the .05 level at least by chi-square tests. The percentages refer to the proportion of the category who are in each quadrant; for example, clinical [35%] indicates that 35% of all the clinicians were in that quadrant; Rogers [60%] indicates that 60% of all those who cited a name in the Rogers category were in that quadrant. Nothing was listed as distinctive of a quadrant unless the percentage of respondents exceeded the total percentage in the quadrant by at least one fourth again.)

The largest quadrant for both faculty and graduate students was that representing pro-experimental and pro-social-concern attitudes. Because of their defining attitudes, the overrepresentation of those with applied interests (educational-developmental, general applied), and especially because of the heavy mention of B. F. Skinner as the psychologist of choice,[7] this quadrant has been labeled *social engineers*. Even without the clinical psychologist, this quadrant was still the largest among students, and, among faculty, as large as its nearest competitor, basic researchers.

Those in the final quadrant combined anti-experimentalism with anti-social-concern in a double negative. Their negativism contrasted sharply with the comparative zealousness of those in the other quadrants, all of whom were "pro" about something. We have, therefore, named them *Mugs* after

[7] Skinner alone received about 13% of both the student and faculty responses to the most-respected-person item, outdistancing his nearest competitors by margins of two to one or more. Half or more of all the responses in the Skinner category came from those in the social engineers quadrant.

TABLE 3

Response to Issues by Those in Each of the Factor Quadrants

Response	Pro-social concern				Anti-social concern			
	Anti-experimentalism		Pro-experimentalism		Anti-experimentalism		Pro-experimentalism	
	Humanist-activists		Social engineers		Mugs		Basic researchers	
	% faculty	% students	% faculty	% students	% faculty	% students	% faculty	% students
Most important issue								
Relevance	49	45	41	50	17	19	11	20
Caution about relevance	10	6	10	4	9	8	23	8
Identity; goals	6	6	3	6	21	16	9	16
Research and theory	11	12	18	10	29	22	23	23
Therapy and humanism	6	15	4	12	3	8	1	7
Psychology is[a]								
Aware of issue	13	28	45	29	39	29	52	46
Not aware of issue	25	24	14	17	20	25	11	12
Dealing with issue	0	1	2	3	2	3	4	5
Not dealing with issue	70	60	39	45	59	59	36	39
Psychology should be concerned with social problems.	99	99	98	99	69	74	67	78
Psychology is now contributing to their solution.	3	3	27	11	14	6	25	17
Not contributing; lacks only application of findings.	16	31	40	47	8	20	25	37
Not contributing; lacks relevant findings.	79	63	34	41	69	67	45	43
Not contributing; problems are impossibly complex.	1	3	0	1	8	7	6	3

Note. The relationship between each of the items in this table and the four quadrants was statistically significant at the .05 level at least using chi-square tests. The percentages in each quadrant refer to the proportion of those in that quadrant giving a particular response.
[a] Percentage marking the extremes (1 and 2 or 6 and 7) on 7-point rating scales.

the "scientific conservationists" described by Nigel Calder (1969) in *Technopolis* as "disinclined to believe anything." This quadrant was the next to smallest among both faculty and students, though it contained a greater proportion of faculty than of students. Those faculty and students interested in perception, cognition, and quantitative psychology were overrepresented in this quadrant, and they frequently mentioned psychologists such as George Miller, Piaget, and Meehl as important contributors.

ISSUES

The position of each group on the major issues in psychology, of course, was pretty much given by the factor definition of their quadrant—pro- or anti-social-concern, pro- or anti-experimentalism. Table 3, however, shows that more information about these positions may be gained by examining the responses to the other issue-oriented items.

Among faculty, social relevance was identified as the most important issue confronting psychology by fewer basic researchers than any other subgroup, and the proportion was next to smallest for the student basic researchers. The basic researchers were also most likely to mention apprehension over demands for relevance or urge that a balance be preserved between basic and "relevant" research. The most frequent citation of relevance among the faculty came from the humanist-activists; among the students, from the social engineers.

A sizable majority of those in every quadrant felt that "academic psychology should be concerned with contemporary social problems," though fewer of those in the anti-social-concern groups felt that concern with social problems was legitimate. The majority of those in each quadrant, however, also believed that academic psychology was *not* contributing to the solution of contemporary social problems. Most of the Mugs and humanist-activists felt that the reason for this failure was that "academic psychology does not yet have much knowledge relevant to social problems," and the largest proportion of basic researchers agreed. The

TABLE 4

Response to Graduate Training Items by Those in Each of the Factor Quadrants

Response	Pro-social concern				Anti-social concern			
	Anti-experimentalism		Pro-experimentalism		Anti-experimentalism		Pro-experimentalism	
	Humanist-activists		Social engineers		Mugs		Basic researchers	
	Faculty	Students	Faculty	Students	Faculty	Students	Faculty	Students
Mean satisfaction with graduate school	—	4.02	—	3.50	—	3.57	—	3.18
Very pleased	—	24%	—	33%	—	32%	—	42%
Very disappointed	—	25%	—	15%	—	15%	—	10%
Type of graduate department[a]								
Self-understanding								
Desirable	67%	79%	56%	65%	30%	44%	18%	28%
Undesirable	8%	3%	7%	8%	19%	16%	24%	22%
Professional training								
Desirable	37%	64%	48%	70%	90%	84%	95%	82%
Undesirable	7%	3%	3%	3%	10%	5%	19%	4%
Broad education								
Desirable	72%	74%	87%	80%	90%	84%	95%	82%
Undesirable	4%	1%	2%	1%	0%	1%	0%	2%
Research institution								
Desirable	49%	43%	64%	59%	72%	64%	76%	76%
Undesirable	12%	9%	2%	5%	6%	5%	1%	3%
Social problems								
Desirable	52%	61%	42%	62%	14%	27%	12%	26%
Undesirable	5%	5%	11%	4%	30%	27%	27%	21%
Student plans after degree								
Academic	—	52%	—	62%	—	71%	—	82%
Teaching	—	13%	—	14%	—	11%	—	12%
Research	—	10%	—	16%	—	20%	—	28%
With clinical	—	14%	—	12%	—	8%	—	8%
Clinical/counseling	—	30%	—	19%	—	12%	—	5%

Note. The relationship between each of the items in this table and the four quadrants was statistically significant at the .05 level at least using chi-square tests. The percentages in each quadrant refer to the proportion of those in that quadrant giving a particular response.

[a] These percentages refer to those marking the extremes on a 7-point scale: 1 or 2 for desirable, 6 or 7 for undesirable.

largest number of social engineers, however, believed that "academic psychology *has* produced much relevant knowledge, but it is not being fully applied to 'real world' problems." And a larger number of the social engineering faculty than in any other quadrant thought that academic psychology already was "contributing significantly" to the solution of social problems.

Not all mention of significant issues in contemporary psychology, of course, emphasized social relevance. In fact, the most frequently mentioned issues among Mugs and basic researchers dealt with aspects of psychological research and theory. Such essentially scientific concerns, however, did not indicate that the Mugs and basic researchers were untouched by the difficulties the discipline is experiencing. Each of these groups identified more issues in the category involving the identity and goals of the discipline than the humanist-activists and social engineers combined.

GRADUATE TRAINING

Attitudes toward social concern and experimentalism were also related to the graduate student's satisfaction with his department, his career plans, and both the student and faculty description of the ideal graduate department, as is shown in Table 4.

The most frequently offered graduate degree, according to the APA publication *Graduate Study in Psychology: 1972–73,* is in experimental psychology. If the typical department is organized around the needs and values of the basic researcher in psychology, we might expect the student basic researchers to be the most satisfied with their graduate training. That was indeed the case. The students were asked to rate their reaction to graduate

TABLE 5

Response to Research and Publication Items by Those in Each of the Factor Quadrants

Item	Pro-social concern				Anti-social concern			
	Anti-experimentalism		Pro-experimentalism		Anti-experimentalism		Pro-experimentalism	
	Humanist-activists		Social engineers		Mugs		Basic researchers	
	Faculty	Students	Faculty	Students	Faculty	Students	Faculty	Students
Doing research	92%	82%	95%	86%	93%	90%	100%	92%
Mean research satisfaction	3.26	3.77	3.03	3.24	3.23	3.46	2.60	2.79
Very satisfied	46%	34%	47%	47%	47%	40%	60%	61%
Very dissatisfied	18%	25%	10%	18%	17%	19%	4%	12%
Average number of journal publications	10.22	.52	12.50	.55	15.41	.76	15.21	.97
Those reporting a rejection[a]	79%	11%	83%	13%	82%	12%	84%	15%
Average number of rejections, if any[a]	2.59	1.29	2.81	1.29	2.76	1.19	2.77	1.36
Those attempting to have a theoretical article published	40%	3%	46%	3%	49%	4%	42%	5%
Those successful if theoretical article attempted	82%	61%	93%	48%	66%	50%	88%	56%

Note. Except as noted, the relationship between each of the items in this table and the four quadrants was statistically significant at the .05 level at least using chi-square tests. The percentages in each quadrant refer to the proportion of those in that quadrant giving a particular response.
[a] The responses to these items were not related to the factor quadrants at a significant level.

school relative to their expectations on a 7-point scale from extremely pleased (1) to big disappointment (7). The mean rating indicated greater satisfaction for students in the basic research quadrant than for students in any other quadrant. The humanist-activist students, most discrepant from the values of the basic researchers, were the least happy with their graduate school experience. The Mug and social engineering students, each agreeing with the basic researchers on one factor and disagreeing on the other, showed similar, intermediate levels of satisfaction with their graduate departments.

Differences also emerged in the type of graduate department thought ideal by the students and faculty in each quadrant. Various graduate school descriptions were rated on a 7-point scale, and we have translated the results into the percentage who gave a very desirable rating (1 or 2) and the percentage who gave a very undesirable rating (6 or 7) in order to facilitate comparison. All groups except the humanist-activist students found as most desirable a graduate department composed of "teachers and students involved in scholarly investigation and broad education in their field of interest" with the greatest endorsement coming from the faculty basic researchers. The humanist-activist students thought the most desirable department was "a place for students to gain a deeper understanding of themselves and of their fellow man," and a large number of their faculty also found that description highly attractive.

RESEARCH AND PUBLICATION

The overwhelming majority of both students and faculty in all the quadrants reported that they had been doing research (see Table 5). Though the proportion who were doing research did not vary as much as 10% from quadrant to quadrant, the proportion who were personally satisfied with their own research varied much more widely. A majority of the basic researchers who were engaged in research reported being very satisfied with it and few reported great dissatisfaction. The humanist-activists, on the other hand, were considerably less pleased with their research progress: Fewer than half reported great satisfaction and more expressed great dissatisfaction than in any other quadrant. The Mugs, who, unlike many of the humanist-activists, brought a basic science orientation to their research, were surprisingly similar to the humanist-activists in their relatively moderate research satisfaction. The social engineers, though more satisfied than the Mugs, resembled them more than they did the basic researchers. Apparently, the difficulties and frustrations of academic research increase the more remote the researcher is from the attitudes and values of the basic researchers.

TABLE 6

Response to Journal/Communication Items by Those in Each of the Factor Quadrants

Item	Pro-social concern				Anti-social concern			
	Anti-experimentalism		Pro-experimentalism		Anti-experimentalism		Pro-experimentalism	
	Humanist-activists		Social engineers		Mugs		Basic researchers	
	Faculty	Students	Faculty	Students	Faculty	Students	Faculty	Students
Sources of "valuable ideas"								
Journals	69%	49%	82%	67%	78%	62%	91%	79%
Periodicals	31%	21%	10%	13%	15%	9%	8%	5%
Fiction books	—	15%	—	8%	—	5%	—	3%
Texts	—	26%	—	35%	—	29%	—	25%
Nonfiction books	21%	32%	9%	15%	19%	22%	9%	11%
Graduates	—	50%	—	42%	—	45%	—	34%
Courses	—	33%	—	39%	—	29%	—	38%
Mean satisfaction with journal editorial policy	4.50	4.36	3.56	3.55	4.06	3.78	3.20	3.37
Very satisfied	19%	15%	27%	28%	25%	25%	40%	35%
Very dissatisfied	34%	31%	15%	12%	25%	14%	9%	13%
Mean interest in journal reading	4.26	4.34	3.25	3.45	4.06	3.78	3.20	3.37
Very interesting	21%	19%	39%	35%	32%	29%	49%	45%
Very dull	31%	34%	13%	14%	21%	22%	8%	10%
Suggestions for improving the journals								
Organization/writing	31%	28%	39%	31%	30%	29%	28%	35%
Editing	10%	16%	14%	20%	33%	20%	37%	26%
Few biases/more unorthodoxy	39%	27%	26%	17%	18%	16%	9%	6%

Note. The relationship between each of the items in this table and the four quadrants was statistically significant at the .05 level at least using chi-square tests. The percentages in each quadrant refer to the proportion of those in that quadrant giving a particular response.

Academic research usually results in the publication of journal articles, so we also took a look at the lifetime publication experience of those in the different quadrants. The basic researchers might be expected to be the most frequently published group and that was true for student basic researchers. The basic research faculty, however, averaged slightly fewer journal publications per person than the Mug faculty. Humanist-activists, as might be anticipated given their clinical orientation, were the least published group, and the social engineers were more similar to them than to Mugs or basic researchers. There were fewer persons in some of these quadrants who were engaged in research than in others, of course, but adjusting the publication frequency according to the number of active researchers did not alter the rank order of the results.

There were no significant differences among the quadrants in the percentage who reported that they had had at least one manuscript rejected by a journal nor in the average number of rejections reported by those with rejections. Because the publication frequency varied, however, these rejection figures indicated that on a per publication basis humanist-activists and social engineers were somewhat more likely to have their manuscripts rejected by the journals, while Mugs and basic researchers were less likely to have this happen.

Respondents were also asked if they had ever attempted to publish a "theoretical article." There were no significant differences among the quadrants in the number reporting that they had attempted a theoretical article, but there were marked differences in the success they reported. Of those attempting such articles, the social engineering and basic research faculty were the most successful, with the humanist-activist faculty not too far off their pace. The Mugs, however, were notably less successful, a rather strange finding considering that their overall publication record was the best of the four groups.

JOURNALS AND COMMUNICATION

Attitudes on the social concern and experimentalism issues were also strongly related to utilization

of the various communication modes in psychology and to satisfaction with the professional journals, as Table 6 shows. Faculty and graduate students were asked to check from a list of information sources those from which they received "valuable ideas" relevant to their work and interests. Those in each of the four quadrants showed a different pattern of dependence on the various information sources. The professional journals were the most frequently cited by all groups except humanist-activist students, but the response was greatest for social engineers and basic researchers. In place of the journals, many Mugs and humanist-activists relied for useful information on nonprofessional periodicals and nonfiction books other than texts; they cited these sources about twice as frequently as their pro-experimental counterparts.

Many of the most prestigious of the professional journals, of course, are directed toward the basic researcher. It was not surprising, therefore, that satisfaction with the journals was highest among basic researchers and least among those most different from basic researchers. On an item requesting a rating of satisfaction with the "editorial selection and contents of the journals," basic researchers were found to be the most satisfied, humanist-activists were the least satisfied, and Mugs and social engineers were in between.

We can understand each group's perception of the professional journals better by examining the write-in responses they made when asked to suggest a way of improving the journals. We have collapsed these write-in responses into three large, general categories: (*a*) organization/writing, (*b*) tighter editing, and (*c*) fewer biases/more unorthodoxy. Most of the suggestions for improving the journals fell into the organization/writing category, citing, for the most part, the need for better or clearer writing and, especially, for more interesting writing. The Mugs and basic researchers, however, were particularly desirous of more restrictive editing, though about twice as many Mugs came out for the opposite—more tolerant editing allowing more unorthodox articles. Humanist-activists and social engineers were also disproportionately interested in looser editorial criteria, especially those that would allow publication of more socially relevant and applied research.

Faculty to Student Trends

We have seen that psychologists are divided over the issues of social relevance and the serviceability of the experimental tradition and, moreover, that there were faculty–student differences on these issues: 78% of the students, but only 52% of the faculty, were in the pro-social-concern quadrants and 47% of the students were in the anti-experimental quadrants with only 38% of the faculty. By further subdividing the faculty and student groups according to the number of years experience they have had in psychology, it was possible to get a cross-sectional look at the changes taking place in psychology. We assume that if the students' attitudes differ from those of the faculty and if those attitudes do not change dramatically as they become faculty, then the disciplinary consensus will change in the direction of those attitudes.

Notice, however, that it is quite misleading to compare the faculty at graduate schools to *all* the graduate students in psychology. As academic researchers and teachers, the faculty necessarily embodied many attitudes inappropriate to the large numbers of students who were not training to become academicians themselves. In the remainder of this article, therefore, we will narrow our focus to *academic* psychology and examine only those students who planned an academic career.

THE SOCIAL CONCERN FACTOR

Comparison of the factor scores on the social concern factor for five academic student and faculty groups[8] revealed a generally decreasing function relating social concern to experience in psychology (see Table 7). The midcareer faculty expressed a lack of enthusiasm for the promoting-human-welfare, solving-social-problems model of psychology that distinguished them from all the other groups. Indeed, their mean score shaded over into the area where they were for the most part disagreeing with the positively worded items that comprised that factor and agreeing with the negative ones. New students, older students, and new faculty lived up to their stereotype and showed the most desire for a "relevant" psychology, with new students and older

[8] The following subdivisions were used: new students ($N = 619$): first- and second-year doctoral candidates planning an academic career; older students ($N = 625$): third- and last-year doctoral candidates planning an academic career; new faculty ($N = 65$): faculty members in a graduate department who received their doctorate between 1966 and 1968, inclusive; midcareer faculty ($N = 163$): faculty members in a graduate department who received their doctorate between 1957 and 1965, inclusive; posttenure faculty ($N = 120$): faculty members in a graduate department who received their doctorate during or before 1956.

TABLE 7

Analysis of Variance of Social Concern and Experimentalism Scores

	New students	Older students	New faculty	Midcareer faculty	Posttenure faculty
Social concern scores					
Group mean[a]	50.90	50.10	47.62	42.94	43.60
Experimentalism scores					
Group mean[b]	45.16	43.77	46.18	45.85	43.24

Note. Higher social concern scores indicate a more pro-social-concern attitude; higher experimentalism scores indicate a more pro-experimentalism attitude. The neutral point for the social concern scale was 43.33; for the experimentalism scale, 42.50.

[a] Between ANOVA: $MS = 3{,}151.49$, $F = 16.21$, $df = 4$, $p < .01$. Within ANOVA: $MS = 194.34$, $df = 1{,}587$.

[b] Between ANOVA: $MS = 313.83$, $F = 2.58$, $df = 4$, $p < .05$. Within ANOVA: $MS = 121.69$, $df = 1{,}587$.

students very similar and more eager than the new faculty. The real surprise was the relative moderation of the posttenure faculty. That group contained the most experienced and influential psychologists in the sample, and though their concern that the discipline relate to the larger society was very similar to that of the midcareer faculty, it was nevertheless greater than would be expected by extrapolating the trend of the other four groups.

The same gradient that characterized the overall groups also described, more or less, the pattern of scores within major areas of specialization in psychology. To see this, each of the student and faculty groups was subdivided into four general areas labeled *experimental, cognitive, social,* and *clinical.*[9] Since the numbers for the faculty subgroups were then rather small, the faculty and student subgroups were combined and the faculty–student difference on the factor scores was determined within each area of interest group (see Table 8).

The most dramatic difference emerging from the breakdown by area of interest, however, was between those in experimental or cognitive areas and those in social or clinical areas. The factor scores for experimental and cognitive faculty were below the neutral point, that is, in the range where they were for the most part disagreeing with the social concern items that comprised that factor. The cognitive and experimental students were clearly on the pro-social-concern side along with all the social and clinical groups. In terms of the polarization across the neutral point on this issue, it seems to be the experimental and cognitive faculty versus everyone else including their own students.

THE EXPERIMENTALISM FACTOR

The pattern of means across the academic student and faculty groups on the experimentalism factor was somewhat similar to that which appeared on the social concern factor, though the differences were not nearly so marked (see Table 7). The midcareer faculty, who were the least concerned about producing socially useful research, were the second most accepting of the experimental tradition. The students and posttenure faculty differed from them by being less imbued with the experimental values, and the new faculty, in this case, were the most experimental. The means for all groups fell above the neutral point on the scale; thus, none of the groups was really *anti*-experimental; on the average they agreed at least slightly with pro-experimental items and disagreed at least slightly with anti-experimental items.

Collapsing the subgroups of the students and of the faculty and subdividing by areas of specialization produced no statistically significant student–

TABLE 8

Comparison of Student and Faculty Factor Scores for Areas of Specialization

Area	Student mean	N	Faculty mean	N	t
Social concern scores					
Experimental	46.23	415	37.90	122	5.90*
Cognitive	46.99	223	39.99	64	3.49*
Social	54.41	311	49.27	95	3.56*
Clinical	55.64	249	52.31	61	1.99*
Experimentalism scores					
Experimental	47.45	415	46.94	122	.46
Cognitive	42.83	223	45.00	64	1.38
Social	42.36	311	44.18	95	1.47
Clinical	43.60	249	42.51	61	.67

Note. Higher social concern scores indicate a more pro-social-concern attitude; higher experimentalism scores indicate a more pro-experimentalism attitude. The neutral point for the social concern scale was 43.33; for the experimentalism scale, 42.50.

* $p < .001$.

[9] The label *experimental* covers general experimental, motivation, learning, operant, physiological, animal, comparative, mathematical, and quantitative; *cognitive* covers perception, developmental, psycholinguistics, and cognitive; *social* covers social, personality, personnel, industrial, applied, educational and school; *clinical* covers clinical, behavior modification, and community.

TABLE 9

Faculty and Student Rankings of Graduate School Descriptions

Description	Accuracy ranking	Desirability ranking
Professional training stressing practical skills and background study in preparation for a career.	1	3
Teachers and students involved in scholarly investigation and broad education in their field of interest.	2	1
A research institution where junior and senior investigators are free to work on problems of basic science.	3	2
A place for students to gain a deeper understanding of themselves and of their fellow man.	4	4
A pool of experts and trainees to which administrators and citizens can appeal for help with social problems.	5	5

Note. Rankings are based on the rank order of the mean desirability and accuracy ratings made for each item; ratings were made on 7-point scales from desirable to undesirable or from accurate to inaccurate.

faculty differences at all on the experimentalism factor score (see Table 8). This was largely due to the moderation of the posttenure faculty, but nevertheless indicated that within no specialty area is there a really strong faculty to academic student trend away from the support of experimentalism in spite of the very slight trend overall. Only the social students fell below the neutral point on the scale and could thus be called anti-experimental (though ever so slightly); the others were just more or less pro-experimental.

The experimentalism and social concern factors were, of course, independent, yet the pattern of differences across the faculty and academic student groups were similar: Where a lot of pro-social-concern sentiment existed, there generally coexisted somewhat less pro-experimental sentiment. It seems that the criticisms that have been leveled against experimental psychology, of which irrelevance is one, have had a slight chilling effect on the vigor with which that tradition is defended, especially among students and, to a surprising degree, posttenure faculty. The overall trend, from faculty to academic students, lends support to the view that the discipline is undergoing some change in its attitudes toward experimentalism as well as toward social concern, although at a considerably slower rate.

POSSIBLE EFFECTS ON GRADUATE DEPARTMENTS

The discussion of faculty to student trends has been restricted to those students who were planning careers in academic psychology, because only that group was truly comparable to the faculty, all of whom were academicians. Academic psychology, by and large, means graduate departments in psychology, and at this point it seems pertinent to wonder if the apparent trends that have been identified toward increased concern for social problems and the slight weakening of the research values of the experimental tradition portend any significant changes in the structure and operation of graduate departments.

The similar and generally pro-experimental attitudes of most of the faculty and student groups indicated that high value was put on the kind of research associated with an academic setting in spite of the skepticism expressed by many psychologists. This interpretation was corroborated by more direct evidence from the questionnaire item that dealt with the accuracy and desirability of various brief descriptions of graduate departments. Table 9 presents the order in which the various descriptions pertinent to this discussion were ranked by the faculty and graduate students planning academic careers.

The typical graduate department was thought to be most accurately portrayed as a place for "professional training," "scholarly investigation and broad education," and "work on problems of basic science." The descriptions dealing with humanistic concerns ("deeper understanding") and social involvement ("help with social problems") were seen as the least accurate descriptors.

The ranking that indicates which descriptions were seen as most *desirable* or highly valued was almost identical to that for the accuracy of the description. Most significantly, the descriptions dealing with "scholarly investigation" and "basic science" were still ranked at the top, while those for "deeper understanding" and "help with social problems" were still ranked lowest.

Clearly, then, neither faculty nor academic students were prepared to desert the "ivory tower"—scholarly investigation and broad education in a basic science research institution—in favor of either active involvement with social problems outside the department or the personal development of the students within the department.

This article was introduced with the not so original claim that something was happening in psychology. The findings that have been presented indicate that a large part of that "something" involves not so much workaday research and theory but "metapsychology" or, if you will, "ideology"—the values and attitudes of the psychologist regarding his work and his discipline. The issue that generated the most heat was social relevance, the demand that psychology involve itself constructively in the widespread social problems that beset society. A large majority of both students and faculty felt that the discipline should be contributing to the solution of social problems, but an equally large majority felt that at present it was making no important contribution. Most significant were the large faculty–student differences on this social relevance issue with a smooth gradient from mid-career faculty, the least responsive, to new graduate students, the most responsive. If the students and young faculty retain their present attitudes as they mature in the discipline, the relevance cause will be sailing under an increasingly high wind for many years to come.

The position we associated with the basic researcher—defined by both disinterest in social problems and commitment to experimental methodology—constitutes the dominant tradition which is under attack and susceptible to change. Even its methodology, seen by many as the *sine qua non* of science, receives less support from the upcoming generations of psychologists than among most of its current faculty practitioners. There does not appear to be a single, unified "revolution" against the experimental tradition, however. Instead, our data seemed to show at least three separate countermovements, each divided from the others on matters of fundamental importance to its followers. We labeled these three movements humanist-activists, social engineers, and Mugs.

The students seemed to be attracted primarily to the ranks of the humanist-activists and the social engineers. Though this makes it appear that it will be one or both of these positions that becomes dominant in academic psychology, we noted that both groups showed applied interests—clinical, educational, etc.—and fewer of their students were planning academic careers. Furthermore, faculty and student ratings of their preferences for a graduate department showed that the department offering "help with social problems" was the least preferred of all the possibilities, while "scholarly investigation" and a "research institution" were highly valued. There thus seemed to be little support among academicians and potential academicians for a sudden transformation of the present-day graduate department in spite of their considerable sympathy with the cry for relevance. With increasing numbers of students attracted to their ranks, however, the humanist-activists and social engineers have the potential for making a significant social contribution in those areas in which they involve themselves. Indeed, it may be on their shoulders that the charge to make psychology relevant rests most heavily.

The unmistakable academicians of our three countermovements were the Mugs. Their attitudes were compatible with academic values and their rate of publication, an index of basic research interests, was as great as that of the basic researchers. Their cognitive and methodological interests could conceivably result in a choice of problems of broad enough significance to have the "relevance" that comes from asking important questions. Consider, for example, the influence Piaget's developmental psychology already has had on education and the potential inherent in Campbell's (1971) "experimenting society." Thus, the insurgence of the Mugs, because it dovetails both with academic values and, potentially at least, with the demand for social relevance, seems to be the countermovement most likely to succeed within *academic* psychology.

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The Two-Year College Instructor

A Profile

J. RUSSELL NAZZARO *American Psychological Association* [1]

There was a time in the not too distant past when teachers of psychology were thought to be people who worked in four-year colleges and graduate schools. In recent years, however, psychology courses have also become popular offerings at the high school and two-year college levels. In response to this growth, the American Psychological Association has begun to examine how it might expand and improve its services to a new population of psychology teachers. Acting on a recommendation from the Ad hoc Committee on the Two-Year College, the APA Educational Affairs Office conducted a survey during the spring of 1973 to gather information about the two-year college teacher and to explore how the national organization might serve these teachers' needs.

Since there existed no national roster of two-year college psychology instructors, a search for names was first necessary. A letter was sent to all chairpersons (by title) of psychology departments listed in the 1972 *Directory of the American Association of Community and Junior Colleges*. The chairpersons were asked to supply the names of the teachers in their departments and to indicate their status—full time or part time. A follow-up inquiry was sent to the president of the nonresponding institutions. In all, the names of 3,812 teachers (representing about 70% of the institutions) were submitted. A four-page questionnaire [2] was then sent to each teacher in an attempt to find out something about his training and present needs. At the time the information was compiled, 798 individuals (21%) had returned the questionnaire.

Table 1 shows the location of the teachers' employment. At least one person responded from each of the 50 states and the District of Columbia. Responses were also received from three Canadian provinces and a number of United States territories. California led with 143 responses; New York was second with 52 responses.

Teachers were asked in what type of two-year college they were employed. Table 2 shows a summary of responses to this question. The total number responding was 1,075 because a number of respondents classified their school in more than one category. It can be seen from Table 2 that most two-year college teachers of psychology were employed in public and comprehensive community colleges. Further, most of the teachers (89%) were full-time employees. Among those who worked part time, 34 (42%) were professional psychologists, 20 (24%) were staff members of other colleges or universities, 5 (6%) were high school teachers, and 7 (8%) were counselors in educational institutions.

The questionnaire listed five professional activities and asked what percentage of time was spent in each. Teaching was found to occupy, on the average, 61.7% of the respondents' time, counseling approximately 25%, administrative responsibilities 12%, research 9.8%, and community relations 6.6%.

TABLE 1

Responses by Location to the Questionnaire

Number responding	Location
51 and over	California, New York
41–50	Illinois, Michigan, Texas
31–40	Florida, Pennsylvania
21–30	Maryland, Massachusetts, Ohio, Washington
1–10	27 states, District of Columbia, 3 Canadian provinces, U.S. territories

[1] Requests for reprints should be sent to J. Russell Nazzaro, Educational Affairs Office, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

[2] The Ad hoc Committee on the Two-Year College assisted in the preparation of the questionnaire.

TABLE 2

Responses to the Questionnaire by Type of Two-Year College

Number responding	%	Type of school
522	49	Public two-year college
363	34	Comprehensive community college
72	7	Private two-year college
47	4	Technical school
36	3	Branch of university
21	2	Two-year college with university affiliation
14	1	Other
Total 1,075	100	

The final background information on the sample deals with educational training. Table 3 summarizes the highest degrees held by respondents. The majority of respondents appeared to have master's degrees in psychology; only 139 (18%) of the sample had PhDs and 51 (7%) had EdDs; 221 (29%) teachers were working toward an advanced degree, either a PhD or an EdD, but 529 (71%) were not actively pursuing higher degree levels.

Asked if their graduate education had included special training for teaching in the two-year college, 573 teachers said no (75%); the remaining 192 (25%) who responded to this question said that they had courses on junior college teaching and community college curriculum and had participated in either an internship or teaching assistant program. Most of those who had special training (86%) found it valuable. The most valuable components of special training, among nine listed choices, were considered to be units on individualizing instruction, instructional methods, evaluation of student achievement, and curriculum development; only about 35% felt that their graduate teaching internships had been helpful. Specific courses such as history, philosophy, objectives of the two-year college, and faculty–administration relations were listed by approximately 235 (approximately 30%) of the respondents. About 20% of the teachers found useful a course in problems of higher education. Other helpful aspects of training were listed at a lower frequency, possibly due to their unavailability in most training programs.

Open-ended responses to the question on helpful aspects of training can be grouped into six different areas. The first area is methods of teaching. Here, innovative techniques, audiovisual aids, communication skills, and group dynamics were considered helpful for presenting materials in the actual classroom setting. The second area most often mentioned can be characterized as skill in understanding the junior college student, including motivation, life-style, needs, age group, and minority status. Third, teachers found courses in counseling and advising techniques to be of help. Since almost one quarter of the teachers' time is spent in counseling, this should certainly have a place in training the two-year college teacher. A fourth area of importance was preparing lessons in applied psychology for students taking only one or two psychology courses or planning to enter vocational rather than academic fields. Human relations training and humanistic approaches were grouped together in a fifth area, and self-evaluation skills was the sixth area.

The most important aspect of the survey is summarized in Table 4. If APA plans to improve and expand its services for two-year college teachers, these data can be used as a basic needs assessment summary. A list of 10 possible association activities was presented to the survey population, and they were asked to rate each from very important to very unimportant. An open-ended question allowed them to list activities that were not in the 10 possibilities. The total number of teachers responding is given in the final column of Table 4. Activities are listed in descending order of importance to respondents. The responses show an overwhelming interest in improving communication, expressed as 80% agreement among respondents that developing a resource center, sponsoring meetings, and developing a periodical should be considered among the most important activities of the APA.

The actual wording of the activity receiving highest priority was "development of a resource center (clearinghouse) for instructional materials,

TABLE 3

Level and Field of Educational Training

Degree	Psychology		Education		Other		Total	
	n	%	n	%	n	%	N	%
BA	5	1	3	0	2	0	10	1
MA	319	41	145	19	109	14	573	74
PhD	93	12	24	3	22	3	139	18
EdD	19	2	11	1	21	3	51	7
Total	436	56	183	24	154	20	773	

TABLE 4

Proposed APA Activities for Two-Year College Teachers

Activity	Rating					Total
	1	2	3	4	5	
1. Develop a resource center	397	235	105	30	15	782
2. Sponsor meetings	345	289	114	22	21	791
3. Develop a periodical	332	320	82	31	29	794
4. Articulate training levels	299	263	161	42	15	780
5. Increase professional association involvement	289	279	141	48	25	782
6. Improve teacher training programs	278	273	154	45	26	776
7. Compile employment information	251	263	194	58	22	788
8. Increase number of teacher training programs	205	255	218	71	24	773
9. Increase professional committee involvement	190	285	224	58	18	775
10. Compile a national register	[illegible]	229	277	137	70	791

Note. Rating scale: 1, very important; 2, important; 3, slightly important; 4, unimportant; 5, very unimportant.

educational consulting services, etc. . . ." Undertaking such a project would probably mean obtaining a grant from some outside source. Since clearinghouse organization and operation are often grant supported, a clearinghouse for two-year college teachers of psychology could be adapted from models already existing in related disciplines. There is much to be said for initiating this activity at the present time. For example, it is expected that before the end of the decade an educational satellite will be launched to provide communication between classrooms and resource centers throughout the country. It would be most useful to have at least a nucleus center established by then.

Two-year college teachers also felt that APA should sponsor more regional and local meetings for two-year college teachers of psychology, including workshops, programs at meetings of psychological associations, etc. Until recently, most programs at the regional association meetings and the national APA Convention have had little to offer two-year college teachers. Programs generally have been research oriented rather than teaching oriented and have been run by PhDs for small groups involved in similar research projects. Travel to the meetings is expensive, as is registration. It is increasingly more difficult to gain administrative approval and reimbursement for travel to professional meetings, even for many four-year college instructors, and without a meaningful program it is almost impossible for the two-year college instructor to get this administrative support. If programs could be provided closer to home and with greater participation by the teachers themselves, this problem might be overcome.

Items 5 and 9 in Table 4 are related to the problem of professional meetings. The responses to these items indicate that many teachers are interested in being more involved in work at the regional, state, and local levels. Table 5 summarizes teachers' interests in professional association activities. Although 28% of the respondents were members of the APA, only about 5% have been active. In response to the question, "Which psychological associations would you like to be most involved in?" highest interest was shown in local associations, with state, regional, and national associations following in that order. Only 34 (4%) of the population sampled had participated in special sessions for two-year college teachers held in conjunction with 1972 regional psychological association conventions. Asked if they would like to participate in similar meetings in the future, an overwhelming 608 (90%) responded affirmatively. It appears that there is high interest among two-year college teachers in attending appropriate meetings and workshops.

The third area that could be subsumed under improved communication is that of developing a periodical. For the last few years various groups within the APA have urged the development of a journal specifically designed for the teaching of psychology. Most other disciplines already have this kind of publication. If a journal were chosen as the best vehicle for exchange of information, a special section might be devoted to issues concerning the two-year college instructor. It must be remembered that the community and junior colleges generally serve a much more diverse group of students than do the four-year institutions. Students' abilities and goals are often quite different from those seeking the BA or BS degree. At the same time, it should be noted that one third to one fourth of all college graduates start in junior colleges. Therefore, it is essential to provide them with

TABLE 5

Interest in Professional Association Activities

Organization	Member	Active	Interested in joining
American Psychological Association	230	41	282
State psychological associations	175	62	138
APA Division 2 (Teaching)	147	5	250
National Education Association	132	16	24
American Personnel and Guidance Association	126	34	62
Regional psychological associations	125	31	94
Local psychological associations	99	48	54
Other	294	204	16

courses that will be credited and meaningful in their later training.

Of the respondents, 72% assigned a rank of 1 or 2 to Item 4 in Table 4, articulating training levels. Improved articulation of programs across training levels could avoid much repetition and waste of course requirements. It could also lead to the development of a career ladder within the field. Items 6 and 8 in Table 4 reflect on the training of the teachers themselves. In the earlier discussion, important and helpful aspects of training were revealed, but it was noted that the majority of institutions provided no training experiences specifically designed to prepare the teacher of psychology.

Moderate interest was shown in the development of data on employment practices in the job market (Item 7, Table 4). It is likely that persons seeking employment at the two-year college level apply directly to the school in the area where they wish to teach; therefore, efforts to maintain an updated employment file would be less important than efforts to generally upgrade professional standards and support in other areas. Similarly, maintaining an up-to-date national register of two-year college psychology instructors was not considered of utmost importance (relatively speaking) by the teachers themselves (Item 10, Table 4); however, it would be most valuable to anyone attempting to organize meetings or disseminate information on a national level. This activity may be more of a necessary record-keeping service than an actual service to the two-year college teacher.

In response to the open-ended question asking for activities the teachers would like to see the APA undertake, a number of workshop possibilities were generated. Interest appeared to be high in instructional techniques, resources (including audio-visual material and texts), new developments in research, workshops between two- and four-year college instructors, interdisciplinary opportunities for curriculum development, psychology in industry, psychology in law enforcement, and psychology in medicine and psychotherapy. Teachers asked for support in academic freedom, flexibility of class arrangements, increased opportunities for graduate study and research, and help in course development. They would welcome financial support for travel and research as well as more reasonable professional fees. A request was made for the development of a master–teacher system whereby a partnership is arranged with a scholar in a senior institution. Teachers would like to be able to exchange their assignments with instructors in other parts of the country, and they would like to see summer institutes for training sponsored by the national association.

If the American Psychological Association decides to assist the two-year college professional teacher, the needs and priorities are clearly established. All that remains is to determine how much of APA's resources will be allocated to this group or whether alternative mechanisms should be developed to help them help themselves.

Eighty-Second Annual Convention of the American Psychological Association

Convention Information

THEODORE G. DRISCOLL, JR. *Convention Manager*
CARL N. ZIMET *Chair, Board of Convention Affairs*

This is New Orleans! Air conditioning . . . Al Hirt . . . Andrew Jackson . . . antebellum plantations . . . antiques . . . Antoine's . . . Arnaud's . . . Audubon Park . . . bananas Foster . . . Basin Street . . . Battle of New Orleans . . . bayous . . . Bourbon Street . . . breakfast at Brennan's . . . Café du Monde . . . café au lait and beignets . . . Canal Street . . . cemeteries . . . chicory coffee . . . Commander's Palace . . . Court of the Two Sisters . . . courtyards . . . Creole cuisine . . . dixieland . . . female impersonators . . . five-cent telephone calls . . . French Market . . . French Quarter or "Vieux Carre" . . . funeral marches . . . Galatoire's . . . Garden District . . . gumbo . . . hot and humid . . . Jackson Square . . . jazz . . . Jean Lafitte . . . Jim Garrison . . . lace balconies . . . "ladies" of the night . . . Lake Pontchartrain . . . levees . . . Longue Vue Gardens . . . the Longs of Louisiana . . . Mardi Gras . . . mausoleums . . . Old Absinthe House . . . oysters Rockefeller . . . Pat O'Brien's . . . pecan pralines . . . Pete Fountain . . . prawns . . . Preservation Hall . . . Ramos gin fizz . . . river boats . . . St. Charles streetcar . . . streetcar named Desire . . . Sugar Bowl . . . Superdome, finished or unfinished . . . topless dancers . . . voo-doo.

And that is where the 1974 Convention of the American Psychological Association will be held. The dates of the Convention are **August 30 through September 3.** While August is not the ideal time to be in New Orleans, the attractions of the city should compensate for the heat and humidity. Meetings are scheduled in the **Fairmont Hotel,** the **International Hotel,** the **Marriott Hotel,** the **Monteleone Hotel,** the **Royal Orleans Hotel,** and the **Rivergate Exhibition Center.** The bulk of the Convention program is scheduled between 9:00 a.m. and 6:00 p.m. during the five days of the meeting. Because hotels are fairly close to one another, APA is not planning to run a shuttle bus among the hotels. Public transportation buses run up and down Canal Street every few minutes at a cost of 15 cents per person.

Canal Street, New Orleans' main thoroughfare, separates the French Quarter from the business district and uptown area of the city. There are historical sights, shops, nightclubs, and restaurants within the French Quarter. In order to sample the cuisine in local restaurants it would be wise to make reservations for all meals. A handy reference on inexpensive and good restaurants is *The New Orleans Underground Gourmet* by Richard H. Collin.

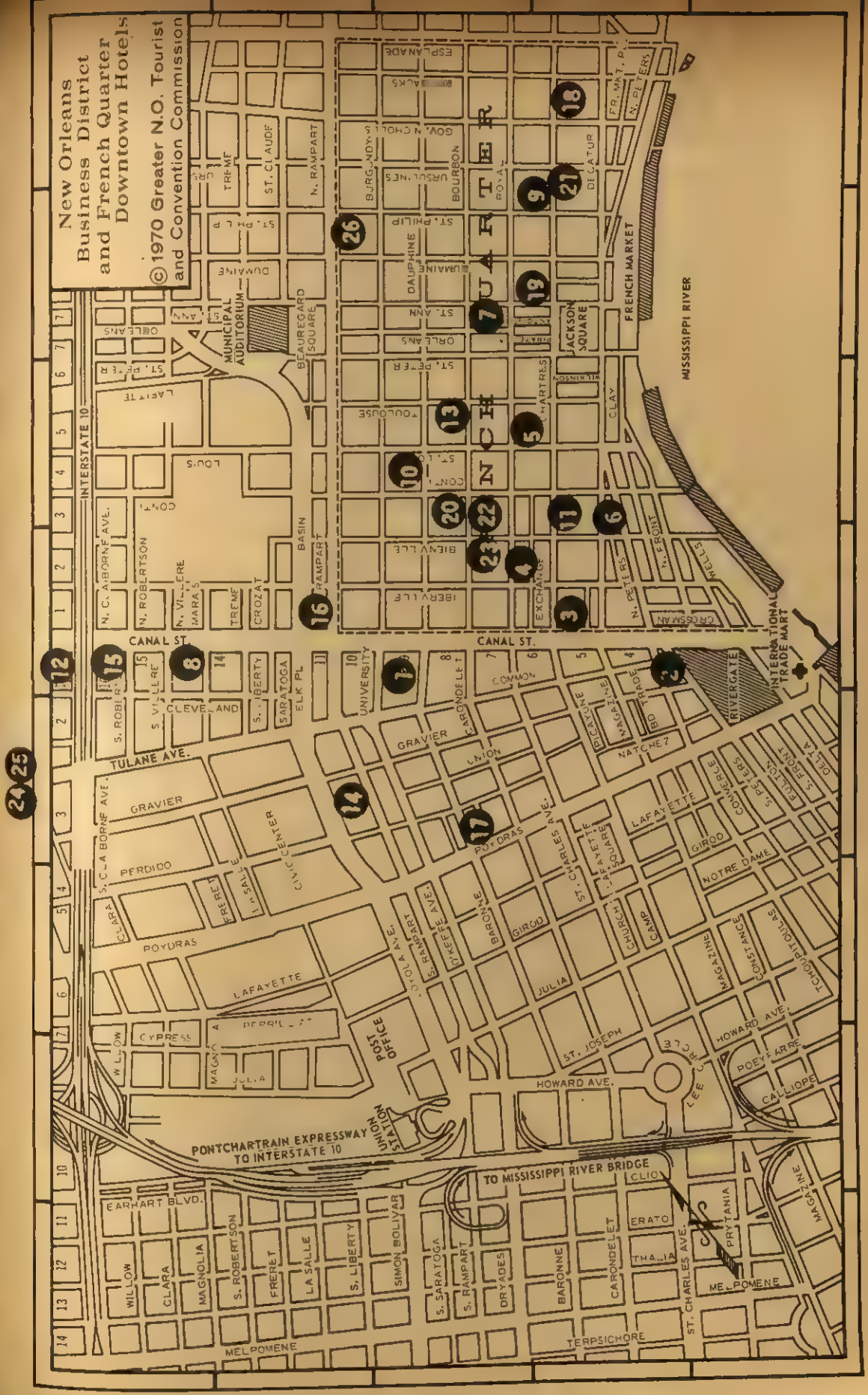
Transportation from the New Orleans International Airport to the downtown area is available by taxicab or limousine. It takes about 30 minutes to make the trip. The taxicab fare is approximately $9 plus tip for one to three persons; there is an additional charge of $3 per person for more than three. Limousine service is available directly to the hotels at $3 per person. Public transportation is also available from the airport for a fare of 35 cents, but it may be necessary to transfer to another bus within the city in order to get directly to your hotel. See you in New Orleans!

Housing

In cooperation with APA, all of the hotels have set aside, at guaranteed rates, substantial blocks of rooms. These hotels have guaranteed rates for the Convention only when registration is made through the APA Housing Bureau on the official Hotel Reservation Form. Also, these rates are guaranteed only when the Advance Registration Form and Hotel Reservation Form are returned *prior to August 1, 1974.* After August 1, every effort will be made to assign rooms at the guaranteed rate, but such assignments cannot be certain, and it is quite unlikely that late requests can be honored.

Members should also bear in mind that with the large number of rooms used, some members may not be assigned a room in the specific hotel they requested nor the type of room at the rate requested. It is advisable to return the Advance Registration Form and the Hotel Reservation Form as early as possible. These forms appear at the end of this issue.

All housing forms are sent from the Central Office to the New Orleans Convention Bureau for room assignments. We anticipate a three- to four-week lag between receipt of request in the APA office and your receiving confirmation from the hotel. If after four weeks you have not heard from New Orleans, please contact *di-*


New Orleans
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and French Quarter
Downtown Hotels
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MUNICIPAL AUDITORIUM
BEAUREGARD SQUARE
JACKSON SQUARE
FRENCH MARKET
MISSISSIPPI RIVER
INTERNATIONAL TRADE MART
RIVERGATE
INTERSTATE 10
CANAL ST.
TULANE AVE.
GRAVIER
POYDRAS
LAFAYETTE
HOWARD AVE.
POST OFFICE
UNION STATION
PONTCHARTRAIN EXPRESSWAY
TO INTERSTATE 10
TO MISSISSIPPI RIVER BRIDGE
EARHART BLVD.
MELPOMENE
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rectly the New Orleans Convention Bureau, 334 Royal Street, New Orleans, Louisiana 70130.

Wheelchair Accessibility

To facilitate making hotel reservations for those people concerned with wheelchair accessibility, Division 22, the Division of Rehabilitation Psychology, compiled the following information on the accessibility of some of the Convention hotels. Wherever possible, the information has been corroborated by site visits made by knowledgeable persons. Detailed information about the accessibility of various churches, restaurants, museums, and stores may be obtained by writing for a free copy of *Guide to New Orleans for the Handicapped* from Louisiana Chapter, National Society for Crippled Children and Adults, Inc., 843 Carondelet Street, New Orleans, Louisiana 70130.

Rivergate Exhibition Center

Main entrance: Accessible
Restrooms: Accessible
No sleeping rooms

International Hotel

Main entrance and elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Two meet accessibility requirements and *must be requested.*

Monteleone Hotel

Garage entrance and three elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Hotel has 56 accessible sleeping rooms which *must be specifically requested.* They are rooms on the fifth floor in the following series: 53, 56, 57, 58, 63, and 69.

Fairmont Hotel

One of the main entrances to the hotel is ramped.
Public restrooms: Women's—accessible
Men's—inaccessible
Sleeping rooms: Inaccessible

Royal Orleans Hotel

Main entrance: Assistance available to negotiate the steep ramped entrance from garage and staired main entrance.
Elevators: Accessible
Public restrooms: Accessible
Sleeping rooms: Inaccessible

Marriott Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Royal Sonesta Hotel

Main entrance: Accessible
Elevators: Accessible
Public restrooms: Inaccessible
Sleeping rooms: Inaccessible

Persons requesting wheelchair-accessible accommodations should check the appropriate box on the Hotel Reservation Form as well as indicate the hotel and type of accommodation requested.

Registration

Convention attendees are urged to register for the meeting in advance. Not only will advance registration assist APA in planning for the meeting, but advance registration represents a saving to the registrants as well as the opportunity to obtain desired hotel space and rates. At its meeting on January 18–20, 1974, the APA Council of Representatives approved the following registration fees for the 1974 Convention:

For those registering in advance (prior to August 1, 1974):

$30—Nonmember
$20—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

For those registering on-site in New Orleans:

$35—Nonmember
$25—APA Member, Fellow, Associate, Foreign Affiliate, High School Teacher Affiliate

Key to Map of New Orleans

1. Fairmont Hotel
2. International Hotel
3. Marriott Motor Hotel
4. Monteleone Hotel
5. Royal Orleans Hotel
6. Bienville House
7. Bourbon Orleans Ramada
8. Braniff Place
9. Chateau Motor Hotel
10. Dauphine Orleans Motor Hotel
11. de la Poste Motor Hotel
12. Delta Towers Hotel
13. Downtowner du Vieux Carre
14. Downtown Howard Johnson's Motor Lodge
15. Governor House
16. La Salle Hotel
17. Le Pavillon
18. Le Richelieu Motor Hotel
19. Place d'Armes Motor Hotel
20. Prince Conti Motor Hotel
21. Provincial Motor Hotel
22. Royal Sonesta Hotel
23. Saint Louis Hotel
24. Tamanaca (Best Western) Downtown Motel
25. Thunderbird (Quality Inn)
26. Vieux Carre Motor Lodge

Students and other authorized exemptions from these fees appear on the Advance Registration Form.

The convention badge, with name and institutional affiliation, will be mailed in advance of the Convention to those who preregister. Advance registrants will need only to obtain a badge holder at the APA registration area in New Orleans to complete the procedure—thus avoiding possible delays.

Complete member and nonmember registration facilities will be maintained at three locations—the Fairmont Hotel, the Marriott Hotel, and the Rivergate Exhibition Center—according to the following schedule:

Thursday, August 29, 3:00 p.m. to 9:00 p.m.
Friday, August 30, 8:30 a.m. to 5:30 p.m.
Saturday, August 31, 8:30 a.m. to 5:30 p.m.

From Sunday, September 1 through Tuesday, September 3, registration facilities will be available only at the Rivergate Exhibition Center. Hours will be 8:30 a.m. to 5:30 p.m. on Sunday and Monday; 8:30 a.m. to 12 noon on Tuesday.

Placement

The APA Convention Placement Office will be located in the Rivergate-South Hall. Hours will be from 9:00 a.m. to 5:00 p.m., August 30–September 3, except 1:00 p.m. closing day. Fees required are as follows:

Applicants—$2 for reproducing (1) availability information. $3 for use of Convention Placement facilities includes a copy of the *Position Openings Bulletin*. (Nonmembers will be charged $20 for use of Convention Placement facilities.) July 19, 1974, is the deadline for applicant listing. Appropriate fee must accompany listing.

Employers—$5 for reproducing (1) Job Description. $10 for use of Convention Placement facilities includes a copy of the *Availability Notices Placement Bulletin*. July 26, 1974, is the deadline for employer listing. Appropriate fee must accompany listing.

Employers: Please note the addition of the GENERAL OPENINGS category to be used by organizations having widespread needs for psychologists who may be recruited through a central office.

Prelisting in the more widely distributed printed booklet is encouraged, but Xerox forms will be available for on-site listing. The forms, accompanied by the appropriate listing fee, must be received by the date indicated above. Please do not include the Convention Placement facilities fee which will be collected ONLY when you register at the Placement Office. After the above date, Xerox forms will be available for listing, and requests should be directed to the address below.

A complete set of applicant availabilities will be available to employers: $15 for each category—please indicate choice. After the Convention, order from the Convention Placement Office, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Mail orders for the printed *Availability Notices Placement Bulletin* and the *Position Openings Bulletin* are $1.50 per copy (available August 7). Your check (no billing) may be sent to the above address.

Convention Personnel

You may wish to contact one or more of these people before or during the Convention:

Board of Convention Affairs: Carl N. Zimet, Division of Psychology, University of Colorado School of Medicine, Denver, Colorado 80220.

Convention Manager: Theodore G. Driscoll, Jr., Driscoll and Associates, 7109 Masters Drive, Potomac, Maryland 20854.

Assistant for Convention Management: Candy Won, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Placement Office: Robert L. Campbell, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Please Note

Meetings for Divisions 19 and 21 will be located in the *Monteleone Hotel* rather than the Royal Orleans Hotel as noted in the April convention insert.

Convention Child Care Service

Provision has been made for a professional child care service during the 1974 Convention. CON-SERV has been engaged and will offer an extensive program.

Parents desiring to use child care facilities must preregister no later than July 15, 1974. To preregister, complete the form at the bottom of this page. Please note that upon filing of the preregistration form and payment of the advance deposit as indicated on the form, CON-SERV will provide complete detailed information about the services offered and final registration materials necessary to plan for your children's comfort and safety. If for any reason your plans change, the deposit will be refunded in full if CON-SERV is notified of cancellation by August 10.

Some of the essential features of this service, as planned, are indicated below:

- The facility will be located in Braniff Place Hotel and will be open from 8:00 a.m. to 6:30 p.m., August 30–September 3, 1974. While the cost will be about $1.50 per hour per child, APA will subsidize $.75 of this cost. Hence, the cost to parents during the daytime will be $.75 per hour per child.
- If there is sufficient need, the facility will remain open after 6:30 p.m. until 12 midnight to accommodate parents who may wish to leave their children in the facility during evening hours. The parents will bear the full cost during these hours. If the need for evening child care does not justify keeping the facility open after 6:30 p.m., personnel at the facility will make every effort to help individual parents locate qualified baby-sitters to stay with children in their hotel rooms for those who might need this service. In any event, every effort will be made by CON-SERV to assist parents with evening child care arrangements.
- During the regular daytime hours, a variety of optional tours will also be offered at modest cost to parents. APA will subsidize part of the cost to the extent of $.75 per hour per child as though the child were in the facility.
- In order to evaluate the child care services at past conventions, the Task Force on Convention Child Care would like you to answer the following questions: (*a*) What was your general reaction to past child care facilities? (*b*) What was the least-liked aspect? (*c*) What was the most-liked aspect? (*d*) What was needed and not provided? Please attach your answers to the preregistration form.

Preregistration Form: Child Care Service—APA Convention
New Orleans, Louisiana, August 30–September 3, 1974

Preregistration form must be returned no later than July 15, 1974.

Name of Parent or Guardian: _____

Street Address: _____

City, State, Zip: _____

Name(s) of children to be registered in the child care facility:

Name	Age

If your child has a specific problem or handicap that we should be aware of, that is, speech impediment, retardation, epilepsy, etc., please let us know so that we may make the necessary provisions. _____

Please check this box if you are interested in tour information for your child

_____
Signature of Parent or Guardian

Please return this form to **Ms. Diane Snow, Con-serv, 1318 Arabella Street, New Orleans, Louisiana 70115,** and enclose a check in the amount of $7.50 for one child or $15.00 for two or more children as listed above. Make checks payable to "Con-serv." Indicate amount enclosed: $_____.

NOTE: This is only your preregistration form. Upon receipt of this form you will be furnished complete registration materials.

Employers of Psychologists:

If you have or anticipate openings for psychologists, you may wish to list them with the APA Convention Placement Service. The Job Description Form below is for this purpose. Additional copies supplied upon request.

A service fee of $5.00 per insertion will be required for listing in the **Convention Placement Bulletin.** FEE MUST ACCOMPANY COPY—no billing by APA—DEADLINE JULY 26. If you recruit at the Convention, an additional $10.00 fee will be required for use of the Placement Office facilities. This fee payable ONLY when you register at the Placement Office.

The FINAL FORM of your job description below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for publication. Please list your job data of the kind and in the order illustrated. Do not skip any lines. Thank you.

ILLUSTRATIVE FORMAT

Job title (caps) ↓ Number of openings ↓ Degree required or preferred ↓ Date job begins ↓

Starting salary basis →

Duties →

Employer name & address →

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CLINICAL PSYCHOLOGIST: 1 PhD or MA lacking only PhD thesis.  Now or Jan. 1975
$14,000-16,000/12 months.  Internship + 2 yrs experience in mental health
setting; teaching experience desirable.
Teach intro clinical psych to interns, nurses in university hospital: perform
diagnostic & therapeutic activities in univ mental health clinic; do research
in area of own interest as time allows. Joint appointment in clinic & psych
dept possible.
JA Smith, Chairman, Dept Psych, Univ of Erie, Centerville 3, NY.
See him Aug. 30, 31, Sept. 1, 2, am & pm, or write.
```

Tentative interview schedule (Aug. 30-Sept. 2, am & /or pm, Sept. 3 until 1 pm are the available times). If none, enter name to whom inquiries may be mailed.

WORKING COPY—Draft your job description here, following the above guidelines.

1 2 3 4 5 6 7 8 9

FINAL FORM—TYPE YOUR FINAL JOB DESCRIPTION WITHIN BOUNDARY ONLY.

Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research ☐ General Openings

Detach on dashed line and return to:
DEADLINE: JULY 26, 1974
FEE MUST ACCOMPANY COPY

Convention Placement Office, American Psychological Association
1200 Seventeenth Street, N.W., Washington, D.C. 20036

The FINAL FORM of your qualifications below will be the actual copy used in photo offset printing of the **Convention Placement Bulletin.** The box thereon is the maximum space usable. An illustration and a work form are provided for your guidance. Only clean typed copy within the boundary on the FINAL FORM will be accepted for printing. A service fee of $2.00 will be required for listing in the **Convention Placement Bulletin. Fee must accompany copy**—no billing by APA—**Deadline is July 19.** A further charge of $3.00 will be made at the Convention for those members who use the Placement Office facilities. **The fee will be $20.00 for nonmembers.**

Please list your qualifications of the kind and in the order illustrated. Do not skip any lines. Thank you.

STRATIVE
ORMAT

Name (caps) — If known, convention address, phone — Home Address

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LEO SCHOPPMEYER, Fairmont, New Orleans, 529-7111. 3456 Shadygrove Drive, Anytown, Ohio. APA member. PhD 1958 U of Minn.
1963-present: sr clinical psychologist, Fairview Psychiat Hosp, Ohio, diag & ther wi children & adults, exp'l resch on commun rehab program, teaching interns. 1960-63: clinical psychol, Brooklyn Mntl Hlth Clinic, NYC, diag & ther incl supvsn of spec classes for retarded. 1959-60: clin psycho, VA Hosp, Mpls, Minn., obj & proj test admin, supvsd therapy after 1 yr internship.
1 paper, 2 articles. Desire supervisory clinical psychology position in forward looking community clinic setting. $18,500. East or midwest.
See Aug. 30, 31, Sept. 1, am & pm, or write.

Tentative interview schedule (Aug. 30-Sept. 2, am &/or pm, Sept. 3 until 1 pm are the available times). If not attending, so indicate.

K COPY—Draft your qualifications here, following the above guidelines.

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L FORM—TYPE FINAL COPY OF YOUR QUALIFICATIONS WITHIN BOUNDARY ONLY.

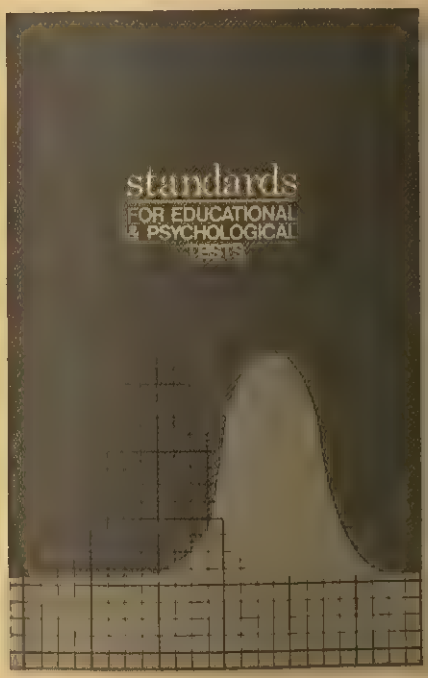
1. Check one category for listing: ☐ Academic ☐ Clinical & Counseling ☐ Industrial & Research
2. Check: ☐ Member ☐ Associate ☐ High School Teacher Affiliate ☐ Foreign Affiliate ☐ Student Journal Group ☐ Non-Member

etach on dashed line and return to:
EADLINE: JULY 19, 1974
E MUST ACCOMPANY COPY

Convention Placement Office, American Psychological Association
1200 Seventeenth Street, N.W., Washington, D.C. 20036

Standards for Educational and Psychological Tests

1974 revision.



Developed under a grant from the Russell Sage Foundation by a joint committee of members from the APA, the American Educational Research Association, and the National Council on Measurement in Education.

This current revision of **Standards** is directed equally toward establishing guidelines for test developers and test users. It proposes *essential, very desirable* and *desirable* considerations for the effective development and use of standardized tests. Additionally, each standard is accompanied by an exemplary or clarifying comment. 80 pp. $3 to APA, AERA, and NCME members; $5 to nonmembers.

Contents:

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Standards for Tests, Manuals and Reports

Standards for Reports of Research on Reliability and Validity

Standards for the Use of Tests

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American Psychological Association
Publication Sales Dept.

1200 17th Street, N.W.
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Comment

Graduate Training in Philosophical Psychology

I was instructed by the Executive Committee of Division 24 to bring to the attention of the larger psychological community the results of a survey on graduate training in philosophical psychology sponsored by the Division. The results of the survey follow. I think the findings speak for themselves and I shall let each of you draw your own conclusions.

1. Total number of schools written to = 305.

2. Total number of schools responding = 160.

3. Sources of information:

Anonymous written note	30
Note by chairman	67
Bulletin (brochure, flyer)	37
Personal letter	26
Total	160

4. Is there a specialty offered in philosophical psychology?

Yes = 5 No = 155

5. If Yes, then what is the nature and extent of it?

(*a*) Georgia State University, Atlanta: Foundations of Psychology Program still in formative stages.

(*b*) Union Graduate School, Antioch College: Union Graduate School designs the program for each student to fit his background, needs, and goals. A few do major in philosophical psychology, all are PhD candidates.

(*c*) University of Ottawa, Canada: Specialty in philosophical psychology is open to any PhD candidate who should elect it. There are psychology professors competent in this area to guide the candidate in his academic work.

(*d*) Duquesne University: Existential phenomenological philosophical approach is applied to numerous psychological problems at both the MA and PhD levels.

(*e*) Center for Advanced Study in Theoretical Psychology, University of Alberta, Canada: The Center is a doctoral and postdoctoral research organization devoted to the clarification of conceptual problems in psychology and investigation of the field's theoretical-philosophical presuppositions.

6. If No, then is there an emphasis on theoretical psychology?

No information	48
No	63
Some	2
Slight	9
Average	5
More than average	2
Strong	4
Varies with professor	2
Only in standard courses	10
Only as related to research data or content areas	10
Total	155

Amedeo P. Giorgi
Duquesne University

On "Obedience to Authority"

Obedience is not to be understood solely by reference to the individual's conforming deed; that is merely the end product of a long process of prior programming by which the rationality of power, dominance, and authority become impressed upon us. We are controlled not by the physical strength of adversaries but by the symbols, rules, and words manipulated by those of our own kind. The technology of behavior control begins with the subtle family processes involved in "civilizing" infants to be "good," "acceptable" children. The major lesson taught in all traditional school systems is the necessity to obey trivial, irrelevant rules and to observe protocol, while at all times respecting authority because it exists. Such control is perfected by a variety of social institutions that encourage adults to exchange freedom of thought, independence, and individuality for the delusions of social and economic security, collective political strength, and personal approval.

I believe that the three major themes of Milgram's research (for review, see Milgram, 1974) and of our companion research on simulated prisons (*New York Times Magazine*, April 8, 1973) are

(*a*) that obedience to authority requires each of us to first participate in the myth-making process of creating authority figures who then must legitimize their authority through the evidence of our submission and obedience to them;

(*b*) that the reason we can be manipulated so readily is precisely because we maintain an illusion of personal invulnerability and personal control, all the time being insensitive to the power of social forces and "discriminable" stimuli within the situation, which are in fact the potent determinants of action; and

(*c*) that evil deeds are rarely the product of evil people acting from evil motives, but are the product of good bureaucrats simply doing their job.

Rather than misfocusing attention on the ethics of Milgram's research, the dramatic aspects of the subjects' conflicts, or the ingeniousness of the experimental contrivances, we should focus on the very different concerns that are engendered by a meaningful reflection on *obedience to authority*. We must first increase our sensitivity to, and our need for, more knowledge about those conditions in our everyday life where, despite our protest—"*I* would never do what *they* did"—*we* would, and we do, behave contrary to our expectations. Second, we must critically reexamine the ethics and tactics of our revered social institutions, which lay the foundation for our mindless obedience to rules, to expectations, and to people playing at being authorities.

The question to ask of Milgram's research is not why did the majority of normal, average subjects behave in evil (felonious) ways, but what did the disobeying minority do after they refused to continue to shock the poor soul, who was so obviously in

pain? Did they intervene, go to his aid, denounce the researcher, protest to higher authorities, etc.? No, even their disobedience was within the framework of "acceptability"; they stayed in their seats, "in their assigned place," politely, psychologically demurred, and they waited to be dismissed by the authority. Using other measures of obedience in addition to "going all the way" on the shock generator, obedience to authority in Milgram's research was total!

Such an experimental result is more than merely "probably important" (as Steven Marcus's review in the *New York Times,* January 13, 1974, suggests). It ought to give each of us pause as no other single bit of research has. But it will not, because the vital lessons about human conduct are really not influenced by research psychologists or heeded even when nicely expressed by English professors. The lessons reach the people through their momma and poppa, the homeroom teacher, the police, the priests, the politicians, the Ann Landers and Joyce Brothers, and all of the other "real" people of the world who set the rules and the consequences for breaking them.

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PHILIP G. ZIMBARDO
Stanford University

Reinterpreting Sarason's Blackishness

Psychological science could benefit significantly from further examinations of nature and nurture issues as presented by Sarason (November 1973). The long-range payoff in societal amelioration and true human development would be evident. On the other hand, behavioral scientists who promote quasi-scrutinous evaluations of the confounding matrix of "nurture" variables also could be handicapped by Sarason's insistence on using a comparative model: blackishness versus Jewishness. As a black psychologist who has devoted much thought to racial–ethnic psychodynamics, I find such ethnic parallelism highly inappropriate and objectionable. It becomes increasingly obvious to me that until the real dimensions of nurture as explicated from differential intraethnic perspectives are fully explored, the real danger exists not in the metatheories of advocates promoting genetic inferiority (of blacks) but in the environment–nurture camp whose perspective is weak, faulty, and misguided.

First, to counter Sarason's notions let me present a series of arguments that should expose the fallacy of a comparative analysis between the black and the Jewish experiences:

1. While a fairly large number of Jews seem to have inculcated "a respect for book learning," it would be fallacious and deceptive to assign this proclivity to the overwhelming majority of Jewish persons. The broad-based Jewish scholarship in many fields is frequently used as the empirical yardstick of Jewish achievement, yet quantitatively this phenomenon represents neither innate superiority nor a unique "nurturance for scholarship" factor. A constellation of various political, economic, and religious considerations has contributed significantly to Jewish advancement. At present, no other minority group has had the opportunity to even approach this level of accomplishment in America.

2. Sarason stated that "blackishness has not had at its core unbounded respect for book learning and the acquisition of academically soaked, cognitive skills [p. 968]." Such a notion typifies a weak diagnosis of social phenomena. Actually, the real issue here is that historically, American institutions have deliberately and consistently engaged in denying and excluding black people from educational achievement. The fact that large numbers of blacks have still managed to endure and achieve represents human accomplishment that has few parallels.

3. The attitudinal core of blackishness, for example, toward education, does not require a modicum of change as Sarason suggested. The real change is needed fundamentally in America's social, political, and educational institutions and in those individuals who have promoted oppression.

4. Although Sarason is correct in asserting that societal amelioration must occur within a context of a time perspective, his allegiance to time has potentially dangerous political implications. Our policymakers could very well interpret such a perspective as indicative of the need to minimize, or even discontinue, such programs as Head Start with such rationales as "it's too early to know if the program has really worked" or "the small IQ gains don't justify the cost benefit factors."

5. Sarason seriously failed to comprehend the black experience when he asserted that both Jews and blacks believe that "this is a hostile world." In my opinion, many blacks today have not reached this important level of consciousness, but the number is increasing steadily. In fact, I have often advocated that young black children should encounter direct socialization experiences through which they can understand white society, and not their own, as the cause of interethnic difficulties. To many, such enculturation smacks of promoting racial hatred and bigotry as inevitable consequences, but, from my perspective, such developmental experiences would contribute significantly to the future of black mental health.

Sarason's autobiographical account was seemingly written for the purpose of capturing a typical Jewish developmental experience. In a similar vein, my own black autobiography should be equally enlightening, al-

though I disclaim its generalizability to any other black experience. The reason for this disclaimer will be presented subsequently.

Following Sarason's format, let me begin with my father, also a simple and unassuming man. Consistent with many sociological analyses, he migrated during the 1920s from the rural South to a northern metropolitan area, in his case, Washington, D.C. Although undereducated, he did believe, like many blacks, that the educational process unlocked the chains of economic oppression. Unfortunately, he was only able to obtain a night school certificate indicating completion of the eighth grade because the Washington school system made no provisions for adult secondary education at that time. He did not read books, but he was a copious and avid reader of the *Washington Post*. In fact, as a long-range consequence, I am one of those several people who knew BW (Before Watergate) that the *New York Times* is *not* the world's best newspaper.

My elementary school experiences were significant in molding my orientation toward education and the learning process. Refusing to enroll me in the school directly across from our house, my mother insisted that I travel to a more distant school. She reasoned that her choice was based on the fact that the more distant school's principal was her former fourth-grade teacher who fostered quality education. As a child psychologist I now suspect that she wanted to enjoy the peace and quiet that most mothers cherish when their offspring enter school.

Although my elementary school had excellent teachers, perhaps the ostensibly minor variable of its location contributed to my climbing the academic ladder. This school was only two blocks from Howard University, the center of black higher education. Some of my fondest memories during my elementary school years were roaming Howard's campus engaging in all sorts of children's games, or wandering through its biology laboratories. During those years, my school friends' choice of adulthood occupations varied as a function of our visits through the campus buildings. At one time it was architecture, later botany, then physics. During these childhood days, I knew I was going to college, but was unaware that I would return to Howard as an undergraduate psychology major.

Like Sarason, I also had a cousin who provided significant role modeling for me. My cousin Bobby, now a prosperous gynecologist, was a Howard University medical student during my elementary school years. He also stopped at our house after classes to conduct his scheduled inventory of our refrigerator and pantry. But most impressive to my young mind was that Bobby always carried tons of books and enjoyed studying them. Even more so, he always received academic honors for his endeavors. Instead of jealousy, my response was basic admiration reinforced by my family's expectation that I would follow in his footsteps. Perhaps the only mistake I made in emulating my cousin's achievements was not pursuing a career in gynecology. In comparison to psychology, it's much more profitable.

My undergraduate experiences at Howard University set the foundation for my subsequent adult experiences as a psychologist. The educational curriculum, typical of many black colleges, was the traditional no-nonsense approach, except for one major difference which many, if not most, black college students are presently denied. My awareness of Howard's unique offering was brought home to me recently when one of my black students commented that I was the first black psychologist she had ever met. My immediate reaction was to discount the student's statement until I remembered that unlike herself, a college senior, my first undergraduate psychology course was taught by a brilliant black psychology professor who also was a significant role model for me.

The remainder of my academic-professional career is fairly standard—graduate school, dissertation, research, and teaching, etc. However, although academic-intellectual pursuits have been fairly easy for me, I have always recognized that my achievements have been keynoted by chance. For example, most of my childhood and adolescent peers did not go on to college and graduate school. Many of my childhood acquaintances are members of what social science has labeled the "culturally disadvantaged." It is my belief that such "options" were also available to me, and I count myself among the too few fortunate blacks who have excelled in the academic arena.

My exceptional developmental experiences have direct implications both for the future of black children and for Sarason's notions of nurture. First, unlike Sarason's claim of a commonality in the Jewish experience, black achievement (and failure) is ineligible for comparisons to other ethnic groups because of its great diversity and heterogeneity which many blacks are beginning to consider as strengths. Second, instead of considering time as a variable in societal amelioration, social scientists need to examine the intensity and pervasiveness of society's commitment to social advancement. Many blacks have long recognized that this nation's dedication to eradicating oppression, racism, and poverty is equivocal, at best. Finally, Sarason needs to understand the true and positive implication of the black adage: Any black man who is not paranoid must be crazy!

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John R. Dill
The City College of the City University of New York

You Have Got to Have a Dream, But It's Not Enough

Having grown up in Jewish families, being psychologists, and having followed Seymour Sarason and his contributions to psychology and human welfare (he completed graduate school the year we were born), we would like to comment on his thesis and then try to pick up where he left off in his recent article (Sarason, November 1973).

On the face of it, Sarason's thesis on the difficulty of using short-term psychological or educational interventions to alter historically rooted and culturally transmitted ways of viewing the world may be accurate. However, we are left with an uneasy feeling about what that thesis implies for the future of psychoeducational or other social change interventions. We fear that Sarason's arguments, left without further comment, may inadvertently be construed to support "benign neglect" and other such forms of status quo social policy. This is particularly critical today when there appears to be a halt to, or movement away from, social change and reform. For example, on our campus last week there was a minuscule demonstration by black students protesting the fact that changes for black people are no longer "in style."

To argue that the power of cultural transmission is so overwhelming that one needs to think in terms of a century in order to see significant alterations in the status of minority groups is most important if the goal is *assimilation*, but less important if *accommodation* is the aim. Viewing the issue in this old-fashioned sociological terminology makes it clear that the problems discussed by Sarason may not simply necessitate strategies for changing minority groups but rather *require* strategies for changing the *majority*. The question is not simply how to change the internalized social attitudes of the minority culture via the socialization process but how to change the objective opportunity structure for minorities, as well as the attitudes and behaviors of the majority.

Although Sarason briefly noted the problem of "blaming the victim," he did not point out, as did William Ryan (1971), that blaming the victim involves the attribution of responsibility for some socially defined problem to the minority individual. The problem may be viewed as a result of genes, home life, ability, background, etc. While Sarason excluded genes, "cultural transmission" encompasses all of the other blaming strategies.

Given that the cultural attitudes transmitted by Jewish and black families have been dissimilar in regard to education, it is understandable that these two cultures will produce different numbers of "successes," as defined by an educational criterion. As Sarason is aware, that is not the only criterion by which groups can be compared. For example, a stereotype comparable to the Jewish intellectual might be the black jazz musician. There is little objective basis for choosing who is more worthwhile—a college professor or a jazz musician. That is a matter of values. An article like Sarason's could be written by a black jazz musician arguing that the problem for Jews is not that they do not have good rhythm or "soul" genes, but that Jewish culture does not emphasize music in the way that his family and friends' and cousins' families do. That would not help to produce more Jewish jazz musicians, to encourage such efforts, or even to understand what it is that makes one a good jazz musician.

It is not debatable that values are in question here. One is bordering on trouble when he implies, however understandingly, *why* this is the case, that one man's family transmits better values than another's. Sarason was clear on the relativity of the criterion when he discussed genetic explanations. He was not clear when he discussed the outcome of socialization processes. His arguments allowed the conclusion, albeit inadvertently, that we need a kind of *social eugenics*, or a means of getting black families to teach their children the proper values, especially the value of education. Of course, this takes a long time.

We could all agree that it is important that the opportunity structures be open to blacks, whites, Jews, or whomever, on the basis of a desire to pursue a given set of values and goals. The problem, however, for blacks in the educational system is not only that there are not enough blacks in the system but, more specifically, that blacks who *potentially* may want to get into the system are not allowed in. This restricting of opportunity does not have to be enforced by traditional and obvious forms of discrimination. Take, for example, a primary-school black child whose mother wants him to be educated every bit as much as did the Jewish mother who lived in the same house several generations before. When this child is taught in school not to value education because the white teacher, who cannot understand black dialect, concludes that this child's family does not give him the "proper" background, we are not dealing with a problem of minority group cultural transmission. We are dealing with a *majority* group cultural transmission that does not afford the minority the equality of educational opportunity. A recent study by Rubovits and Maehr (1973) demonstrated the potentially devastating effects of even positively manipulated teacher expectations on black children.

And what about the positive side? What kind of psychoeducational and social policies do encourage social change? First, programs that support a minority group's strengths, rather than seek out their weaknesses, are imperative. Also desirable are programs that teach standard English as a second language and that start with a child's individual ability to learn transfer skills so as to make use of cultural amplifiers (Cole & Bruner, 1971). Second, working to change the attitudes and behaviors of the dominant cultural majority

seems essential. This does not mean ignoring the problems of the minority but, rather, entering into such problems with the realistic optimism that these are people with as many strengths as any other group.

Gurin and Gurin (1970) had a particularly good analysis of the problem of changing individuals versus changing society's opportunity structure. They applied Rotter's (1954) social learning theory, which argued that the likelihood of a given behavior being performed is a function of the expectations one has that the behavior will lead to some goal, and the reinforcement value of that goal. Gurin and Gurin (1970) argued that for people who are poor, many things have the same reinforcing value as they do for middle-class people; the poor do not want things substantially different. The problem, in this view, becomes the subjective expectations that going to school, getting a job, and working for success in that job will lead to the things one values. For many minority people such expectations are low. To deal with the problem of low expectations is to work at changing the subjective probabilities that a given behavior, such as going to school, will lead to success in life. If a minority person raises his or her expectations in the absence of changes in the external objective opportunity structure (where the real probabilities rather than the subjective probabilities lie) he or she may be moving toward disappointment and failure. On the other hand, if there are changes in the external opportunity structure that are not perceived at the individual level, a minority group member may not take advantage of the changes. From Gurin and Gurin's (1970) analysis it is clear that one cannot ignore the cultural transmission of expectations, but one also must simultaneously work to change the external opportunity structure.

Having discussed several suggestions concerning the content and direction of future social change interventions, we turn now to a consideration of the form of these interventions. Hopefully one would not conclude from Sarason's article that a long-term intervention, that is, a century, is the only meaningful form. Obviously, any contemplated intervention of such long duration is not only politically and personally unrealistic but ignores a rapidly changing world.

On the other hand, we need to engage in a series of short-term interventions in a somewhat paradoxical fashion. We need to have, and more importantly to communicate, high expectations for the efficacy of our short-term interventions, while not being discouraged by our modest accomplishments. Achievement rarely, if ever, matches one's goals; however, if our goals are set and communicated at a minimum, we are bound to accomplish less than if they are set at a maximum. Politicians think of massive social change occurring during a single term of office. We must not let this phenomenon lead to pessimism and the curtailment of continued thought and energy on our part. Instead, we must continue to push forward with the next phase and the concomitant revision of specific goals and methods in quest of our long-term aims.

Failure, or more realistically partial failure and partial success, is not *only* negative. The feedback from this failure leads to progress. The fact that we lost the war on poverty taught us a great deal about how social change requires more than working with the victims, even if we allow them to participate in their own "cure." The civil rights era taught us that laws that supposedly change the opportunity structure are also insufficient for social change. We did not know that until we tried it. Even with failure, there are now more blacks in visible places, for example, on television, in universities, to model for their younger counterparts. Failures can lead to future successes; we should not fear failure.

In pursuing social change we need to set a series of short-term, specific, and realistic goals, as well as long-term aims. We need to work with the powerful and the majority as well as the powerless and the minority. We can afford the perspective of a century in the solitude of our offices but not in the streets of our cities. We must put together a series of modest advances, all aimed toward a larger future perspective. Our errors in the past have not been due to overoptimism; they have more likely been due to the failure to specify short-term and specific goals in conjunction with our larger dreams.

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EDWARD SEIDMAN
JULIAN RAPPAPORT
University of Illinois at Urbana-Champaign

Jewishness, Blackishness, and the Impending Retreat of Psychology

I would like to commend Professor Sarason (1973) for writing his article on *Jewishness* and *blackness* in such a personal, meaningful genre. This kind of essay has a vital, illuminating power and has largely been underused in psychology. I have recently returned from spending two years in Tanzania and one month in Israel where I had the opportunity to observe Jews and blacks in other than an American context. Having evolved some ideas from the perspective afforded by a prolonged separation

even the "familiar." I would like to take issue with both the spirit and conclusions of Sarason's article.

What seems to be of critical importance about the Sarason article is the timing of its appearance. On returning to the United States, I was struck by the drastic shift of mood concerning the involvement of psychology in social affairs. The middle and late 1960s saw many psychologists alter their focus of applied and theoretical interest to the "system" rather than the individual (Sarason, Levine, Goldenberg, Cherlin, & Bennett 1966). This spirit was exemplified by the development of programs, journals, and even the tenuous title "Community Psychologist." The 1970s, however, seem to be typified by the rapid erosion of optimism concerning social change and a feeling of disillusionment with the application of the social sciences to solve problems of poverty, racism, and the broad spectrum of cultural evils.

Though Sarason drew no explicit conclusions concerning psychology's future in social affairs, remarks such as "What centuries have produced will not quickly change even under external pressure [p. 967]," certainly have a pessimistic ring. If it is true that psychology is, in fact, withdrawing from its very short-lived assault on the social system, I am very fearful that Sarason's article will provide a rationale for the retreat. It is therefore imperative that some serious flaws in his rationale be identified and discussed.

I should mention that I, like Sarason, am Jewish and can identify with many experiences to which he makes reference. He argued that historically determined characteristics, such as valuing intellectual accomplishment are powerful and difficult to change. He stated, *"These aspects have been and will continue to be immune to change in any short period of time, by which I mean a minimum of a century* [p. 964]."

However, when he attempted to support the position by making reference to Israeli Jews, the argument was weakened rather than strengthened. Israel provides a perfect example of people in a different situational context, making a rapid and stable transformation. The change from passive victims of fascist terror to a militant people determined to defend their lives and land to "the end" did not take a century, or even generations. Rather, the children of immigrants from Europe and elsewhere have emerged as a *new kind* of person (and Jew), heir to some of the traditions (the intellectual), but with an essentially unique character.

One can also look to a country such as Tanzania to see the same rapid transformation of individuals. Students in both secondary- and university-level training have in one generation moved from a simple peasant social heritage to modern institutions of learning, and have performed admirably. Men and women whose childhoods were spent in huts without electricity, plumbing, or books are now pursuing graduate-level degrees. This phenomenon is occurring in developing countries around the world and I would urge Sarason to take notice.

What Sarason has perhaps identified accurately is the fact that American compensatory programs have not accomplished miracles and that there have not been dramatic changes for blacks in the intellectual arena. His conclusion, however, that change will take "centuries" is both unwarranted and as socially irresponsible as the Jensen (1969) article. If I may borrow from Sarason's analogy of the child who does not learn to read, one need not necessarily conclude that it will take *the child* years to change, thus putting the blame for failure on *the victim*. I do not mean to invoke timeworn platitudes but rather to remind Professor Sarason that programs are often irrelevant, limited in scope, unimaginative, and hence produce no obvious consequences.

Psychology has not even scratched the surface concerning the difficulties of American blacks. Our programs, to date, have been at best piecemeal and have represented isolated and sometimes hasty attempts to correct the mistakes of history. What has made the difference in places like Tanzania and Israel is the comprehensiveness and consistency of the programs. It would be an understatement to say that we have really not seen any substantial consensual national efforts to deal with the problems of our oppressed minorities.

I take for granted that historical and cultural forces operate to make change a conservative process. However, it is critical to recognize that the process of change can either be facilitated or obstructed by systemic factors. To date, the social and economic environment in the United States has dramatically impeded the aims of any restorative efforts. As one psychologist, I cannot accept the cynical conclusions of Sarason. I genuinely hope that the many psychologists who have lost their direction in the community do not welcome the article as license for abandoning their social responsibilities.

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HERBERT RAPPAPORT
Temple University

A Reply to Gillen

Gillen (November 1973) reported Flesch formula ratings of reading ease and human interest for 34 introductory texts. This report represents a basic attempt to quantify two factors that might be considered in the selection of a textbook. Our response to the aim of this report is favorable. However, we are skeptical concerning the value of these reported ratings and we remain convinced of the need for more compre-

because programmatic research to guide decisions about textbooks.

Our skepticism stems from three concerns: (a) no substantial validity has been established for the use of such ratings with introductory psychology texts, (b) the ratings are not based on a comprehensive analysis of physical characteristics, and (c) no mention is made of the variability across sampled passages within the textbooks (Stevens & Stone, 1947).

The only reference to the validity of Flesch ratings for textbooks in Gillen's report was to an article by Stevens and Stone (1947). These authors reported agreement between Flesch ratings and ratings of readability made by students and teachers. Striking differences in both content and structure are revealed by an examination of the textbooks in the Stevens and Stone sample from more than 25 years ago and Gillen's sample today. Flesch ratings do not reflect variability from many of the sources that are more prevalent in the modern introductory textbook. To the extent that raters are influenced by the additional sources, there will be a reduction in the agreement between Flesch ratings and the ratings by judges. McKeachie (1969) has indicated a more important validity concern. If the aim is selection of a textbook, the most important consideration is the utility of the textbook in helping to meet particular course goals.

The second concern from which our skepticism arises relates specifically to the measurement of physical characteristics of modern textbooks. Flesch ratings are based on limited samples of continuous verbal text. Anastasi (1964) stated that one common criticism of the Flesch ratings is that they do not reflect many sources of variability in the verbal text, such as organization, use of inappropriate words or circumlocutions. Additional criticism must be made when Flesch ratings are obtained for introductory texts. A modern introductory text does not typically communicate solely through a continuous presentation of verbal text. Many devices are employed (graphs, tables, photographs, illustrations, chapter outlines, and chapter summaries) are commonly used. Many texts also contain relatively unique characteristics such as illustrative autobiographical inserts in the margin and offset boxes with empirical findings. Comprehensive measurement is a feasible method and should be considered an essential foundation. One workable measurement procedure can be developed to parallel Brown and Owen's (1967) approach to forms. Available measures can be applied to the sample of textbooks, and a factoring process can be used to yield physical dimensions. Subsequently, the utility of the resulting dimensions can be determined relative to judges' ratings. Most importantly, relationships can also be determined between factor loadings for texts and success in meeting particular course goals.

The third concern underlying our skepticism centers on the fact that no mention was made of variability across passages within textbooks. This can ultimately be included as one measure in a more comprehensive measurement procedure. Stevens and Stone (1947) lent some attention to the standard deviations in their report. Rosenzweig (1947) suggested that more extensive use be made of the standard deviations reported by Stevens and Stone. For the sample of texts by Gillen, such a measure may have proven even more interesting than it did with the Stevens and Stone sample. The increased interest and information from this measure stem from differences in the two samples. Gillen's sample contained books written by one author and many authors (e.g., Morris versus CRM). In addition, the books in Gillen's sample are typically suited to "basic processes" courses. Variability across topics (e.g., physiological psychology and social psychology) may be considerable and may reflect more than varying a literary style to ease the reading.

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DAVID T. LANDRIGAN
JOSEPH J. PALLADINO
Fordham University

A Cautionary Note

Gillen (November 1973) attempted to provide instructors of introductory psychology courses with readability and human interest scores for a number of general psychology texts. Such an endeavor may be of value to some instructors in making text selections, but I feel that Gillen's information did not provide an up-to-date evaluation of several of the books that he surveyed.

Specifically, Gillen overlooked in his analysis the fact that at least six of the books which he evaluated have come out in revised editions. These revisions include Deese (1967), McKeachie and Doyle (1970), Ruch and Zimbardo (1971), Hebb (1972), Munn, Fernald, and Fernald (1972) and Cox (1973). Furthermore, revisions of Kimble and Garmezy (1968) and Morgan and King (1971) are currently in preparation.

In fairness to the authors of the above texts, their revised editions are probably somewhat more readable and more human-interest oriented than were the original editions. I make this assumption because (a) feedback from publishers to authors about revisions can be expected to dictate such a course of action; (b) the fact that the books are judged worthy of revision should be some index of how users view the texts.

and a there seems to be a strong competitive trend within the textbook industry to put out introductory psychology texts which sustain student interest by including more socially relevant material (e.g., Bourne & Ekstrand, 1973; Buss, 1973; Dushkin Publishing Group, 1973; and Gazzaniga, 1973). In this regard, it would be interesting to see how Flesch ratings of the *original* versus the *revised* edition of various introductory texts compare. If such a comparison reveals significant differences, then Gillen's rankings would have to be updated.

In closing, I would like to emphasize that assessing the readability and interest value of instructional texts is a legitimate problem which instructors must consider. An endeavor should be made, however, to constantly update such rankings as revisions and new books are added to the introductory psychology textbook market.

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ROBERT ALLAN YOUTH
University of North Carolina at Chapel Hill

Psychic Determinism versus Freedom of Choice: Order versus Chaos

This statement is a reply to Tyler's (December 1973) article and a clarification of what determinism implies for psychology and society. Tyler presented some of her thoughts and supporting philosophical writings for the hypothesis that we have freedom of choice even though our actions and thoughts are partly determined. I propose that orderly thought or behavior, which is based on a sequence of choices, is either determined by the external environment or a combination of the latter plus internal causes or control. Behavior which is not determined by some combination of external and internal control, that which is completely free, is random, chaotic, and possibly detrimental to the behaving individual and the society in which he lives. If Tyler agrees with this definition of behavior, whether or not she agrees that determinism exists, then I would not disagree with her statements. However, if she denies determinism, whether external or internal, and proposes complete freedom of choice, I would definitely disagree with her proposal.

Furthermore, by assuming internal as well as external control of behavior, when an individual makes a choice he can essentially individually determine his course of action. To assume he has complete freedom of choice is an absurdity, because this would imply estrangement from oneself or at least an individual unconscious of his own motivations and goals. Even if such a mode of existence were possible, I doubt whether such an individual would help society.

I therefore submit that a psychologist who recognizes his own motivations and goals is freer, in the sense that Tyler emphasized, than one who always is compelled to judge his research or behavior in terms of what "society" considers profitable. Researchers have often rebelled against accepted doctrines of their times, resulting in contributions to the future. Research psychologists are not very different from other researchers in areas such as physics, biology, chemistry, or medicine. If at every step of a research project the investigator had to question his actions in terms of the immediate benefit to society, he would become the proverbial armchair psychologist.

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STANLEY MORGANSTERN
William Paterson College of New Jersey

Is the "Design for a Hopeful Psychology" Working?

It was heartening to read "Ethical Principles in the Conduct of Research with Human Participants" prepared by Cook and other members of the Ad hoc Committee (APA 1973). Like earlier APA publications regarding ethical standards, these principles were based on incident reports giving them an empirical as well as an idealistic base. A most welcome change was the substitution of participants for subjects.

Then came Tyler's (December 1973) address suggesting, among other things, that concern for the participants might really be covering up results. Then one might anticipate Tyler's "Design for a Hopeful Psychology" to be already reflected in the recent experimental literature. On the very next page of the *American Psychologist* however, appeared the article by Menges (1973), citing evidence that students tend to lose the amount of deception in experiments increased in contro-

journals, while remaining about the same in others.

During the winter academic quarter ending in March 1974, I conducted a seminar entitled "Ethical Principles in Educational Research and Development." While this seminar was offered as a graduate education course rather than as a psychology course, the students agreed to use as their guidelines the APA publications that formulated ethical principles. After the students demonstrated knowledge of and concern for the ethical principles stated in the document by Cook and others (APA, 1973), they were asked to search journals reporting educational research. They reviewed articles published mainly between 1970 and 1973 in the *Journal of Educational Psychology*, the *Journal of Experimental Education*, the *Journal of Educational Research*, and various journals for teachers such as *The Mathematics Teacher*. No experiment involving deception was found in the journals for teachers, but in the other journals experiments involving deception, lack of debriefing, and other ethical questions were located. Because of the small sample of the literature as compared to the sampling by Menges (1973), no quantitative data are reported here. It is sufficient to say that a dozen or so experiments were found that would have been included in a sampling procedure such as that used earlier by Menges.

While direct comparisons of the 1970–1973 period cannot be made with the earlier period reported by Menges, it is clear that deception and other similar issues have not disappeared from the literature searched. In fact, not one of the dozen articles reported included any *discussion* of possible ethical problems related to the experiments.

The main purpose of this comment is to ask whether editors should require a discussion of ethical as well as technical aspects of experimental procedures. If graduate students can identify articles for which such discussion was needed but was lacking, should responsibility for such a discussion be left to reviewers (as was the case for all journals except one, as reported by Menges)? Should APA, as a body, advise editors of APA journals on this question? Should not the articles in APA journals model the behavior held up as standards in official APA statements of ethical principles?

Of course, journals not sponsored by APA have no obligation to adopt APA standards. But what is the obligation if an editor of a non-APA journal is himself a member of the APA?

For articles relating to the ethics of experimentation, if frequency of appearance in the *American Psychologist* is a measure of our concern, then have we done all we should in encouraging authors and editors of other APA journals to report and discuss debriefing and other procedures when appropriate?

The students in our seminar were not surprised about the experimental procedures themselves but were surprised that neither authors nor editors saw fit to report the safeguards or conferrals with colleagues that might have been employed. The students made no judgments concerning the ethics of deciding to perform the reported experiments because authors did not include the reasons for their decisions nor how they arrived at them. In many cases, however, the students did doubt that the experiments were important enough to offset the ethical questions left dangling.

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Leslie J. Briggs
Florida State University

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AMERICAN PSYCHOLOGICAL ASSOCIATION

In accordance with the established nominating and election procedures, the next President-elect of the American Psychological Association will be Wilbert J. McKeachie.

The American Psychological Association, founded in 1892 and incorporated in 1925, is the major psychological organization in the United States. The purpose of the APA is to advance psychology as a science, and as a means of promoting human welfare. It attempts to accomplish these objectives by holding annual meetings, publishing psychological journals, and working toward improved standards for psychological training and service.

APA Revises Its Publication Manual

ANITA DEVIVO *American Psychological Association*[1]

This month APA publishes a new edition of the *Publication Manual*, the first revision since 1967. The new Manual, a year in preparation, replaces the familiar green-covered 1967 Manual. It incorporates suggestions of authors, editors, psychology department chairs, students, typists, printers, and publishers who use the Manual most frequently, and reflects the experience of the APA journal staff in preparing more than 2,000 manuscripts a year for publication. As an expanded second edition, it is a timely, comprehensive handbook for students of psychology, and for psychologists (and perhaps other social scientists) who write for publication.

A guide for the preparation of manuscripts is a long APA tradition. The first one appeared as a 7-page article in the *Psychological Bulletin* in 1929 when APA had 893 members. It was written by a committee of editors and managers of scientific journals for the National Research Council and was intended for science writers in general. The guide bluntly states:

> The writer who is incompetent in spelling, grammar, or syntax should seek help. . . . A badly prepared manuscript always suggests uncritical research and slovenly thinking [Conference of Editors, 1929, p. 58].

Four subsequent guides more specifically addressed APA journal authors. These appeared in 1944 (a 32-page article in the *Bulletin*); in 1952 (a 60-page supplement to the *Bulletin*); in 1957 (a separate publication); and in 1967 (a separate publication). The title *Publication Manual* was first used in 1952.

As with the original guide, writing the Manual has continued to be a joint venture. Journal editors authorized John Anderson and Willard Valentine to prepare the 1944 instructions. The editors themselves undertook the writing of the 1952, 1957, and 1967 manuals, with Laurance F. Shaffer as coordinating editor. The new second edition is largely the combined writing efforts of the APA journal office, Susan Bunker in particular; and APA members Charles Cofer, Robert Daniel, Frances Dunham, Walter Heimer, and William Mikulas working as a task force of the Publications and Communications Board.

Why a Manual?

E. B. Strunk remarks in the closing pages of *Elements of Style:* "If one is to write, one must believe—in the truth and worth of the scrawl, in the ability of the reader to receive and decode the message [Strunk & White, 1972, p. 77]." One of the ways readers receive and decode printed material is through the conventions that grow out of usage: capital letters in important places, italics in special places, quotations in small type, lists in a column, footnotes at the bottom of a page. The conventions increase in number and complexity for readers of scientific literature who learn to expect orderly citation of references, and common forms

[1] Anita DeVivo is executive editor of APA's journals and directed the revision of the *Publication Manual*. Copies of the new 136-page Manual are available for $3 from APA. Orders of $15 or less must include payment unless they are submitted on institutional purchase forms.

for abstracts, tables, figures, and mathematical display.

The purpose of a publication manual is to record and sometimes establish such conventions so that authors, editors, and printers may refer to it as a manuscript moves from typewriter to printing press. Editors and reviewers filter a manuscript for stylistic problems that might affect content. Technical editors copy-edit for clarity of expression, for consistency of style within a journal and within the literature, and for consistency with printing requirements.

This continuing function of the APA Publication Manual is impressive when APA's journal publishing efforts at the time of the first guide are compared with the present volume of publication. In 1929, APA published 4 primary journals containing 200 articles selected from an unknown number of submissions. Today, APA publishes 14 journals, which in 1973 contained 2,500 articles selected from 6,000 submissions. The population that received the 1929 guide was around 900. The population that received the 1967 Manual was 300,000. In sheer economic terms, manuscripts that follow the instructions of the Publication Manual are handled more quickly, cost APA less in editing and printing dollars, and authors less in alterations. In terms of communication, articles published according to the Manual give a consistency and authority to APA literature, and meet readers' expectations of quality in the printed word.

DIFFERENCES BETWEEN EDITIONS

Authors who are familiar with previous editions of the Publication Manual will note in the new edition some changes in emphases as well as points of style. The Foreword states that the Manual "recognizes alternatives to traditional forms." Some examples of breaks with tradition are new requirements for metrication and the loosening of formal expression. In the chapter on writing style, authors will find that "I" and "we" are often acceptable substitutes for the traditional third-person, passive voice construction (It was found that . . .). Consistent with its trend toward simplification, the Manual eliminates unnecessary underlines (in an a, b, c series for instance), brackets, and other devices that are more ornamental than functional. A summary of such changes appears below.

In format, the new edition is designed for authors and typists. Examples appear in typewriter type, and a complete manuscript illustrates the chapter on typing. Proofreader's marks, a checklist of common oversights, and model reference citations are placed for easy access. All material is numbered and cross-referenced.

Summary of Changes in the Second Edition of the APA Publication Manual

Publication Policies

1. Authors may no longer request early publication.
2. Authors are responsible for depositing any material for auxiliary publication. APA no longer performs this function.
3. The *Journal of Experimental Psychology* will be published in sections in 1975.

Manuscript Organization

1. Article title and by-line no longer carry footnote numbers.
2. Authors now supply cover sheets and running heads.
3. An appendix is occasionally acceptable.
4. Nonretrievable references cited in the manuscript are listed in a new "Reference Notes" section.

Manuscript Preparation

1. Some journals now require three copies of a manuscript.
2. Some journals now employ blind review procedures.

Style

1. *Webster's New Collegiate Dictionary* (1973) is the authority for spelling and abbreviation.
2. The abbreviations *S*, *E*, and *O* are no longer used in APA journals.
3. Metric units are used.
4. Statistical presentation used in the *Journal of Experimental Psychology* is now used in all journals.
5. In displayed equations, equation numbers are enclosed in parentheses, not brackets.
6. The letters (a), (b), and (c) in a series are not underlined.
7. The source of a quotation is cited in parentheses, not brackets.
8. Rules are to be used in tables only when necessary for clarity.

References

1. References include only retrievable material.
2. Arabic numerals are used exclusively in reference lists.
3. Volume numbers appear in italic rather than boldface type.
4. The form of reference citation is simplified.

TRANSITION TO THE NEW EDITION

Differences in style from page to page of a journal are distracting and confusing. To avoid the possibility of error, APA will continue to publish articles in the present style until the end of 1974. All manuscripts to be published in 1975 will be copy-edited to conform to the new second edition. Starting in 1975, manuscripts that depart significantly from the new Manual will be returned to authors for adjustment.

CHANGES BETWEEN REVISIONS

Changes between revisions of the Publication Manual cannot always be anticipated. They may occur in APA policy, printing requirements, or in psychology itself. Users of the new Publication Manual can refer to the *American Psychologist* for change notices that will be keyed to the Manual. Notices will be issued as necessary and listed in the *AP*'s Table of Contents and annual Volume Contents for readers who need a comprehensive review.

Conclusion

Before the revision of the 1967 Manual started, APA invited suggestions from psychology department chairs. The new Manual incorporates two of their suggestions as appendixes—one on the preparation of student papers, and another listing 71 non-APA journals who indicated to APA that they use the Publication Manual (see Appendix). In acknowledging this broader population of users, the Manual increases its influence on the preparation of future psychological literature. Even as this influence grows, publishers are seeking economically feasible means to continue to produce the significant literature. Ironically, while students learn the disciplines of style for printed articles, more and more psychologists are learning to use microform and data banks. As Strunk observed:

> The language is perpetually in flux: it is a living stream, shifting, changing, receiving new strength from a thousand tributaries, losing old forms in the backwaters of time. To suggest that a young writer not swim in the main stream of this turbulence would be foolish indeed, and such is not the intent of these cautionary remarks. The intent is to suggest that in choosing between the formal and the informal, the regular and the offbeat, the general and the special, the orthodox and the heretical, the beginner err on the side of conservatism, on the side of established usage. No idiom is taboo, no accent forbidden; there is simply a better chance of doing well if the writer holds a steady course, enters the stream of English quietly, and does not thrash about [Strunk & White, 1972, p. 76].

The Publication Manual is based on the certain logic of converting typewritten manuscripts to traditional printed forms. As such, it becomes a model for future guides dealing with alternative forms of publication, and ultimately, I suppose, a third edition of the Publication Manual.

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(*See Appendix on page 580*)

APPENDIX

Non-APA Journals Using the APA Publication Manual

Although the APA *Publication Manual* is intended for authors writing for APA journals, many related journals instruct their authors to refer to the Manual in preparing their manuscripts. Some journals recommend the entire Manual, and others recommend only the reference style. The following list includes the names of some non-APA journals using the Manual at the time the second edition went to press. These journals have indicated that they plan to use the second edition.

American Educational Research Journal
American Journal of Community Psychology
American Journal of Mental Deficiency
American Journal of Psychology
Animal Learning & Behavior

Behavior Research Methods & Instrumentation
Behavior Therapy
Behavioral and Social Science Teacher
Bulletin of the Psychonomic Society

Canadian Journal of Behavioural Science
Canadian Journal of Psychology
Canadian Psychologist
Child Care Quarterly
Child Development
Clinical Social Work Journal
Cognitive Psychology
Community Mental Health Journal
Counseling and Values
Counselor Education and Supervision

Elementary School Guidance and Counseling

International Journal of Clinical and Experimental Hypnosis
International Journal of Group Tensions
International Review of Applied Psychology

Journal of Abnormal Child Psychology
Journal of Applied Behavior Analysis
Journal of Applied Behavioral Science
Journal of Applied Rehabilitation Counseling
Journal of Autism and Childhood Schizophrenia
Journal of College Student Personnel
Journal of Cross-Cultural Psychology
Journal of Employment Counseling
Journal of the Experimental Analysis of Behavior
Journal of Experimental Child Psychology
Journal of Experimental Social Psychology
Journal of Homosexuality
Journal of Humanistic Psychology
Journal of Individual Psychology
Journal of Marriage and the Family
Journal of Mathematical Psychology
Journal of Motor Behavior
Journal of Non-White Concerns in Personnel and Guidance
Journal of Personality
Journal of Research in Personality
Journal of School Psychology
Journal of the Student Personnel Association for Teacher Education
Journal of Verbal Learning and Verbal Behavior

Learning and Motivation
Life-Threatening Behavior

Measurement and Evaluation in Guidance
Memory & Cognition
Mental Retardation
Monographs of the Society for Research in Child Development
Multivariate Behavioral Research

The Ontario Psychologist
Organizational Behavior and Human Performance

Perception & Psychophysics
Perceptual and Motor Skills
Personnel and Guidance Journal
Personnel Psychology
Physiological Psychology
The Psychological Record
Psychological Reports
Psychology
Psychometrika
Psychophysiology

Rehabilitation Counseling Bulletin
Representative Research in Social Psychology
Review of Educational Research

The School Counselor

Training School Bulletin

The Vocational Guidance Quarterly

Albert, Peter, and John B. Watson

MARY COVER JONES *University of California, Berkeley* [1]

I was first introduced to John B. Watson at a lecture, which included motion pictures, in New York City in 1919. I had gone from Vassar College to New York for a weekend and was steered, more or less accidentally, by a friend to this lecture, rather than to the usual Friday night theater.

John Watson's lecture and movies reported his observations and experiments at Johns Hopkins University and set forth his proposition that there were three basic emotions present at birth—fear, rage, and love—that were called out by *specific* but limited stimuli. More elaborate emotional responses were learned by association, or conditioning. To demonstrate his thesis, Watson chose 11-month-old Albert (according to Watson, "a child with a stolid and phlegmatic disposition") as the subject of his conditioning experiment. As is well known, a loud sound, which called out the fear response, was coupled with Albert's positive response of reaching interestedly for a white rat of which he showed no fear.

After several associations of the startling sound with the presentation of the rat, Albert not only withdrew in fright from the rat but the negative reaction to the rat eventually persisted without reinforcement of the loud sound. As far as we know, this was the first laboratory attempt to condition an emotion in a child. Transference had also occurred to a white rabbit, to other furry objects, and even to a Santa Claus mask with a white fuzzy beard (Watson & Watson, 1921)!

The possibility of using the learning approach in the modification of behavior appealed to me. If fears could be built in by conditioning, as Watson had demonstrated, could they not also be removed by similar procedures?

I believe I was the last graduate student fortunate enough to work with Watson. (If there is any value in having me here today, it is to illustrate how events may shape lives.) This is how it came about: Watson was by that time in exile from Johns Hopkins University and was associated with the J. Walter Thompson Advertising Company in New York City. I was a graduate student at Columbia. Not to be suspected of trading on the sensationalism of this period in Watson's life, in the following account I quote from Watson's (1936) own autobiographical statement to indicate how his move from Baltimore to New York occurred:

> 1919 [was] my last full year at Hopkins. At this time, my infant work was in full swing. . . . All of this work came abruptly to a close with my divorce in 1920. I was asked to resign. . . . I got my first business tryout with the J. Walter Thompson Company. . . . This [was in the] Fall of 1920, the whole of the trying time when I was front-page news in Baltimore [p. 279].

R. S. Woodworth (1959), in a memorial statement at the time of Watson's death, said of this episode: "Misfortune overtook Watson in the shape of a sensationally publicized divorce suit [p. 302]."

Rosalie Raynor Watson, his second wife, who figured in the divorce suit, was a Vassar classmate and a friend of mine. It was through our association that I had the opportunity to work with her husband.

And so we come to Peter. It has always been of the greatest satisfaction to me that I could be associated with the *removal* of a fear when I came in contact with this three-year-old in whom a fear of animals was already well established (Jones, 1924). I could not have played the role of creating a fear in a child, no matter how important the theoretical implications. When my own children came across the case of Peter in their college textbooks, they too were relieved to find that I had functioned in this benign capacity in the psychological experiments with children and children's fears. Watson (1924a) also underscored this point in a lecture included in his book *Behaviorism:* "Finding that emotional responses could be built in with great readiness

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[1] This article is a slightly revised version of a paper presented at the First Annual Southern California Conference on Behavior Modification, Los Angeles, October 1969.

Requests for reprints should be sent to Mary Cover Jones, Institute for Human Development, 1203 Tolman Hall, University of California, Berkeley, California 94720.

[Albert], we were all the more eager to see whether they could be broken down, and if so by what methods [p. 132]."

The patient, meticulous, painstaking procedures used in the experiments with Peter reflect the careful methodological style of John B. Watson, who faithfully followed Peter's progress, reverses, and final freedom from his fear. Watson paid us a professional visit on most Saturday afternoons throughout the conduct of the therapeutic sessions.

This brings us to John B. Watson himself, founder of the school of behaviorism in the United States. In 1913 Watson issued what Woodworth (1959) called the "behaviorist manifesto." The study of behavior was not only to be free from the domination of consciousness but was to take entire possession of the field of psychology (Watson, 1913). The manifesto initiated rebuttals from classical psychologists. Titchener, a member of the traditional school, published a critique in 1914, but very fairly described the positive side set forth by Watson as "the working out of general and specific methods for the control of behavior, the regulation and control of evolution as a whole [p. 3]." It is primarily this aspect of Watson's doctrine that interests the present-day behavior modification movement.

Other professional antagonists have been less kind, less fair, less understanding. Henry Murray (1960) accused Watson of having seized upon Pavlov's formulas, "[labeled] it 'behaviorism,' and sold [it] to all but a few American psychologists as the only model on which to build a . . . science [p. 6]."

As graduate students at Columbia University, my husband, Harold E. Jones, myself, and other members of our student group were among those to whom Watson "sold" behaviorism. I can still remember the excitement with which we greeted Watson's (1919) *Psychology from the Standpoint of a Behaviorist*. It shook the foundations of traditional European-bred psychology, and we welcomed it. This was in 1919; it pointed the way from armchair psychology to action and reform and was therefore hailed as a panacea.

Had Watson stayed in the psychological laboratory, I am sure that his theoretical position would have become more dynamic, more mellow. He would have modified an oversimplification of his concept of native disposition in view of more complex aspects of human development and functioning that were coming to his attention in our studies. He saw my own daughter in our home, placidly playing through a loud sound that he expected would cause a traumatic response, just as it had to Albert, alone in a strange situation (Watson, 1924a, footnote, p. 121). The conclusions based on observations published by Jones and Jones (1928) could have been his: Children tend to show timidity or fear of the unexpected and unusual; and fears develop when the individual knows enough to recognize his inadequacy to cope with a strange or sudden experience.

And what about Watson as a person? Henry Murray (1960) expressed one point of view when he called him "that persuasive, one-tracked, charismatic, timely publicist [p. 6]." But for many years Watson had been a respected experimental psychologist, and, as late as 1924, though he labeled Watson's behaviorism "crude," Titchener (1914) considered a paper by Watson (1924b) in *Psychological Review* to be "[one of] the most important of the year [Larson & Sullivan, 1965, p. 348]." Angell, a former associate of Watson's when he was at the University of Chicago, though unfriendly to his behavioristic views, nevertheless stated: "For much of his actual work I have very high regard, as I have for him personally [Larson & Sullivan, 1965, p. 342]."

In his autobiography, Watson was apologetic about two books that were published after he left the academic field, *Behaviorism* (1924a) and *Psychological Care of Infant and Child* (1928). But his ability to acknowledge imperfections was an admirable quality that should recommend him to us. Referring to these two books, he said: "*Psychological Care of Infant and Child* was another book [in addition to *Behaviorism*] I feel sorry about . . . because I did not know enough to write the book I wanted to write [Watson, 1936, p. 280]." This is the book for which generations of mothers, including my own, have flayed Watson. He himself quoted one parent, "a dear old lady," who said, "Thank God, my children are grown and that I had a chance to enjoy them before I met you [Watson, 1928, p. 69]." The following is one reason for the distress that mothers expressed. Watson advised: "Never hug and kiss them [children], never let them sit on your lap. If you must, kiss them once on the forehead when they say good night. Shake hands with them in the morning [Watson, 1928, pp. 81–82]."

Watson was admirable in his willingness to criticize himself. He was also generous in giving credit

to others far beyond the professional requirements. He would not coauthor my papers because, as he said, his name was well known, and I still had my reputation to make. He wanted me to have all the recognition. In his book *Behaviorism*, he wrote: "While I spent considerable time as consultant and helped plan the work, Mrs. Mary Cover Jones conducted all of the experiments and wrote up all of the results [Watson, 1924a, p. 132]."

His expression of gratitude and appreciation to fellow scientists sounds like a roll call of the prominent names of the period: Titchener, Angell, Yerkes, Dunlap, J. McKeen Cattell, Adolph Meyer, Lloyd Morgan, and Edward Thorndike, to mention just a few. A remark in reference to a period of sleeplessness and anxiety in Watson's university graduate days may come as a surprise to those who see him in juxtaposition to Freud: "It . . . prepared me to accept a large part of Freud, when I first began to get really acquainted with him around 1910 [Watson, 1936, p. 274]."

Watson was magnanimously appreciative of his contemporary scientists. He also regularly and publicly acknowledged financial support, even including that munificent sum of $100 contributed by the American Association for the Advancement of Science for his work with infants, which gave us, among other outstanding contributions, the case of Albert.

You may have noted that I have never assumed a first-name basis in talking of John B. Watson. Our relationship was more formal than not. This may be due to the custom at that time for contact between colleagues, especially in the student–teacher relationship, to remain impersonal. Watson regularly addressed his colleagues in correspondence by their last names—as Dear Yerkes or Dear Titchener (Larson & Sullivan, 1965). I never lost the feeling of awe that I had for Watson, the scientific innovator. It would have been very uncomfortable for me to have called him John. This may be only a reflection on professional style in the 1920s, but I suspect it tells you something about our respective personalities as well.

A discussant of Watson's school of behaviorism said: "The strength of the movement lies not in its fundamental logic but in its laboratory performance [Larson & Sullivan, 1965, p. 348]." It is this aspect of Watson's contributions with which I am most identified and which I most appreciate.

We now use the term *behavior modification therapy* to describe the practical approach of John B. Watson and his followers. I welcome this opportunity to acknowledge with gratitude our indebtedness to this impressive figure in American psychology.

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A Taxonomy of Instrumental Conditioning

PAUL J. WOODS *Hollins College*[1]

The history of science contains numerous instances whereby a classification schema has proved to be of value in organizing our knowledge of the world (e.g., see Hall & Hall, 1964, pp. 95–108). Various taxonomies have been developed in the biological sciences for classifying plants and animals; in chemistry the periodic table of the elements is a prime example which illustrates not only the organizational value of such a system but its *discovery value* as well. Before Mendeléeff arranged the elements according to their atomic weights, the discovery of new elements had been essentially "accidental." The classification by atomic weight, however, allowed the known elements to be grouped into families provided that empty cells were left in the table. Mendeleeff not only successfully predicted that elements would eventually be discovered to occupy these empty spaces but he also was remarkably accurate in predicting many of the properties of these yet-to-be-discovered elements (Cragg & Graham, 1954, pp. 28–31).

For the most part, classification schemes in other sciences have dealt with "things" having physical properties, such as plants, animals, rocks, basic elements. Classifications based on *operations*, however, as have been evolving in the psychology of learning (Grant, 1964), are not without precedence (e.g., see Serfass, Steinhardt, & Strong, 1950). Furthermore, there seems to be no logical reason why the rules for classificatory schemes cannot be applied to experimental operations (Cohen & Nagel, 1934, pp. 241–244).

In their classic work *Conditioning and Learning*, Hilgard and Marquis (1940) described four types of instrumental conditioning procedures: reward training, escape training, avoidance training, and secondary reward training. In their notes following the chapter on instrumental conditioning, they presented a historical summary of the distinction between classical and instrumental conditioning and an additional discussion of the above four types. In retrospect, it seems somewhat curious that they considered secondary reward training as a separate type rather than as simply another way to use the reward training paradigm itself. (It is also interesting to note how far back some of this work extends. As early as 1910, for example, Glaser reported using an escape paradigm to motivate rats in maze learning.)

In his revision of Hilgard and Marquis' *Conditioning and Learning*, Kimble (1961) also discussed reward and avoidance training but considered escape training to be a subvariety of avoidance. In addition, he noted another variation of the avoidance paradigm in which the exteroceptive "warning" stimulus is omitted (cf. Sidman, 1953a, 1954b). Kimble (1961) then introduced two new categories of instrumental conditioning: (*a*) one in which "a specified response would lead to the non-occurrence of a positive reinforcer" and (*b*) one in which "it would lead to the occurrence of a negative reinforcer [p. 70]." For each of these categories, Konorski (1948) reported that an organism learned a response that was *antagonistic* to the one called for by the experimental paradigm. In the first of these two new categories, the organism received a positive reinforcer for *omitting* or failing to emit a specified response. Thus, Kimble termed this "omission training." The second category was punishment training in which a response was followed by an aversive event. Although Hilgard and Marquis (1940) discussed this, it was not included as a type of instrumental conditioning because they considered it a "method for the elimination rather than the acquisition of a response [p. 74]."

The next advance was made by Grant (1964), who introduced the first true classification scheme and, in the process, illustrated its discovery value as well. He suggested a classification based on three dichotomous dimensions. (*a*) whether there was a "cue to impending reinforcement", (*b*)

[1] This work was supported by Public Health Service Research Grant M 02551 from the National Institute of Mental Health.

Requests for reprints should be sent to Paul J. Woods, Department of Psychology, Hollins College, Roanoke, Virginia 24020.

whether the response was "positive or negative," by which he meant *emitted* or *omitted;* and (c) whether the "reinforcement" was a "reward or punishment." All possible combinations of these dichotomies resulted in eight categories of instrumental conditioning. In Grant's scheme, escape training appears again as a separate category rather than as a subvariety of avoidance, as Kimble (1961) had treated it. Furthermore, discrimination training, which was treated in separate chapters by both Hilgard and Marquis (1940) and Kimble (1961) and not discussed as another type of instrumental conditioning, now takes its place *within* Grant's scheme. But even more important, Grant's classification generated two additional logical possibilities, which apparently had never been studied in the laboratory, although examples cited by Grant from everyday situations appeared quite feasible. Grant (1964) termed these new types "discriminated punishment training" and "discriminated omission training."

Yet, there was still something missing. The newest scheme had no place for the, by then well-known, unsignaled, or Sidman avoidance, paradigm nor did it allow for another obvious possibility, "signaled escape." In this paradigm, an exteroceptive signal informs the subject whether the escape response will result in "relief" from the aversive stimulation to which he is exposed. Evidence that this is a real paradigm for laboratory studies has been reported (Davenport, 1970; Davenport & Eschenbrenner, 1970; Davenport & Lerner, 1968; Woods, 1975b). To deal with these problems and to attempt to improve on the classification scheme and thus, possibly, clarify our thinking, it is proposed that Grant's third dimension be modified. Rather than referring to "rewards and punishments," the modification incorporates "positive or desirable" stimulus consequences and "negative or aversive" stimulus consequences and, furthermore, provides that each of these consequences can be either "presented or increased" in magnitude or intensity or, conversely, "removed or decreased" in magnitude or intensity. With these additional considerations, we can then generate the classification scheme shown in Figure 1.

As can be seen, each cell or operation is unique and is described by four characteristics. Also, it is important to note that the classification scheme is exhaustive, including as it does all possible combinations of these characteristics. Later in this presentation, each operation is dealt with in more detail. For the present, to help familiarize the reader, I will consider several of the cells briefly. Cell IA, for example, defines the conditioning paradigm in which the *emission* of a response is followed by the *presentation* of an *unsignaled* stimulus event which is *positive* or desirable to the organism. This is recognizable as the common paradigm of reward training. Likewise, Cell ID describes escape conditioning: An aversive stimulus is removed or decreased in intensity following a

		CONSEQUENT STIMULUS EVENT UNSIGNALED		CONSEQUENT STIMULUS EVENT SIGNALED	
		Operation performed on stimulus event			
		Present or increase	Remove or decrease	Present or increase	Remove or decrease
STIMULUS EVENT CONTINGENT UPON RESPONSE EMISSION	Positive or "desirable"	IA	IB	IIA	IIB
	Negative or aversive	IC	ID	IIC	IID
STIMULUS EVENT CONTINGENT UPON RESPONSE OMISSION	Positive or "desirable"	IIIA	IIIB	IVA	IVB
	Negative or aversive	IIIC	IIID	IVC	IVD

Figure 1. A taxonomy of instrumental conditioning.

specified response, and there is no signal to forewarn of the outcome. Finally, at this point, we can consider a paradigm in which a signal gives the organism information ahead of time as to the consequences of his behavior. In all of these cases the *absence* of the signal means that the same behavior will not be followed by any consequent stimulus events. Take Cell IIA: If a response is emitted in the presence of a particular cue or following a particular stimulus, then a positive event or a reward will follow, and the reward will be presented if and only if the response occurs when the signal or some cue is present. These are the operations we have traditionally used to teach a *discrimination*.

For the moment, let us return to some general considerations. That the present scheme does not represent merely a symmetrical extension of Grant's (1964) classification can be seen in Table 1, which indicates his types as well as a proposed nomenclature for the newly generated and old familiar paradigms. As can be seen, Grant's classification included all four of the present A cells, none of the B cells, three of the C cells, and only one of the D cells.

The proposed nomenclature has been generated on the basis of the assumption that it is desirable in science to be systematic and consistent and to use a terminology that has some communication value. Indeed, the proposed labels for each paradigm not only indicate the relationships among the various types of conditioning but, by themselves, they also indicate fully the response requirement and the operations to be performed without any further need for definition or explanation. (Such a nomenclature scheme has an advantage similar to the one used in organic chemistry, where a compound's name denotes its molecular structure.) While it must be admitted, in this connection, that it is perhaps more colorful to refer to one of our basic learning paradigms as "Sidman avoidance," it is certainly not defensible on any grounds other than a wish to honor Sidman.

It has been assumed that some terms can be left out in order to ease communication. In their absence, however, the operation is meant to be "understood." For example, if the paradigm is not labeled as *signaled*, it is understood to be *unsignaled* without our having to say so. Likewise, paradigms in Groups I and II require a response to be *emitted* without explicit specification. In the third term of the nomenclature, *penalty* is introduced to describe the removal of desirable stimuli and *escape* has been changed to *relief*. This change follows from the assumption regarding the value of being consistent: That is, the experimenter arranges for the delivery or administration of a reward, a penalty, or a punishment; he does not administer an *escape*. He

TABLE 1

The Relationship between Grant's (1964) Classification and the Present One with a Proposed Nomenclature

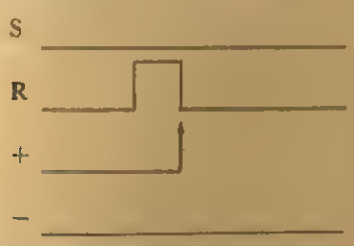
Paradigm	Instrumental types listed by Grant	Proposed nomenclature			
IA	Reward training			Reward	Conditioning
IB	—			Penalty	Conditioning
IC	Punishment (or passive avoidance conditioning)			Punishment	Conditioning
ID	Escape training			Relief	Conditioning
IIA	Discriminated operant	Signaled		Reward	Conditioning
IIB	—	Signaled		Penalty	Conditioning
IIC	Discriminated punishment training	Signaled		Punishment	Conditioning
IID	—	Signaled		Relief	Conditioning
IIIA	Omission training		Omission	Reward	Conditioning
IIIB	—		Omission	Penalty	Conditioning
IIIC	—		Omission	Punishment	Conditioning
IIID	—		Omission	Relief	Conditioning
IVA	Discriminated omission training	Signaled	Omission	Reward	Conditioning
IVB	—	Signaled	Omission	Penalty	Conditioning
IVC	Avoidance conditioning	Signaled	Omission	Punishment	Conditioning
IVD	—	Signaled	Omission	Relief	Conditioning

can, however, administer *relief*, a seemingly more appropriate term to use.

It should be noted that this classification scheme does conform to the general requirements cited by Cohen and Nagel (1934, p. 242) for an ideal classification: (*a*) A division must be exhaustive; (*b*) the constituent species of the genus must exclude one another; (*c*) a division must proceed at every stage upon one principle, the *fundamentum divisionis*.

Descriptions of the Specific Paradigms

With this general, overall introduction let us now turn to a more detailed consideration of each paradigm. The operations for each paradigm can be more clearly seen and kept in mind with the help of symbolic figures. The letter S will denote the antecedent signaling stimulus; R will denote the response; and + and − will signify desirable and aversive consequent stimulus events, respectively. Then ⎍ (with time represented along the horizontal dimension) will denote the occurrence of signals and responses; ⎍ will be used to indicate that a response was omitted or not made at a particular point in time. Finally, ↑ will denote a presentation or increase and ↓, a removal or decrease in the magnitude or intensity of the consequent stimulus events. Thus, simple reward conditioning would appear as follows:

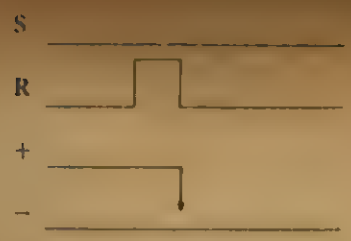


PARADIGM IA: REWARD CONDITIONING

This is the most familiar and most common form of instrumental conditioning and, as such, really needs no further elaboration. Described before the turn of the century by Morgan (1894) and by Thorndike (1898), it is the basic paradigm of the free operant procedure (Skinner, 1938). It has been used in literally thousands of studies (see Table 2).

PARADIGM IB: PENALTY CONDITIONING

This paradigm can be represented as follows:

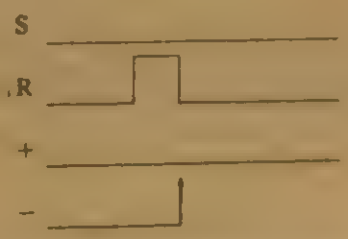


This common form of punishment was not included in Grant's classification nor has it received much experimental attention. A procedure used by Weiner (1962), however, appears related. Human subjects had the task of monitoring a display in a signal detection situation. "Points" were awarded for correctly detecting a signal, but in some conditions it *cost* the subject points to observe the display. Since points, albeit learned both by instruction and prior social conditioning, are positive or desirable stimulus consequences, such a procedure could be used more widely in studying this paradigm.

Even though learning researchers have hardly touched upon this paradigm, it is often used in everyday life; traffic fines, yardage loss in football, time in the penalty box in ice hockey, and imposed fines following conviction for a crime are familiar penalties. In the area of child rearing a mother might send her child's playmate home because of a squabble, thus penalizing both children by removing social companionship; also a parent might take away such desirable things as hammers, drums, horns, etc., because of what the child was doing with them.

PARADIGM IC: PUNISHMENT CONDITIONING

This paradigm (passive avoidance) can be represented as follows:



This was one part of Thorndike's original *law of effect* (the other part being Paradigm IA):

> Of several responses made to the same situation . . . those which are accompanied or closely followed by discomfort

to the animal, other things being equal, will have their connections with that situation weakened, so that, when it recurs, they will be less likely to occur [Thorndike, 1911, p. 244]

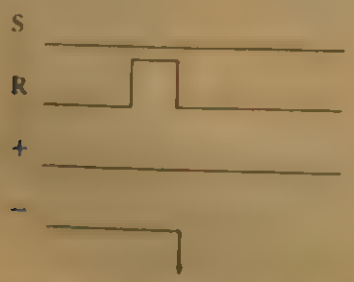
Extensive research then caused Thorndike subsequently to question the *weakening effect* of such stimuli, which he termed "annoyers." It was not that such stimuli could not suppress behavior, for they could. Rather, what was questioned was the notion of the weakening of "connections" between situations and responses. It was subsequently shown, for example, that when an aversive consequence is removed, the response which it had suppressed will return and continue under conditions of extinction for just as long as it would have had it never been suppressed by punishment in the first place (Estes, 1944).

With all of these paradigms, if the response requirement is not met, then no stimulus consequences will follow. That is, in this case, if the animal does not respond it will not be punished. This fact led to the use of the term *passive avoidance*. In retrospect, from the present analysis, the establishment of a nomenclature based upon the *failure* of a paradigm is not the clearest or most desirable course to follow (or, at least, we should be consistent and call reward training, for example, "passive nonreward training").

In real-life situations, from the treatment of pets and children to the treatment of criminals and foreign nations, one could hazard the cynical guess that this learning paradigm is more frequently used, by far, throughout all aspects of human interaction. When some person, group, or nation does something that some other person or group in power does not like, then aversive consequences will be delivered, ranging from verbal chastisement to execution and war. Since this is so familiar to all, no further examples are necessary.

PARADIGM III: RELIEF CONDITIONING

This paradigm (escape training) can be represented as follows:



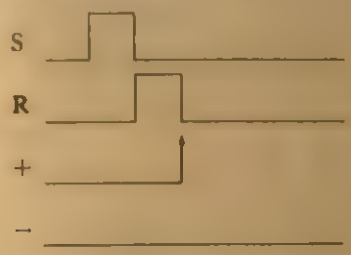
For an aversive stimulus condition to be reduced in intensity it obviously must exist in the first place. In an experimental situation we "establish the motivating conditions" by subjecting the animal to an aversive situation without allowing him the possibility of preventing such an exposure. Then, he is given control over the consequent reduction of the aversive intensity. Many of the published studies using this paradigm manipulated the intensity of an electric shock which the animal's response was then allowed to terminate (e.g., see Boren, Sidman, & Herrnstein, 1959; Kaplan, 1952; Trapold & Fowler, 1960). With this procedure, however, we can discern a serious confound: The amount of relief is directly related to the original shock level, and, hence, the contribution of either one of these variables to any performance differences cannot be ascertained. A procedure that avoids this confound was first reported by Campbell and Kraeling (1953) and was subsequently studied extensively by the present author and his students (e.g., see Woods, 1973a; Woods, Davidson, & Peters, 1964; Woods & Holland, 1966). One general finding from this work is that when the original level of aversive intensity is held constant and increasing amounts of relief are made to follow the response requirement for different groups, then the differential performance that occurs is very similar to that which follows increasing amounts of positive reinforcement in the reward conditioning paradigm. That is, when analogous operations are manipulated in these rather different paradigms, similar behavioral changes occur. Such generality is encouraging and the extent to which it occurs deserves further study. That it is limited, however, has recently been suggested by a study of the partial reinforcement effect in relief conditioning (Woods, Markman, Lynch, & Stokely, 1972).

Humans perform many responses to escape from aversive situations in everyday life: turning down a radio that is too loud, adjusting the thermostat when the room is too hot or too cold, leaving an area that is permeated with obnoxious odors, walking out of a dull or upsetting movie, leaving a group in which the conversation is being dominated by a boor, taking an aspirin for a headache, resting when fatigued, and taking a vacation to "get away from it all." Even at more complicated levels we can discern the basic relief paradigm. A citizens' group, for example, might initiate legal action against an industry that was polluting the environment, or people might campaign against an incumbent poli-

tician they disliked. All of these are clear illustrations of responses being performed in order to reduce or eliminate aversive situations.

PARADIGM IIA: SIGNALED REWARD CONDITIONING

This paradigm (discriminated operant) can be represented as follows:



In all signaled emission paradigms, information regarding the stimulus consequences is given to the organism prior to the performance of the relevant behavior. In effect, another restriction is imposed on the if–then condition. In the unsignaled emission paradigms the consequences will follow if and only if the response is made. Here, however, the consequence follows if and only if the response is made when and only when the signal is present.

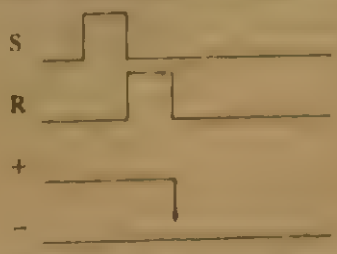
To add a signal to the reward conditioning paradigm is a simple and logical extension. In a free operant, lever-press situation with rats, for example, food pellets will be delivered for lever pressing only when a signal light is lit. Such conditions were demonstrated quite some time ago (Skinner, 1933, 1938). However, as noted in the introduction, discrimination training was often considered as a separate phenomenon in learning rather than as a variety of instrumental conditioning. Historically (Yerkes & Watson, 1911), it was a technique for studying sensory thresholds in animals. It then played a prominent role in early theoretical controversies between Gestalt and stimulus–response positions (e.g., see Krechevsky, 1932, 1938; Spence, 1936, 1937, 1938). It was also studied as a logical step following the establishment of the phenomenon of "generalization." Generalization, responding to *new* stimuli that are physically similar but perceptively different from the original training stimulus, was studied as a phenomenon in its own right, mostly in the framework of classical conditioning (Kimble, 1961, pp. 329–330), although instrumental responses also were used (e.g., Grandine & Harlow, 1948). Of course, trying the same response to similar stimuli is clearly an adaptive strategy with great survival value. (No two apples are identical, but most are edible.)

In summary, then, discrimination has been considered either as a separate phenomenon or as a secondary one to generalization. It was not until Grant's (1964) scheme that it found a logical place within instrumental conditioning paradigms, and, of course, it continues to hold an integral place within the present scheme.

A commonly used procedure for establishing a discrimination in the laboratory is to combine the basic paradigm of rewarding responding to the "correct" stimulus with Paradigm IIC (described below), which consists of punishing responses to the "incorrect" stimulus. Such combinations are also common in everyday life. If you drive through a green traffic signal, you are rewarded by getting to your goal; if you drive through a red one, you may be punished with a summons or an accident. If children laugh and play during recess, they are rewarded by friendly reactions from peers; if they laugh and play during class, they are punished by the teacher. If one laughs at a joke told in a group, he is rewarded by social approval from the group; if one laughs at a sad story told by the same individual, he is punished by social disapproval and made to feel embarrassed. If a girl smiles at a boy, he knows that approach responses will be reinforced; if she gives him a "cold stare," he knows that approach responses will be punished.

PARADIGM IID: SIGNALED PENALTY CONDITIONING

This paradigm can be represented as follows:



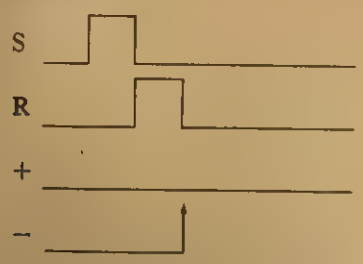
Just as with unsignaled penalty conditioning, this paradigm was not included in previous treatments of instrumental conditioning and to the best of our knowledge has not been reported in the experimental literature.[2] In everyday life, however, such

[2] It appears appropriate to make a point here that will apply to our discussion of a number of these paradigms. A search of the world's literature for instances or examples of the less well known paradigms is really not feasible be-

situations are bountiful. Whenever there is a "warning" that attempts to constrain certain behaviors where the consequences will be a monetary fine, we have an instance of the signaled penalty situation. All sorts of traffic signs are signals that if a particular behavior occurs, the individual will be fined: "speed checked by radar," "school—15 mile limit," "driver subject to arrest for throwing litter from vehicle," "no parking," "car will be towed away at owner's expense," etc.

PARADIGM IIC: SIGNALED PUNISHMENT CONDITIONING

This paradigm (discriminated punishment) is represented as follows:

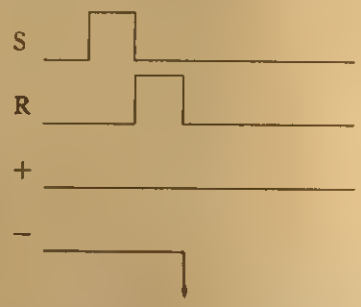


This heretofore unstudied paradigm was generated by Grant's (1964) classification scheme. Grant provided an illustration of this paradigm with a description of a colleague whose speech was "colorful" in informal social gatherings but whose undergraduate lectures were "models of propriety." ("Colorful" speech in the latter context would, presumably, have resulted in the punishment of severe criticism or even dismissal.) He also described the procedure of punishing his cats for scratching the furniture only to have them learn not to scratch the furniture in his presence; that is, they learned that punishment would occur only under the signaled condition of his presence.

Examples of this paradigm are easy to generate: The familiar "wait till your father gets home!" is a warning signal to the child that if certain behaviors continue he will be spanked by the "old man." Likewise, ignoring a "beware of the dog" sign may result in being bitten, and hunting on posted land may lead to a charge of buckshot from a farmer. The paradigm plays a prominent role in many child-rearing situations in which the parent is the signal. The child learns that many behaviors, sexual, aggressive, verbal, etc., will result in punishment if performed in his mother's presence.

PARADIGM IID: SIGNALED RELIEF CONDITIONING

This paradigm can be represented as follows:



This is a simple logical extension of what has traditionally been called *escape training*. A signal merely informs the subject in advance whether the relevant instrumental response will result in successful escape or relief. This paradigm, however, was not one of those generated by Grant's classification, and it has received no experimental study until recently. Davenport and Eschenbrenner (1970) and Davenport and Lerner (1968) have used this paradigm, and they report varying degrees of success. A clear demonstration of its reality has been more recently reported by the present author (Woods, 1973b). Rats swam in a cold-water alley tank to a separate goal tank, which on relief trials was warmer and on nonrelief trials was the same temperature as the alley. At the beginning of each trial, a light signal informed the animal whether the goal tank would be warmer. The animals did learn to swim with greater vigor and fewer competing reactions on those trials in which relief was signaled.

Outside of the laboratory, in everyday situations, illuminated "exit" signs are obvious signals for escape in case of fire or other emergency. And who has not anticipated relief by following a sign to the rest room?

A friend beckoning from the hallway can be a signal one can follow to escape from a boring meeting. When someone is lost in the wilderness, rescuers use signals to guide them to safety, and

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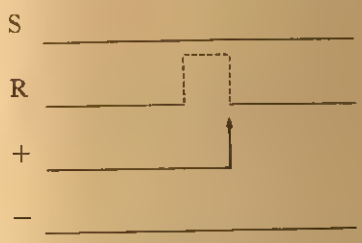
cause the terminology by which information is stored and retrieved has neither been consistent nor readily translatable. For example, what is referred to below as "omission reward conditioning" has been labeled by people working in the free operant tradition as a "differential-reinforcement-of-other-responses" schedule. This is not meant to criticize their choice of terminology; it is merely intended as an explanation for our inability to be absolutely certain as to whether any particular paradigm has been empirically studied and reported. The author would welcome, therefore, communications citing any instances of what we currently believe to be unstudied paradigms.

sailors in fog or distress respond to signals in a similar manner.

PARADIGM IIIA: OMISSION REWARD CONDITIONING

This paradigm (omission training) can be represented as follows:



The first full description in English of this procedure apparently was in Konorski (1948, chap. 12), but the term *omission training* itself did not appear until Kimble (1961). Konorski's work dealt with simultaneously established classical and instrumental responses. In this case, a salivary response in dogs was classically conditioned to a metronome. Then, occasionally, at the same time the metronome was activated, the dog's leg was passively flexed. On these trials food was omitted, while on trials with the metronome alone and no leg flexion, food was continued. The animal's reaction to this was not only to resist the flexion but to actively *extend* his leg in so doing. What is reported, then, is not mere omission but the actual strengthening of an antagonistic response. Whether or not such a reaction is a characteristic of all omission paradigms is an important matter, yet to be determined.

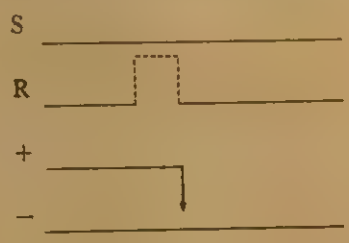
In describing some real-life instances of this paradigm it is easy to see how an antagonistic response may often be involved. The familiar expression of "biting one's tongue" testifies to this: One cannot just *omit* a verbal response but must go one step further to an antagonistic reaction.

In order to stay in old Uncle Harry's will, you must not tell him what you really think of him. Such constraints may result in an exaggerated display of affection. A mother might say to her children, "If you don't argue and fight I'll take you out for ice cream." Their reaction could very well be profuse displays of affection. A teacher might tell her class that if they do not talk during the reading period she will organize their favorite game during recess. Again, overly strong efforts at remaining silent might result.

A behavior modification technique in which someone is given monetary rewards for not smoking is another example of this paradigm. And, finally, parents might resort to this in attempts to control their financially dependent adolescent children. "Don't talk to your father that way! Who do you think is putting you through college?" Translation: Omission of angry critical responses will result in continued financial support.

PARADIGM IIIB: OMISSION PENALTY CONDITIONING

This paradigm can be represented as follows:



In this paradigm we find the case in which someone is penalized for not doing something. As with many of the remaining paradigms it does not yet seem to have been studied in the laboratory.

In real-life situations most penalties are probably the consequence of something an individual *does*, but there are still clear examples of penalties for failing to respond. If you fail to lock your car, it may be stolen; likewise, a fine is the consequence for not returning a library book on time, and late payment fees are often the sanction for not paying bills when they are due. (Our own APA resorts to this procedure; it costs a member $5 if dues are not paid before December 1 each year.)

Parents seem more likely to manipulate rewards and punishments rather than penalties, but we can conceive of some child-rearing illustrations. In the area of "manners" or "social graces," a child might have something taken away for failing to make a particular response. For example, if a child does not say "thank you" when he is given some candy, he might then have that candy taken away.[8]

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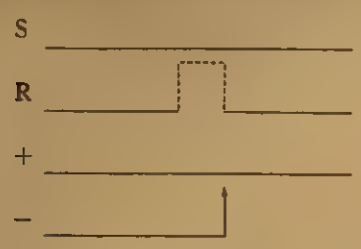
[8] A subtle but important distinction between a *penalty* and a *reward reduction* should be made for terminological clarity. By *penalty* we want to refer *only* to the *removal* of positive stimuli that the individual already has in his possession. Reductions in anticipated rewards, then, are not penalties. If the child's next allowance, for example, is cut down because of something he did or did not do we have a reward reduction (à la Crespi, 1942) within a reward conditioning paradigm.

PARADIGM IIIC: OMISSION PUNISHMENT CONDITIONING

This paradigm is represented as follows:



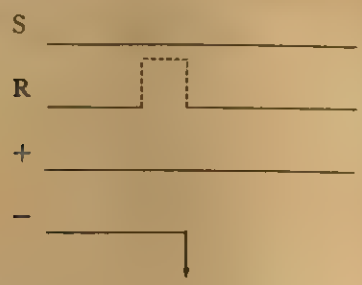
Given our cultural zeitgeist, this is probably one of the more common omission paradigms. We tend to resort to punishment as a consequence for someone failing to react in an expected manner just as we resort to punishment for "undesirable" or obnoxious actions. Also, in many cases, what we term *neglect* results in aversive consequences as a direct result of our failure to act without any intervention from others. If a worker, for example, fails to put on safety glasses or other necessary protective devices, he may suffer bodily injury. If we fail to check the gauge, we may run out of gas; if we fail to check the tires, and hence do not discover the soft one, we may be stranded with a flat; and if we fail to carry out preventive maintenance, the car may not start. When you leave home, you fail to confine the pets, with the result that they damage the furniture.

But, as mentioned above, other people often do intervene to administer punishment for response failure. Children are scolded for not washing, for not straightening up their rooms, for not wiping their feet, for not saying "thank you," etc. If one fails to arrive at work on time, one may be "chewed out" by the boss, and if a student fails to study, he or she may receive an F on a test. Many examples can easily be generated, and it should be clear that this is a commonly used paradigm in everyday life.

But what of laboratory work? This has actually been investigated for over 20 years, but its location in this classification scheme is probably going to come as a surprise to many researchers in this field. We will argue that this truly is the paradigm for what has come to be known as Sidman avoidance (after Sidman, 1953a, 1953b). Since the argument, however, also applies to the more common form of avoidance, we will take it up below under that paradigm (Paradigm IVC).

PARADIGM IIID: OMISSION RELIEF CONDITIONING

This paradigm is represented as follows:



As mentioned in discussing Paradigm ID, in order for "relief" to follow as a consequence of responding, the organism must be in an unpleasant condition to start with; this is no less true in the case of response omission. Here we are talking about the reduction of some aversive state being the consequence of the withholding of some behavior. Again, as far as we can determine, this has not been studied in the laboratory, although real-life illustrations do appear valid. If you stop smoking, various unpleasant side effects such as coughing may go away. (Of course, aversive withdrawal symptoms may also follow as a consequence, which would be omission punishment conditioning, but this merely illustrates that several paradigms can operate simultaneously.)

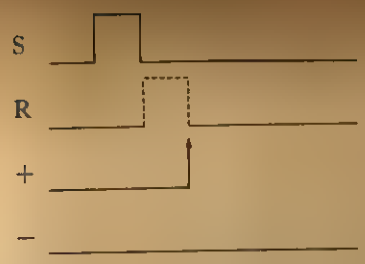
If you do not socially react to a boor, he may leave. If you do not laugh and applaud a friend's rather bad home movies, he may stop showing them sooner.

The consequences of muscle relaxation procedures (Jacobson, 1964) can fall into this paradigm in those cases in which the individual is suffering pain, as in a tension headache. By relaxing, which is the cessation (omission) of muscle-tensing reactions, the individual experiences the consequences of relief from the pain.

And, for a final illustration, we should be able to agree that, unfortunately, some school situations are aversive for many children; when the children are allowed to leave school on time for *not* running and shouting or talking in class, then we have another instance of omission relief conditioning.

PARADIGM IVA: SIGNALED OMISSION REWARD CONDITIONING

This paradigm (discriminated omission training) can be represented as follows:

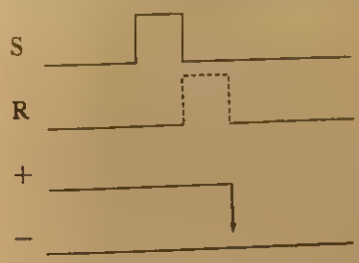


This is the other "new" paradigm generated by Grant's (1964) classification scheme, and he likened it to seeking "the appearance of virtue rather than virtue itself [p. 13]." He then discussed it with an example par excellence concerning a parent who offers to reward his daughter with money or other valuables if she stops smoking, drinking, swearing, or some other such behavior. The young lady, who was maintaining the behavior at a high operant level, interprets the bargain to mean that she should not get caught, and she develops a well-discriminated omission. She continues the behavior but is just careful not to do so in the presence of her father or a likely informant.

Another general class of common animal training procedures should suffice for illustrating this paradigm: Using food rewards, dogs are trained to obey certain commands, and whenever the response requirement consists of the termination (omission) of any behavior we have this paradigm. For example, "stay" commands the dog to not follow, and if this is strengthened with food or petting, the procedure is that of signaled omission reward conditioning.

PARADIGM IVB: SIGNALED OMISSION PENALTY CONDITIONING

This paradigm can be represented as follows:



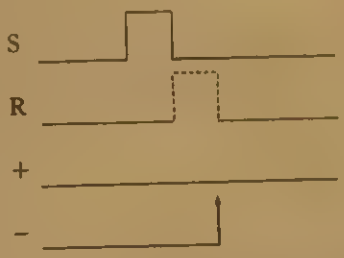
This is another paradigm uncovered by the present scheme, and so far as it can be determined it has no research history. As discussed in the section on Paradigm IIIB, most penalties seem to be the consequence of action rather than inaction, although we can develop some illustrations that argue for the existence of this paradigm.

With the stop sign at the corner as the signal, then failure to stop can result in a fine. A fine also could result if you fail to stop when signaled to do so by a policeman. And a parking ticket is often the result of failing to heed the overtime signal on a meter.

We can also conceive of a situation in which the stock market is dropping and a broker advises a stockholder to sell; the advice is the signal. But the stockholder *fails* to sell with the consequence that the value of his stock declines. Hence, we again have a penalty for failing to respond to a signal.

PARADIGM IVC: SIGNALED OMISSION PUNISHMENT CONDITIONING

This paradigm (avoidance conditioning) can be represented as follows:



If the organism does not respond to a signal, he will be punished; hence, if the designated response is made, the punishment will be *avoided*. As mentioned above, in the discussion of Paradigm IIIC, the location of avoidance conditioning in this scheme will most probably come as a surprise to many researchers in this area. Those who have considered escape and avoidance to be very similar, such as Kimble (1961) and Denny (1971), may be surprised to see that these paradigms differ on three of the four dimensions in the present classification; the only thing in common is that they involve aversive events. But probably the most surprise and possibly the most controversy will be generated by the classification of avoidance conditioning as an omission paradigm. This is because we have long emphasized the active nature of the subject in the avoidance situation with the resulting puzzle as to how an action can be maintained by no consequences. Hilgard and Marquis (1940) emphasized this direction long ago when they said:

> Learning in this situation appears to be based in a real sense on the avoidance of the shock It differs clearly from other types of instrumental training in which the conditioned response is followed by a definite stimulus change—

food or the cessation of shock. In instrumental avoidance training the new response is strengthened in the absence of any such stimulus; indeed, it is strengthened because of the absence of such a stimulus [pp. 58–59].

Our traditional view of avoidance is understandable, but we hope that a reassessment of this position will not meet with great resistance. Viewing it as an omission procedure causes us to see the avoidance response itself as an antagonistic response to the one called for by the paradigm. Then, as such, avoidance appears related to omission training and punishment training in which antagonistic responses have also been reported (Konorski, 1948). The proposed nomenclature in which avoidance is called "signaled omission punishment conditioning" reflects the similarity of these paradigms.

Attempts to explain avoidance conditioning have taxed our theoretical creativity. Miller (1935) explained it in terms of fractional anticipatory goal reactions (r_g's). These anticipatory reactions to shock were seen to increase in intensity as the actual shock itself approached, leading to a heightened state of tension. By running in the other direction, say, the animal is able to relieve this tension. Tension relief is drive reducing, and hence the puzzle of avoidance is explained by this theory as a form of escape conditioning.

Subsequently, more eloquent theories were developed (Mowrer, 1947; Solomon & Wynne, 1954). In these, two processes were seen as being involved in the explanation of avoidance learning. First, in the early stages of learning a neutral stimulus, the signal, being paired with a noxious shock, was seen as acquiring through classical conditioning procedures the capacity to elicit fear. Then the second process, that of establishing the avoidance response itself, was again seen as an instance of escape conditioning with fear reduction being the reinforcement for the avoidance response. Thus, buzzer and shock, say, occur in sequence until the buzzer itself elicits fear; at this point the animal makes a response which both reduces the fear and avoids the shock.

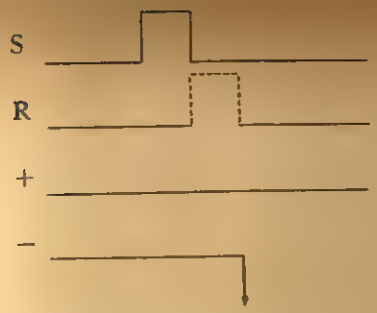
But matters are certainly far from settled regarding theoretical explanations of avoidance, and the area continues to receive considerable attention (e.g., see Brush, 1971; D'Amato, Fazzaro, & Etkin, 1968; Hoffman, 1966; Sidman, 1966). Any further discussion of theoretical positions in this area is beyond the scope of the present article. I merely wish to point out that theoretical concerns might take a different direction if avoidance is viewed as it is classified here. Avoidance, whether signaled or not, is really very similar to punishment as has been tacitly recognized when the latter has been referred to as "passive avoidance." Given this recognition one wonders why there has not been the same degree of empirical and theoretical concern for the punishment paradigm. Apparently, we do not find it as much of a puzzle or challenge to understand why an organism would fail to respond in order to avoid punishment as we do to understand why an organism would respond to avoid punishment. But let us look more closely at what does happen in the punishment situation. Initially the organism has been making a response such as approaching the food cup at the end of an alley. Then the situation is changed so that approach to the food cup results in an electric shock. It does not take many experiences like this before the organism "pulls itself up short," so to speak, after it has started down the alley. Further, it is common for us to observe the animal engaging in other behaviors that compete with the approach behavior. What this argument is suggesting is that in passive avoidance the organism may not be passive at all, but, indeed, may be engaging in active responding in order to avoid the punishing goal box. If one agrees with this analysis then one is agreeing that punishment conditioning is very similar to avoidance conditioning.

In any case, when punishment is the consequence of a response requirement, the organism will typically fail to meet that response requirement. In punishment conditioning an *emitted* response is the requirement for aversive consequences, and that response will be *omitted*. In avoidance conditioning a particular response must be *omitted* for aversive consequences, and that response will thus be *emitted*.

In real life we do many things merely because and solely because not doing things would result in punishment. Some students study because if they did not they would fail; soldiers salute superior officers because if they did not they would be disciplined; drivers keep their eyes on the road because if they did not they would crash; children wipe their feet because if they did not their parents would scold; and, in some cases, professors keep their office hours because if they did not students would complain and the dean would call.

PARADIGM IVD: SIGNALED OMISSION RELIEF CONDITIONING

This paradigm can be represented as follows:



This final paradigm is also one that appears not to have been studied in the laboratory, yet, as before, we can consider some real-life illustrations that argue for its existence. If a person who is suffering from the effects of heavy smoking responds to "Warning: The Surgeon General Has Determined That Cigarette Smoking Is Dangerous to Your Health" by ceasing to smoke, then we have signaled relief for response omission.

Other medical illustrations also seem to fit nicely here; this paradigm is operating whenever a person is advised (the signal) that he or she will obtain relief from some distress by *not* doing certain things. Dealing with someone who is overweight provides a good concrete example. Being overweight has unpleasant consequences, and relief is obtained by following a physician's advice not to eat high-calorie foods. Likewise, the aversive effects of tension and stress are relieved by following advice to stop certain stress-producing activities.

Some Final Observations

There is one point that is terminological and procedural yet relevant to the proposed taxonomy in that it will demonstrate how the system can handle a dominant feature of contemporary research, namely, schedules of reinforcement. In reward conditioning, for example, when the stimulus consequence called for by the paradigm does indeed occur, we have by definition a "rewarded" trial. If the experimenter arranges, however, for the paradigm to fail, that is, prevents the expected stimulus consequence, we have a "nonrewarded" trial. We can generate, therefore, any schedule of reinforcement by intermixing these two possibilities. Various schedules of reinforcement, then, do not represent exceptions or additions to the present classification; they can be incorporated within it.

Still along these lines, if we switch to the paradigm "failure" possibility for 100% of the trials, we establish thereby the conditions for "extinction." This is a well-known manipulation in reward conditioning, and this scheme suggests that the same manipulation with other paradigms might be a fruitful avenue for research. It can also be suggested that subsequent operations analogous to the reinforcement shift studies in reward conditioning would be worthwhile in other paradigms. (This has been done, e.g., with relief conditioning: Woods, 1967; Woods, 1973a.) Another research direction that should be expanded is one in which certain combinations of the basic paradigms are used in the same experiment. Such a development, which has already seen some limited application, might establish experimental situations that are more analogous to "real life." Parents, for example, frequently reward their children for a response that is made and punish them if the response is not made (a combination of Paradigms IA and IIIC).

TABLE 2

A Tabulation of Instrumental Conditioning Studies Cited in Psychological Abstracts

Instrumental paradigm (traditional name)	% of studies reported each year		
	1961 (N = 245)	1966 (N = 607)	1971 (N = 580)
Reward training	40.8	36.7	42.1
Discriminated operant	33.9	28.5	28.1
Avoidance conditioning	7.0	9.9	8.3
Punishment	1.2	5.4	7.1
Escape training	.8	1.5	2.2
Combinations of two of the above	4.9	9.6	5.5
Instrumental paradigms combined with classical conditioning	4.9	2.5	1.2
All other instrumental paradigms and combinations	6.5	5.9	5.5

In closing, it might be illuminating to get some idea of the distribution of our research efforts in these areas. We have, therefore, classified all of the basic learning studies that appeared in *Psychological Abstracts* for 1971 and for 5 and 10 years before that.[4] The results, which are somewhat striking but perhaps not surprising, are shown in Table 2. A very few paradigms have dominated our research efforts in instrumental conditioning. One strategy for the future is to break the "iron grip of tradition" and explore the operational manipulations suggested here. Do learned changes

[4] The author wishes to thank Virginia T. Scott for her help with this tabulation.

in behavior occur with all of these and, if so, do analogous manipulations lead to similar or different phenomena? In a conclusion to a discussion of scientific psychology in transition, Kimble advised:

> If anything should be abandoned for the time being, it is the experimental strategy that allowed me in my doctoral dissertation (Kimble, 1947) to investigate eyelid conditioning at six intervals between 0.10 and 0.40 seconds with substantial numbers of subjects at each point. We need to worry more about the magnitude of effects and the *generality of our laws* [Kimble, 1973, p. 18, italics added].

Such generality should be investigated not only across species and apparatus but across operational manipulations as well.

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A Curriculum for the Teaching of Behavior Assessment

IAN M. EVANS *University of Hawaii*
ROSEMERY O. NELSON *University of North Carolina at Greensboro*[1]

Banned from Soviet Union schools by the Central Committee of the Communist Party in 1935, burned by the school board in Houston, Texas (Nettler, 1959), and frequently harangued by the general public (see Lovell, 1967), psychological tests have not met with universal affection. In behavior modification the cry is "don't test, teach," and of the 168 psychotherapists polled by Meehl (1960), only 17% felt that testing was of much value to them. Advocates of objective psychometric testing conclude from the available literature that projective tests are "unreliable, nebulous, and invalid" (Eysenck, 1970), whereas Mischel (1968) argued that neither projective nor psychometric tests "have served very usefully as indirect signs of internal predispositions [p. 145]." Meanwhile, back in the internship training centers, the most frequently used and most heavily emphasized test in training is the Rorschach (Garfield & Kurtz, 1973), and although often exposed to the critical literature, students in academic training programs continue to be taught tests like the Rorschach and the MMPI, perhaps, as Breger (1968) suggested, to fulfill professional role expectancies.

A more constructive teaching plan might concentrate on procedures that are evolving as alternatives to typical psychological testing. In clinical training programs with a predominantly behavioral orientation, it is particularly incumbent upon the critic of traditional assessment to provide an approach that dovetails, in both theory and practice, with the general principles being taught. The present authors were independently faced with the task of teaching assessment as a first course to graduate students in behaviorally oriented clinical programs.[2] In a recent discussion we discovered that our approaches were strikingly similar, and although none of the parts was uniquely original, it seemed worthwhile to report the whole. There is surprisingly little discussion of course content in graduate clinical education compared with, for example, teaching methods, program goals, or the likelihood of the graduates ever receiving gainful employment. It might also be of interest to practicing psychologists to know what contemporary clinical students are able to offer if they balk at administering test batteries or pronouncing on a projective protocol.

One challenge in constructing an assessment course is to reconcile the skills required for the student's clinical work in the immediate future with a critical faculty needed to appreciate both the limitations of assessment and the corrective steps possible. Given such a goal, neither descriptions of specific tests nor intricate psychometric issues received much attention. The concepts of reliability and validity were naturally omnipresent, but utility was heavily stressed. Nelson chose Mischel's (1968) *Personality and Assessment* as her textbook. Evans referred extensively to Mischel's monograph, but selected as a text the British publication edited by Mittler (1970), *Psychological Assessment of Mental and Physical Handicaps*. These two texts were not on face value the most

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[1] We are grateful to Marvin R. Goldfried for allowing us to examine the syllabus of the assessment course taught at the State University of New York at Stony Brook.

Requests for reprints should be sent to Ian M. Evans, Department of Psychology, University of Hawaii, 2430 Campus Road, Honolulu, Hawaii 96822.

[2] Differences in our respective clinical programs resulted in somewhat different course structures. Evans was fortunate in being able to teach the course after the students had had detailed exposure to the methods of behavior observation of Roland G. Tharp. Nelson included in her course a certain amount on standardized intelligence, screening, and achievement tests; we have not included this in the course description, but the orientation was in line with the course as a whole. Nelson's course was linked to a practicum, whereas at Hawaii the practicum accompanied only Tharp's section of the two-semester course.

compatible of partners, but within the organization of the course they complemented each other well. The organizing principle was the single-subject research model, which integrated both the empirical study of psychopathology and the experimental analysis of individual behavior control and modification.

Behavior Assessment and Psychopathology

The present authors considered it important to teach behavior assessment within the context of psychopathological research. There are two justifications for this, and they overlap, in essence, with the two major alternatives to the medical model of psychiatric disorder: the sociopsychological and the defect models. The defect model will be discussed later. Sociopsychological perspectives were provided by reference to Braginsky, Braginsky, and Ring (1969), Goffman (1961), Plog and Edgerton (1969), Sarbin (1967), Szasz (1961), and Ullmann and Krasner (1969). The weight of this reading assignment was reduced somewhat by the students having had, as expected, exposure to these ideas as undergraduates.

With the backdrop of a sound understanding of the entire labeling process, the student still needed to attend to aspects of the current action on the clinical stage. A minimal classificatory scheme seemed necessary for students to order their experiences, and they may have noticed, with some sense of relief, that a major part of Ullmann and Krasner's (1969) study was organized around the traditional psychiatric nosology. Some familiarity, therefore, was required with the *Diagnostic and Statistical Manual* (1968) of the American Psychiatric Association, and with any of the more empirical psychiatric texts (e.g., Freedman & Kaplan, 1967; Slater & Roth, 1969; Storrow, 1967).

At some time in the future, students would have to label patients for institutional or legal purposes, or simply to communicate with other mental health professionals. In response to a questionnaire, however, 53% of the University of Hawaii clinical students indicated that in field settings they had been obliged to provide formal diagnostic labels and they reported that the demand had come from a secretary. Students seem to have their own way of dealing with the mental health bureaucracy: More than half of the students claimed that under this stricture they typically selected the least offensive label possible, usually *adjustment reaction.*

There are, of course, alternatives to sabotage. The least satisfactory was to attempt to improve upon the notorious unreliability of diagnostic categories (e.g., Beck, Ward, Mendelson, Mock, & Erbaugh, 1962; Schmidt & Fonda, 1956; Zigler & Phillips, 1961; Zubin, 1967). Brief mention was therefore made of diagnostic checklists (e.g., Overall & Gorham, 1962; Rimland, 1968), decision-making flow diagrams (e.g., Nathan, 1967), and the use of objective tests (e.g., Cattell, Schmidt, & Bjerstedt, 1972). Cattell et al. chastised clinicians for not using these tests since they possess "diagnostic certitude that decidedly exceeds that by other methods [p. 240]"—cold comfort for those challenging the utility of the whole categorical system.[3]

A second possibility was to reform the classificatory model and to relate classification to experimentally determined mechanisms of behavior. The most systematic efforts in this direction were made by Eysenck (1960) in his simple but brilliant insistence on dimensional rather than categorical organization. As a strategy, Eysenck's approach was appealing to the students. Enthusiasm was somewhat dampened when it was admitted that despite its promise for the future, the approach had virtually no current clinical utility. It is true that Eysenck's personality theory is the only one to have a strong relationship to behavior therapy (Eysenck, 1957; Eysenck & Rachman, 1965), but at the moment, its relevance to the planning of treatment is extremely tenuous—the few nonexperimental examples given by Eysenck (1970) notwithstanding.

What Eysenck (1957) called "rational" assessment leads to the second major reason why behavior assessment should go hand in glove with psychopathological research. Eysenck (1960) described abnormal behavior as "the defective functioning of certain psychological systems [p. xii]." Following this model with some license, assessment for treatment becomes the delineation of the underlying deficits (or handicaps) in the behavioral repertoire of the patient. The single-subject research strategy which carries this out is discussed in the next section.

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[3] To be fair, Cattell is no admirer of current diagnostic categories. For many years he advocated the use of personality source traits, a strategy which in many ways is similar to Eysenck's.

What needs to be mentioned here is that assigning a descriptive label to a patient should immediately put the clinician in touch with the experimental work that has been conducted with respect to that syndrome. From the many examples given to the students, we can select two widely divergent ones: (*a*) Describing a child as autistic should focus attention on difficulties of processing sequenced perceptual input (Hermelin & O'Connor, 1970); (*b*) in working with an obese patient, the external food stimuli regulating his eating might be more profitably considered than either his oral fixations or his attempts to reduce anxiety (Schachter, 1971). A student cannot be expected to be *au fait* with every new finding, but he can be expected to recognize the current experimental literature as one area from which to draw his hypotheses concerning a patient.

The deficit model need not be exclusively concerned with physiological disturbance, although it is especially appropriate in cases of probable organic pathology and childhood handicaps. Staats (1968), for example, suggested that a reading disability might be a function of deficiencies in a more basic response mechanism (i.e., visual attention) or in the child's motivational system, or both. A good illustration of his theory applied to assessment was recently furnished by a University of Hawaii graduate. E. S. Kubany (1971), in his doctoral dissertation, produced experimental evidence that reinforcing performance on a standard attainment test had a greater effect on the persistence of Hawaiian children than on the Caucasian children (see also Edlund, 1972). When testing children in his clinical practice, Kubany manipulated social reinforcement, nonsocial reinforcement (e.g., toys, candy, or money), and social criticism to observe the effects on behavior, after which recommendations were made regarding the teaching style that might be used most profitably with the child (Kubany & Sloggett, 1973a).

The Single-Subject Research Strategy

In traditional assessment a patient is usually described with reference to a preconceived theory or model of personality and abnormality. The present authors' concern with experimental psychopathology is of a quite different nature—economy of effort. Research findings are neither infallible nor immutable, and they provide a guide to, not a substitute for, the detailed idiographic analysis of the precise variables relevant to the target behaviors of the individual patient. Shapiro (1951, 1957) was one of the earliest proponents of the view that this analysis should adhere to the rules of scientific evidence. He argued, rather neatly, that one does not test the patient but the hypotheses about the patient, and he emphasized that psychiatrists typically do not formulate their diagnostic questions in the form of testable hypotheses. Shapiro wrote a number of articles on this approach, the most comprehensive of which appeared in 1966.

Required reading for the course participants also included the surprisingly few case studies where the use of ad hoc tests had been carefully carried out (some collected in Davidson & Costello, 1969; others cited in Mittler, 1970). Hamilton's (1964) criticism of the impracticality of the method was also recommended, as well as Tizard's (1971) critique of Mittler (1970), and an entertaining paper by Gwynne Jones (1971) that discussed the general issues surrounding idiographic research.

One of the examination questions asked the course participants to compare and contrast Shapiro's experimental approach with a functional analysis in the Skinnerian sense. The logic of the two is essentially the same, although there is, of course, an obvious difference in research methodology. Just as different problems in fundamental psychology require different research methodologies for their investigation, so do different problems in applied psychology; formal hypothesis testing is not the only rigorous strategy (cf. Shapiro, 1952) within the intensive research model (Chassan, 1960). Behavior assessment may begin with the free observation of behavior and its covariation with environmental change, successful intervention in that environment being confirmation of the hypothesized functional relationship. Exclusion of other possibly relevant factors is achieved by additional manipulations, sometimes of the stimulus antecedents (e.g., Evans, 1971; Lovaas, Freitag, Gold, & Kassorla, 1965), but more commonly of the response consequences.

In the course discussed here, therefore, attention was paid to the single-subject research designs typically used in behavior modification. Examples abound in the *Journal of Applied Behavior Analysis*, but useful summaries can be found in Baer, Wolf, and Risley (1968); Bijou, Peterson, Harris, Allen, and Johnston (1969); Browning and Stover (1971); Gelfand and Hartmann (1968). For a more sophisticated discussion, Sidman (1960) was recommended. At the same time it was pointed out that

control techniques like baseline reversals, multiple baselines, differential reinforcement of another response, and so on, have serious limitations when it comes to the broad *evaluation* of therapy or remedial training (Evans, 1971). In addition, one must decide on the significance of the observed changes. Gentile, Roden, and Klein (1972) have suggested that the analysis of variance model is fitting.[4] Also, a key assumption of these strategies is that noncontingent reinforcement will not maintain behavior, but there is evidence in operant conditioning literature that this is not invariably the case (Schoenfeld, Cole, Lang, & Mankoff, 1973), so we expect that orthodox, applied, operant research designs will themselves soon be modified.

A second difference between the two single-subject models was the concern for organismic variables (Mittler, 1970) as opposed to the controlling environmental stimuli, both antecedent and consequent (Mischel, 1968). Those with a Skinnerian bent have tended to ignore, even denigrate, intraindividual causes of behavior dysfunction; those concerned with individual deficits often underplay the force of environmental circumstances. Integrating these two assessment styles provides an objective clinical method of wide generality and power. It is ideally tailored to the planning, guidance, and evaluation of behavior modification (therapy), and so it was to the precise specification of assessment for treatment that the course turned.

Assessment for Behavior Change

Although the assessment procedures typically used in behavior modification are more narrow than the possibilities encompassed by the intensive single-case strategy advocated in the course, a reasonably specific technology for obtaining samples of behavior has been worked out in recent years and students should be familiar with it. The guiding tenets of behavior assessment were most comprehensively laid down by Mischel (1968): (*a*) Behavior is a function of the environment; (*b*) particular behaviors are specific to particular situations; and (*c*) samples of behavior must be obtained by the least inferential methods. Since Mischel's evidence and interpretations have been challenged, the students were referred to some critical comments (e.g., Alker, 1972; Bem, 1972; Craik, 1969). Other discussions of the rationale of behavior assessment can be found in Goldfried and Kent (1972), Goldfried and Pomeranz (1968), and Nurnberger and Zimmerman (1970). Kanfer and Saslow (1969), one of the central references of the course, provide the best, most detailed statement of the kind of information that should be obtained in assessment. The methods by which the behavioral excesses, deficits, and assets of the patient are precisely ascertained can be broken down into two major groups: (*a*) interviewing and (*b*) the observation of behavior.

BEHAVIORAL INTERVIEW

Insofar as one can identify behaviorally the important elements of a good interview, we feel that instruction can best be given in a practicum setting following the microteaching techniques developed at Stanford (e.g., Bush & Allen, 1964). The general principles of a good interview can be obtained from any of the classic psychiatric accounts (e.g., Sullivan, 1954), but Benjamin (1969) is concise and informative. MacKinnon and Michel's (1971) study was also quite useful for more inexperienced students since it contained specific advice on handling difficult and unusual situations. Two guides were available to the kind of interview that is valuable in obtaining detailed information on child behavior problems: Holland (1970) and Wahler and Cormier (1971). To get a flavor of the research related to interviewing, Matarazzo's (1965) study was assigned, and the additional complexities of interviewing children were touched upon by reference to Yarrow's (1960) chapter discussing the interview as a research tool. Finally, to dramatize the potential limitations of the interview, we suggested the students read Labov's (1970) fascinating anecdotes of interviewing black ghetto children.

It is not the techniques of interviewing but the content and focus of the interview itself that most differentiate the behavioral interview from any other (Lazarus, 1971; Wolpe, 1970). A complete

[4] Since this article was written, Gottman (1973) has presented a useful discussion of time-series methods in $N = 1$ research. Gottman's communication is specifically pertinent to this article: (*a*) in demonstrating that baseline shifts published in the behavior modification literature may lack statistical significance, (*b*) in discussing the causal inferences possible from different time-series designs, and (*c*) in emphasizing once again that "by monitoring a patient over time and by monitoring treatment variables simultaneously, the therapist receives feedback which aids the process of therapy and also generates hypotheses for future testing [p. 98]." Behavior therapists have made fair use of assessment devices administered repeatedly, as will be commented on in the next section.

anamnesis is not usually considered of much value in behavior therapy, and where a social history is considered desirable it is often in the form of a questionnaire (Wolpe & Lazarus, 1966) that the patient can fill in at home to save time. A detailed account, both historical and contemporary, is usually required of the problem area, for example, with sexual disorders, a thorough sexual history (Annon, 1971) and survey of experience (Bentler, 1968); with marital disharmony, a detailed analysis of the marriage (Stuart & Stuart, 1972); and with phobias, a set of questions aimed at the specific stimulus elements to be incorporated in the systematic desensitization hierarchy (Wolpe & Lazarus, 1966). All the paper-and-pencil devices that have become popular in behavior therapy are really time-saving extensions of the interview. The Fear Survey Schedule (there are now several, the first being published by Wolpe & Lang, 1964) might be used to turn up fears that were overlooked in the interview or that the patient may have considered irrelevant or embarrassing. For subsequent use in designing contingencies, the behavioral clinician often has the patient list his reinforcers (Reinforcement Survey Schedule—Cautela & Kastenbaum, 1967; Mediator-Reinforcer Incomplete Blank—Tharp & Wetzel, 1969), just as one might ask a parent or teacher to list all the potential reinforcers for a child patient.

Behavior therapists have evolved a number of similar devices that are directly related to the strategies of behavior change. In essence they involve systematization of parts of the behavior change strategy itself: (*a*) identifying the target behavior, (*b*) analyzing its functional relationship with the environment, and (*c*) determining possible sources of behavior influence. Most of the tests are without norms, but that is usually excused by administering them frequently and relying on the individual trend (analogous to an operant baseline) to monitor the client's progress and objectively guide the course of therapy. The utility of these methods is therefore clear and may be evaluated by the relative success of behavioral techniques. In research reports and case studies on some of these "tests," there is a growing reliance that is unwarranted in terms of proven reliability, validity, and immunity to repeated administration. Behavior therapists seem to be in the awkward position of coming to depend on tests that are less psychometrically sophisticated than those they have spurned.

DIRECT OBSERVATION OF BEHAVIOR

It is not really surprising that behavioral observation should be the parvenu of clinical assessment, considering its relative neglect in basic psychological research. The standard references for instruction in behavioral observation are the studies of Bijou, Peterson, and Ault (1968); Bijou et al. (1969); Blackham and Silberman (1971, Chapter 4); Buckley and Walker (1970, Section D); Colman and Boren (1969); and Hall, Christler, Cranston, and Tucker (1970). An ethological perspective was provided by Hutt and Hutt (1970). Various methods of recording one's observations, either directly (Wright, 1967) or by some kind of code (O'Leary, Romanczyk, Kass, Dietz, & Santagrossi, 1971; Patterson & Cobb, 1971; Werry & Quay, 1969), were also discussed.

We were interested and pleased to notice that in the Introduction to the *Standards for Educational and Psychological Tests* (APA, 1974), which, incidentally, will now become part of the required reading for the course, the Joint Committee on Revision of Standards gave tacit recognition to behavioral observation by stating, "the psychologist who counts examples of a specific type of response in a behavior-modification setting is as much responsible for the validity of his interpretations of change or the basic reliability of his observations as is any other test user [p. 4]." The many hazards of behavioral observation were presented to our students in detail; indeed there is extensive literature on matters such as the reactive nature of being observed, observer bias, and the selection of behavioral categories (a problem to which we are particularly sensitive in cross-cultural settings; see, e.g., Weiss & Gallimore, 1973). Lipinski and Nelson (in press) have reviewed this literature so it will not be listed here; needless to say, their review was required reading.

The students were taught not only to perform behavioral observations themselves but also to teach other persons in the client's environment to use this method of data collection (e.g., Kubany & Sloggett, 1973b; Tharp & Wetzel, 1969). Alternatively, especially in adult behavior therapy, the patient may have to be taught to observe and record his own behavior, usually by diaries, but also by frequency counts or by close attention to eliciting stimuli (Watson & Tharp, 1972). The unreliability and reactivity of self-recording, however, limit its potential as an assessment method (Broden, Hall, & Mitts, 1971; Gottman & McFall, 1972; Johnson

& White, 1971; Kanfer, 1970; Lipinski & Nelson, 1974; McFall, 1970; McFall & Hammen, 1971; Simkins, 1971a, 1971b).

While observations may be made in a variety of settings, it is sometimes useful, especially for research purposes or in circumstances of common behavior deficits, to record behavior in standardized situations. A good example of this use of direct observation is the Behavioral Avoidance Test (Borkovec & Craighead, 1971; Levis, 1969), although it has rather serious measurement problems (Bernstein, 1973). Sloggett (1972) used a simulated classroom to observe certain behaviors in teachers—the children having been trained to act in appropriate as well as villainous ways. A number of groups working with alcoholics have observed and assessed their patients' drinking behavior in realistic replicas of public bars (e.g., Schaefer, Sobell, & Mills, 1971). Sobell and Sobell (1972) reported that patients are best interviewed when inebriated because the sedative effect of alcohol reduces anxiety and facilitates information gathering. Structured clinical interviews (Burdock & Hardesty, 1968a; Ulmer & Timmons, 1966) fall into this class, and checklists of behavior in given situations are logically part of these techniques, so the students were exposed to some of the more common examples; Balthazar Scales of Adaptive Behavior (Balthazar, 1971); Cain-Levine Social Competency Scale (Cain, Levine, & Elzey, 1963); Behavior Problem Checklist (Peterson, 1961); Vineland Social Maturity Scale (Doll, 1953); Ward Behavior Inventory (Burdock & Hardesty, 1968b); Hamilton Psychiatric Rating Scale for Depression (Hamilton, 1967).

Conclusion

A student who appreciates the pitfalls of psychological measurement, and whose clinical work follows the rules of applied research, should not find it too difficult to integrate into his empirical assessment strategies the mensural advances of basic research technology. The many developments of special clinical significance are so disparate and scattered that we only attempted to present a few of the more promising ones. But there are many others; for example: penile volume and pupil dilation are among the measures that have been used to gauge sexual orientation and erotic interest, despite certain limitations (Zuckerman, 1971); rate of habituation of the orienting response (Lader & Wing, 1966) and speed of acquisition of the conditioned eyelid response (Martin, Marks, & Gelder, 1969) are possible prognostic indicators for systematic desensitization with phobias; psychophysiological measures, especially forearm bloodflow (Kelly & Walter, 1968), can be used to guide progress up the desensitization hierarchy (Mathews, 1971); operant conditioning techniques have been used in a number of assessment-related ways (Weiss, 1968), including hearing loss (e.g., Evans, 1970) and visual acuity (Macht, 1971); some behavior therapists have become interested in repertory grid measures (Bannister & Mair, 1968).

The usefulness of some of these measures in clinical practice remains to be seen; in any event it depends on the ingenuity with which the applied psychologist can adapt them to his purpose. The above references were presented mainly to stimulate that ingenuity.

Overall, the references cited in this course description represent a formidable reading list for one semester. To some extent the material has been increased by mixing rather than distilling our respective requirements; on the other hand, for the sake of brevity, we have cited reviews of papers that the students read in the original. We are not so naive as to anticipate that every participant read every reference, but we do expect most will have something better to offer their clients than a research-inspired skepticism for projective and other traditional tests.

At the beginning of this article, mention was made of the continued use of traditional assessment methods in applied settings (Garfield & Kurtz, 1973). By comparison with McCully's (1965) findings, Levitt (1973) also noted that field training directors have shifted only slightly in their belief in the importance of mastery of the Thematic Apperception Test (TAT) and the Rorschach and that, furthermore, they continue to express dissatisfaction with students' academic training in projective techniques.[5] Our curriculum is not likely to reduce this discord, but it does offer a complete alternative to the limbo that might result from what both Levitt (1973) and Garfield and Kurtz (1973) see as a dichotomy between "research values" and "clinical expediency." Our students

[5] We suspect that Levitt, Garfield, and Kurtz have fallen into the questionnaire trap and that assessment by direct observation would reveal a steady annual decline in the use of projective tests in clinical practice

face, we hope, no such dichotomy. They were taught a working model for assessment—taught to be applied scientists who bring to the assessment situation knowledge of behavioral disorders, the ability to use objectively a variety of methods of measurement, and, above all, a critical respect for data in the guidance and improvement of their clinical activities.

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Community Psychology and the Competent Community

IRA ISCOE *University of Texas at Austin* [1]

The term *community psychology* was coined at the Boston (Swampscott) Conference on the Education of Psychologists for Community Mental Health in 1965. Although its original purpose was "to consider problems and issues in the education of clinical psychologists for community mental health activities," the conference was unanimous in its recognition of the need for American psychology to develop a wider arena for preventive mental health measures and of the necessity to study and deal with persons and communities in their natural habitats. Explicit in the recommendations of the conference was a dissatisfaction with the then existing models of clinical psychological training in terms of their suitability in preparing psychologists for the delivery of mental health services and research within community settings. The conference report (Bennett, Anderson, Cooper, Hassol, Klein, & Rosenblum, 1966) has become somewhat of a collector's item, representing a new posture for an important segment of American psychology. Numerous developments since 1965 have attested to the viability of the concept of community psychology and to the sensitivity of the conferees to the changing mood and thrust of America. Various pieces of legislation have placed the locus of treatment of broadly defined emotional disturbance within a community, as opposed to an institutional, context, and the nation as a whole has become increasingly aware of enormous unmet mental health and human growth needs.

The formation of the Division of Community Psychology in 1966 was in recognition of the shifting concern from more classical psychopathological models to "problems of living." Schizophrenia and the neuroses pale in magnitude and locus of treatment when compared with social concerns such as violence, drug abuse, the sexual revolution, the legitimate demands of minority groups, inner-city decay, and public education, to mention but the major ones. All of these concerns reflect the psychological state of our society, and how effectively these concerns are dealt with is an index of the well-being of the nation as a whole. They serve as grim reminders that social problems will not "go away," and that the results of neglecting crucial social problems cannot be erased by crash programs or the rapid infusion of money and resources.

In beginning to deal with these problems, we recognize the blurring of jurisdictional and definitional boundaries. Traditional concepts about cities, counties, and states are undergoing realignment. Psychological rather than geographical membership is being emphasized. A safe prediction is that more problems are yet to arise. The lowering of the voting age to 18 will bring about the realignment of political power. College populations have already begun to exert much greater influences in the community, and it is only a matter of time until legal responsibility changes from age 21 to 18. This is part and parcel of the secular trend in America. Things happen earlier than they used to (and faster too). A scientifically based community psychology must be able to predict some of these developments and make preparation to cope more adequately with them. A community psychology that makes its living dealing only with the casualties of social systems will soon lose its viability. It should be proactive rather than reactive, adhering to a fundamental tenet of public health that no condition is ever prevented by treating the victims of the condition itself.

The Growth of Community Psychology

There are several indications of community psychology's continuous growth, despite its rudimen-

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Requests for reprints should be sent to Ira Iscoe, Department of Psychology, University of Texas, Austin, Texas 78712.

tary nature with regard to definition, conceptual clarity, and levels and types of training. An increasing number of university-based programs are offering community psychology as a specialty or subspecialty. Faculty with community psychology backgrounds are in demand, and clinical psychology programs are taking on a more community mental health emphasis. Community psychology has begun to declare its ideological independence from clinical psychology and from community mental health. If tolerance for ambiguity is one of the strong indicators of good mental health, then it must be possessed in abundance by the many graduate students entering this new area.

Rather than become embroiled in definitional aspects or jurisdictional disputes, community psychology has to proceed as best it can, recognizing that it will have overlaps with other disciplines and subareas within psychology. This condition is of course not unique to community psychology. Reiff (1970) pointed out the existing paucity of applicable social science knowledge about communities, the need to develop a body of knowledge via community-based research, as well as the failure to use existing knowledge more effectively. The transfer from the laboratory of the university to the laboratory of the community is a difficult process, but one that must be accomplished if behavioral science concepts are to be tested within community settings. This need was poignantly pointed out by the late Martin Luther King in his distinguished address to the American Psychological Association in 1968. He called upon American psychologists to lend their skills in dealing with the race problem. A five-minute standing ovation and genuine goodwill on the part of the great bulk of American psychologists did not lead to constructive community activities. Perhaps American psychology was not ready to enter into the complexities of communities; indeed, as will be pointed out, psychology and behavioral sciences generally, perhaps for good cause, have not been actively included as an integral part of some of the more prominent human welfare activities in the last 10 years.

Kelly (1968, 1970, 1971) enumerated some of the tasks facing community psychologists and some of the qualities desirable in the community psychologist himself. A developing community psychology could play an important role in community research by designing effective coping methods to deal with some of the more salient social problems that are now faced or will be faced by communities in the future. The first generation of community psychologists "emerged" at Swampscott. A new generation is just beginning to emerge. Hopefully it will bring with it the sophistication and technical competence to work effectively in community settings and to be able to discriminate between "action" and genuine change. The late 1960s were full of action. Just how much change was brought about still remains to be seen. One of the main conceptual approaches of community psychology could be the increasing of the competence of human beings in dealing with the problems they face. Characteristic of high-risk populations such as the poor, minorities, and the aged is a high degree of community disorganization and the lack of effective coping strategies. How a community is to be made more competent is an enormously complex matter, and no prescription presently exists. A consideration of the competent community, its challenges and potentialities will nevertheless be attempted.

Concepts of the Competent Community

The processes by which a community gains competence are presently very unclear and excellent topics for research. However, the concept of the competent community is not a new one. Cottrell (1964) employed it when he urged psychiatrists not to try to "take over" the then developing community mental health movement, but to work instead toward the fostering of competence on the part of consumers of mental health services and the development of new types of manpower and new concepts of treating emotional disturbance in children and adults. Many community mental health centers are under severe criticism today for failures in these very areas. Leighton et al. (1963), in their community research, pointed out that the level of integration of the community more strongly reflects its mental health than the variables of sex, age, or occupational status. The development of the competent community involves the provision and utilization of resources in a geographical or psychological community so that the members of the community may make reasoned decisions about issues confronting them, leading to the most competent coping with these problems. Coping is stressed rather than adjustment, in keeping with a positive, activist point of view. Conceptually, the competent community is one that utilizes, develops, or otherwise obtains resources, including of course the fuller development of the resources of the human beings in the community itself. In this respect, the concept of the competent community

parallels the concept of positive mental health as put forth by Lehmann (1971), who suggested that the criterion of "utilization of resources" be employed as an index of mental health rather than the criterion of absence of symptomatology. It is not the presence of problems but the manner in which they are coped with that is all important. Success in coping with one type of problem should broaden the repertoire of skills of the community and enhance its possibility of more effective coping with other problems that arise. Presently communities differ greatly in this respect. Middle-class communities have the mechansims and the track record. They point to their successes. The lower socioeconomic strata do not have this record, nor are they integrated enough to even communicate what few successes they have. How can a community psychologist or community psychology be of help to a disenfranchised or disorganized community? This is precisely the challenge, and it involves the sailing of uncharted waters as far as psychologists are concerned. A detailed knowledge of the community itself is essential, especially its potentials. From what sources does the present leadership arise? What are the real issues? Who is trusted in the community? What changes are needed in the educational and human service delivery systems? The enormity of the problem should not detract from the potential rewards. Already some glimmerings of progress may be detected by the sensitive observer. Attempts at community controls in deprived districts and a court order prohibiting the classification of minority group children by standard intelligence tests are examples.

The community psychologist should be attuned to the phenomena that deprived communities do not always go for goals understandable to the middle-class power structure. Persons in a constant battle against poverty settle at first for immediate visible goals. As chairman of a community action program, I walked into a meeting and received loud applause from the blacks and Mexican-Americans of the audience. I thought it was for the extra money I had obtained for a summer recreation program for children. In fact, the applause was due to "negotiation" on my part that had resulted in the city rather quickly paving two streets and patching up chuckholes in some others. An increase in the number of children getting lunch at school is tangible; a revision of the curriculum for better competence in mathematics is not quite so tangible. Priorities will differ in different communities, depending on their stage of development. Community psychology not only must be sensitive to where the community is but must also eventually be able to predict the next steps in its development.

Characteristics of the Competent Community

The competent community or, more specifically, persons and groups in the competent community have a repertoire of possibilities and alternatives. Their strength lies in their alternatives. They know where and how to acquire resources. Characteristic of disadvantaged communities today is not only the economic condition of the members but their psychological disadvantages, as exemplified by feelings of hopelessness and powerlessness. The community lacks "clout." It will not be heard by those in power. These communities represent perhaps the greatest single challenge to community psychology. Paradoxically, it is in these same communities that the greatest resistance to research will be encountered in the form of suspiciousness, resistance, distrust, and outright opposition. This is an unfortunate legacy from the "objective" behavioral science research of the past. Persons without hope and without power and with no trust in behavioral sciences are difficult to convince that some benefit will accrue to them or their community as a result of the research and activities being undertaken. Their reinforcement, on the contrary, has been that of being "tooled-over" by the system. The barriers are not insurmountable. Skill on the part of the community psychologists will be needed so that they may find ways of reaching and involving the populations they are working with or researching on. In a time of mounting consumerism, community psychology must be prepared to involve consumers. In doing so, community psychology has to be prepared for the results of success. Mounting feelings of hope and some power lead to demands for independence. Community psychologists must be prepared to part with populations once certain goals have been achieved or once the status of these populations has been improved. If they do not part with the populations, at least the relationships have to be negotiated taking into consideration changed conditions.

Community psychology has grown so rapidly that it is appropriate to call attention to these potential dangers now, lest it become a victim of an all too prevalent ideology that wittingly or unwittingly fosters dependency and fails to support the develop-

ment of competence of the populations and communities it is involved with. If the genuine goal of community psychology is to foster the growth of competence and independence, nothing is healthier than for all persons in a problem area to negotiate from positions of strength rather than from a condition in which there is inequality among the negotiators. Unfortunately, a good part of the helping philosophy today has not been in this direction. Research within community settings has not been of much benefit to the community. There is a cartoon somewhere in which a Southwest Indian tribe, after years of misery and exploitation, finally comes into a considerable sum of money from oil lands. Their first act of independence is to boot out the anthropologist who has been studying them, lo these many years, with no visible benefits to them. It is to be hoped that an effective community psychology will arrange a smoother transition.

Another challenge for community psychology is the bringing about of a productive dialogue between the existing power structure and the emerging competent community. Failure to do so will result in violence, disillusionment, and disadvantage to both parties. This has already happened in some cases and testifies to our failures to date. An activist position without provision of an accommodation for those in power and those out of power can only result in unproductive acrimony, conflict, and polarization. A mature community psychology must help prepare persons for the assumption of responsibilities rather than merely the assumption of power.

The Competent Community, Human Service Programs, and Their Evaluation

Although many programs designed to be of assistance to the economically disadvantaged and ethnic minority groups are no longer supported to the extent that they were under previous government administrations, human service delivery systems are part and parcel of our American society. Despite the billions of dollars involved, very few funds have been allocated to research and evaluation. The reluctance of persons to evaluate their organizations and efforts is certainly not limited to human service programs. However, so marked has been this lack of evaluation and feedback that sometimes one wonders rather facetiously if there does not exist an anticompetence plot in America today. The continuation of poverty, dependency, ignorance, and insecurity are in their long-term effects enormously destructive to human beings as human beings. It is easier and more acceptable under present procedures to administer vast programs designed to treat casualties of social systems, putting on Band-Aids forever, rather than examining the systems themselves. The areas of evaluation and research are not the unique provinces of community psychology or the field of psychology. Yet psychologists interested in behavior should find exciting possibilities—possibilities, incidentally, that might extend to the entire field of psychology. For example, the Job Corps dealt with, and still deals with, a segment of the American population that psychology knows very little about, namely, the economically disadvantaged male and female between the ages of 16 and 21. What enormous benefit to developmental psychology might have emerged if a continuing evaluation and follow-up research program had originally been tied into the entire endeavor! There is a clear, national trend toward evaluation couched in such terms as cost effectiveness, program evaluation, cost benefit analysis, zero-based budgeting, and continuity of care. Behavioral scientists (including community psychologists) must develop valid criteria for evaluations lest less valid criteria are imposed by persons from the accounting offices.

Presently, very little valid data exist to even begin to try to answer some very fundamental questions. What are the critical points of entering, remaining, and leaving public welfare? Is there a welfare generation and, if so, how does it differ from the previous one? What are the crucial intervention points for breaking the cycle of welfare and dependency? If persons have to be on welfare could they not be more effective as human beings, happier, better educated, even though on welfare? What have been the results of the vast rehabilitation programs undertaken in the United States? Given the deprivation and odds against them, what factor or factors contribute to some persons "making it"? How do lower-economic-status persons deal with stress? What factors strengthen their coping responses? These are only samples of the many questions that have to be asked. Presently, human welfare programs, from one point of view, are succeeding beautifully if the recipients cause no trouble and do not raise too much of a fuss. From this view, an ideal welfare recipient is one that does not hassle the welfare worker. A program that fosters competence, independence, and promotes psychological growth is bound to cause

trouble to the existing power structure. Powerlessness means safety for some. The phrase "power to the people" may be better understood in this context.

Consumerism and Community Psychology

In the delivery of most human services, the opinions of consumers have hardly ever been sought or, if given, are usually ignored. Until recently this held for almost the entire spectrum of human services including higher education. Massive programs have been launched with little or any pretesting, and usually without the funding of mechanisms for feedback designed to improve the systems. Medicare paid attention to organized medicine and how physicians felt about the plans for the program, but not to the recipients of medical aid. The physicians and the hospitals had the "clout." The consumers were getting the service and they should be grateful for it and should not presumptuously question the wisdom of the professionals. An effective community psychology "listens" to the recipients of services, not necessarily following their dictates but carefully weighing the inputs they receive. The disassociation of consumers from the delivery system and sometimes even from the setting of services results in a polarization that interferes with a mutuality of respect between provider and consumer. There is no sharing of problems, and there is therefore increasing ignorance of the stresses upon providers and consumers. Worst of all, the consumer takes no responsibility for his part of the transaction. Here again we have the question of developing a productive dialogue between those who have and those who have not. The fostering of responsibility is admittedly difficult, especially after years of procedures calculated to discourage the exercise of initiative.

As a competent community develops, it must allow for failures. Given the lack of experience and opportunity for leadership, it is perfectly understandable that many indigenous local leadership projects have failed and that the input of consumers has not necessarily been as helpful as had been supposed. This does not mean to say that attempts at local leadership and fostering of increased consumer participation should be abandoned. It does mean that the efforts have to be reevaluated, the reasons for the failures examined, and the project go back to the "drawing boards," so to speak.

When a giant space systems corporation with years of experience expends a half-billion dollars in planning and production, and underestimates its costs by a hundred million dollars while all the while producing a super plane plagued with bugs and frequent crashes, the company is not criticized or penalized. On the contrary, efforts are made to help it recoup its losses. The defense of the United States is endangered. The complexities of communities and of the human beings in them are at least equal to those of the wonder plane and the interaction of the engineers, the production line, the space age materials and apparatus. The reasons for failing to return to the drawing boards in the case of social pathology experiments are of course many, perhaps the main ones being the failure to build appropriate corrective mechanisms into the system and the absence of sustained championing of the program and its goals. The citizens concerned do not have the sophistication, and in many cases they have not even been consulted, so they are not ego involved. There is of course much scapegoating couched in terms such as "it's impossible to do things in this neighborhood" or "these people don't give a damn." The leaders of the emergent competent community must go back to the drawing boards again and again. They cannot look for quick laurels and victories. Community psychologists, and indeed behavioral scientists generally, must be intimately involved in evaluation and feedback in most if not all community endeavors. They must expect failure and should not be ashamed of it. The processes by which success or failure comes about should be their concern.

Dangers of Involving the Consumers

The feedback from consumers is frequently highly critical, or, even worse, the consumers may begin to take involvement seriously. Some years ago the Office of Economic Opportunity (OEO) adopted the phrase *maximum feasible participation,* urging the poor to participate as much as possible in OEO-supported activities such as day care, Head Start, neighborhood information centers, family planning, and community development. Speaking as a former chairman of a board of directors of a community action program, I can testify that maximum feasible participation can lead to "outrageous" demands by consumers. They demanded evening hours for the city council, a neighborhood branch of the Welfare Department that would be open on Sundays, a substation for the county hospital, Saturday hours for conferences with teachers in the public schools, neighborhood-based policemen, a stop to rent goug-

ing, action against street violence, better bus services to work, more day care facilities, rodent and insect eradication, better fire safety in public housing, the dismissal of the Director of the Surplus Commodities Program, evening hours for the Employment Commission, plus better job counseling and opportunities for employment. The priorities were closely related to the more immediate needs of the consumers. The satisfaction of shorter range goals leads the community to believe that it has some input into the system, and longer range goals can then be worked for. The chairman has to learn some new rules of procedure. Roberts Rules of Order do not hold up very well. Most of the participants have never heard of them. The community has to learn to participate. Persons who have never had a chance to "say their piece" want to be heard. Meetings last far into the night. The distinctions between the Board of Directors at the table and the surrounding audience break down frequently. The observer of traditional meetings would most likely be appalled by the seeming chaos; yet things do get done. Having tasted a bit of success, the group wants more. They ask difficult questions. It is small wonder that the term *maximum feasible participation* was not stressed quite so strongly in the last few years of OEO. Communities that begin to see a little daylight start generating enthusiasm. Hope is terribly contagious. In some instances, community action programs have been taken over by so-called target groups with very sensible changes introduced. In others there have been disastrous polarizations. Communities, as they begin to develop in sophistication, will predictably begin to ask questions of professionals, even about professionalism itself.

One of the criteria of a competent community is the change in which power is transmitted to once powerless citizenry. The emergence of the sophisticated consumer should be welcomed, even though it may threaten the status quo. Parenthetically this emergence is not limited to the economically deprived alone. Sophisticated graduate students sometimes give us fits. Their demands for participation in graduate education are "unreasonable." Although obviously they have it far better than we had it in graduate school, they are still dissatisfied, they are presumptuous, their questioning of what constitutes a good program is premature since they have not had enough experience, their evaluations of instructors are not fair, their demands for relevance are in violation of our academic orientation, and their attempts at sharing in departmental government are a clear bid for power. Their demands really do not differ too much from other consumers in less sheltered settings.

Some Realities in the Development of the Competent Community

If the poor were articulate, knew their way around the system, and had time to spend building up resources, then they most likely would not be poor and lacking in power. What we take for granted is sometimes quite difficult. Luncheons and weekend retreats are not easily set up in impoverished communities. Communication may be difficult and people who work for hourly wages have to pay a relatively high financial price for taking time off. Appearing before the city council, the hospital board, or the school system can be a frightening experience, and the threat of punitive action on the part of employers is not to be taken lightly. In the complicated arena of striving for human betterment, a strategy might be to engage the efforts of a prominent, hard-working, ambitious member of the middle class. He could very well be the most appropriate person to help a less affluent and influential community. The notion is admittedly speculative and might lead to opposition on the part of social planners. Those who are interested in developing the competent community, however, might recognize that advocacy comes in many forms. Educated persons, after all, with many resources, hire lawyers to represent them in legal matters.

Although there have been many attacks on social benefit programs, the so-called War on Poverty has resulted in some increased competence on the part of communities. It is the type of competence that is only now beginning to emerge. Imagine, if you will, a group of Chicano parents asking for a meeting with the school principal to discuss plans for the coming year. They have had rather good experiences with Head Start or day care programs; they have participated in various ways. They have taken responsibilities; they have a feeling of just a little power and a little more hope that their children may get a better break in school. The principal is unsympathetic and outraged. How dare these "Messcans" even ask about the curriculum! He has told them it is the same for all first graders in the public school system (nondiscrimination is one of the ways of keeping the system as it is). The parents have had contact with persons who are interested in the situation. The parents

obtain expert advice about the discriminatory effects of intelligence testing, grade placement, and the like, from various sources including the community psychologist who was available to them in Head Start days. A million dollar suit is threatened against the principal, the teachers, the superintendent, and the school board, seeking exemplary damages as well as remediation of the current situation. The case is expertly worked up by a high-powered conservative lawyer, socially prominent, with political ambitions. He uses consultants skilled in community action and cognizant of the deleterious effects of inferior education on children. The complaint stresses the lack of validity of standard intelligence tests, the physical inadequacies of the school building, the lack of experience of the teachers compared to a school in a better part of town, the absence of a Spanish-speaking secretary, and the rudimentary bilingual program, etc. The school board capitulates in a hurry. Genuine changes are instituted. Parents are involved in planning a new school. Loud rumbles are heard about outside interference and agitators. Rather than gloat in the victory, community psychology should prepare for the predictable next step in which this same group of parents will go on to activities that will enhance their hopes and their power. In displaying this independence they may appear to be ungrateful to the community psychologist and others who were helpful to them. The rewards in community psychology are the growth of the community, not the thanks that are received from the beneficiaries.

It has been easier to keep groups dependent and therefore lacking in competence. The increase in competence of communities will result in many challenges and could be enormously rewarding areas for research. If programs are successful, individuals and groups will assert their independence and do more things on their own, not doing it "our way" but "their way," different as it may be from ours. Achieving the goal of independence does not guarantee similarity of actions and attitude between initiators and those who presumably have benefited. On a poetic if not prophetic note, it is fitting to close with a reference to Tennyson's *Morte D'Arthur*, in which the dying king says, "I perish by this people that I made." Community psychology, in fostering the competent community, need not perish. It can grow and become more competent itself.

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Corporal Punishment

ADAH MAURER *Committee to End Violence Against the Next Generation, Berkeley, California* [1]

Fear of punishment has always been the great weapon of the teacher . . . .
The subject is so familiar that nothing more need be said about it.

William James, 1892

Part 1: The Semantics of Punishment

The word "punishment" comes from the same root (*L. poena*) as do the words "penalty" and "pain." Thus, we may assume that semantically the relationship is of ancient origin. "Cruel and unusual punishment" in the Bill of Rights was intended to prohibit torture for the purpose of extracting confessions or in retribution for unlawful acts. Both penalty and pain are implied. Webster's Dictionary lists a number of metaphorical uses of the word such as the deliberate punishment of automobile tires to test their endurance and safety; but as applied to human interactions, punishment means "to impose a penalty for some fault," "to inflict a penalty in retribution," or "to deal with harshly."

The synonyms "chastise," "discipline," and "correct" are differentiated by noting that while punishment indicates some retribution inflicted after a disobedience, chastise is likely to suggest the infliction of pain in the hope of effecting a reformation; discipline may involve punishment but always suggests action in the interest of order, rule, or control by authority; while correct may indicate a guiding away from error. There is much overlap and loose usage especially in the vernacular of those involved in the training of children, but the dual concepts of penalty and pain—or shame, deprivation, suffering, or loss—predominate.

When punishment is prefaced by "corporal" (*L. corpus*—body), the meaning is unmistakably that of pain upon the body of the person, usually by some instrument such as a whip or paddle wielded by an agent of the offended jurisdiction. For example, "Corporal punishment was abolished in the Navy in 1850." In California juvenile institutions where corporal punishment is forbidden, restraint is permitted, but not the use of a strait jacket, gag, or thumbscrew or by tricing up, stomping, arm twisting, washing of mouth with soap, ducking, rabbit punches, curtailment of food, or extremes of temperature. Comparable forbidden school punishments would include, besides paddling, such refinements as stuffing the mouth with paper tissue, masking tape as a gag, shaking, ear pulling, lifting by the hair, or throwing against the wall or floor. Not definable as corporal punishment are fines, discharge, loss of privileges, and, in school, detentions, exclusions, and extra assignments.

Electric shock is a comparatively new method of inflicting pain and cannot thus be included easily and incontrovertibly. In varying degrees of intensity, it is designated as a punishing agent by experimental psychologists; it has been used to execute criminals; it is touted as a curative of depressive psychoses; and it is accepted with equanimity by telephone installers, for example, as a minor job-related annoyance. Even though it is painful and is applied to the body, electric shock can be called corporal punishment only when the intent of the punisher is taken into consideration. Clearly, the effort to pin down a definition of punishment, and especially corporal punishment, is not without its controversial aspects.

In scientific endeavors it is common for new words to be invented or for old ones to be given new meanings to fit the needs of the new tech-

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[1] Requests for reprints should be sent to Adah Maurer, Committee to End Violence Against the Next Generation, 977 Keeler Avenue, Berkeley, California 94708.

niques. In computer technology, for example, common English words like "bit," "word," "address," and "loop" have special meanings within that particular discipline in addition to their ordinary meanings. Thus, when experimental psychologists define punishment as "a reduction of the future probability of a specific response as a result of the immediate delivery of a stimulus for the response [Johnson, 1972, p. 1034]," they are within the scholastic tradition which permits the use of any combination of phonemes to convey any concept, provided only that the terms are defined and that the new definition gains wide acceptance. The special definition of punishment as a technical term in experimental psychology is thus acceptable by these criteria.

The words "stimulus" and "response" used in so redefining punishment have not themselves been as carefully redefined from the common meanings. Stripping them of the ordinary implication of chronological sequence—stimulus first followed by a response—and assigning instead meanings tied to the status of the participants in which the experimenter (high status) gives the stimulus and the subject (low status) gives the response, regardless of timing, has gained wide but by no means universal acceptance even among experimental psychologists. Uneasy jokes about rats laughing among themselves at how cleverly they have conditioned the white-coated human to provide food indicate an awareness that the power relationship is an essential ingredient in the stimulus–response experiments and in the reasoning that follows from them.

Special technical definitions are useful within a discipline and among scholars who agree on the unexpressed emotive load of the terms that is conveyed without having either to state it or defend it. Communication is expedited by the sense of exactness these special terms convey, and further exploration of statistical parameters can be carried on free of the doubts and hesitations that more philosophical or semantically oriented phrases inevitably pose. The laboratory term "punishment" is stripped of all historical associations, all derivational implications. No consideration of the intention of the experimenter or of the feelings of the subject is admissible. The idea of retribution, the law of the talon, the eye-for-an-eye prescription, all are outside of and irrelevant to the pursuit of the scientific study of punishment. Aversiveness is only implied, not stated. "Any stimulus that reduces the frequency of the behavior that precedes it [Baer, 1971, p. 34]," as the simplified version has it, does not limit in any way what the stimulus might be and does not require that it be painful or even unpleasant. The only stated criterion is that it be successful in diminishing or eliminating the behavior chosen by the experimenter as the object of the procedure.

Outside of the special environment of the laboratory or seminars in experimental techniques, the stripped-down-clean meaning of punishment as defined by experimental technicians shivers naked in a world in which tradition, ethics, social policy, law, legislation, child rearing, delinquency, war, feuds, power, gambling, daredeviltry, ambivalence, masochism, and assorted evils and goods vie with the spirit of scientific inquiry for priority. Words used in a technical sense in the laboratory are heard elsewhere as if embedded in another matrix. In the case of computers, if laymen conjure up visions of loops and bits dangling like tinsel monkey wrenches inside a magic machine, no particular harm is done. But in the case of punishment, the consequences of a dual vocabulary can be fraught with legal, medical, social, and educational booby traps that range far beyond the narrow applications found in the literature on punishment in the laboratory sense.

A startling example of this is supplied by B. F. Skinner (personal communication, 1972). He wrote: "My name was being cited by both sides in a lawsuit in a southern city in which a school was being sued for using corporal punishment." Editorialized the *Phi Delta Kappan:* "Dallas public schools can keep the spanking rules they adopted after consultation with B. F. Skinner, the U.S. Supreme Court ruled recently [Editorial, 1973]."

In the city of Dallas, 24,035 instances of corporal punishment were recorded for the 1971–1972 school year, some of them so severe as to need medical attention and, in several cases, hospitalization. Thus, many thousands of children are subjected to punishment in the ordinary meaning of retribution and pain under laws that are defended, at least in part, by the laboratory definition of whatever will successfully change behavior. The dual definition is shockingly sharp in the cartoon by Johns in the *APA Monitor* (1972, 3[9 & 10], p. 3) of Scene: Rat Lab. Graduate student (with hair) to professor (bald): "Incidentally, we'll be short one rat this morning. The black and white one bit me and I bashed his skull in."

Psychologists seem to have been only peripherally aware that the laboratory definition has not been

adopted by the general public and that nonprofessionals do not understand the scientific attitude toward punishment with the same detachment that they do. In a personal communication (E. R. Church, 1963) we read:

> Frankly, I had not considered the distinction between correction and retribution, although I can appreciate that it may be a distinction of importance for human adults and for children over a year. The writers of dictionaries have considerable trouble in making their definitions compatible with current usage and also internally consistent. Since the concept of punishment has been used by psychologists, at least from the time of Thorndike, without any implication of retribution, I think the dictionary will eventually recognize our usage.

To date, this has not happened.

This article examines in some detail the two sets of literature on punishment: the laboratory literature as analyzed by Johnston (*American Psychologist*, November 1972) and as reported in the *Psychological Abstracts* (Vol. 47, No. 1–6; Vol. 48, No. 1–4) contrasted with the field study literature listed in Bibliography 2. I attempt to show that the laboratory group has underemphasized the reality of educational and child-rearing customs while the field study investigators have underemphasized the scientific aspects of their efforts. This dual approach to the study of human activities has given rise to acrimonious misunderstandings. In the decade ahead, the legal and judicial aspects of students' rights and responsibilities are slated for a national airing. Psychologists, both laboratory and educational, will be called on to act as expert witnesses to testify for legislative committees and to lead community groups as they try to come to grips with the new concepts involved in the civil rights of children. It would be simpler if one set of psychologists raised the banner, PUNISHMENT WORKS! and the other set waved the placard, BEATING IS BRUTAL! Alas, our unexamined impulse to take sides in such a debate depends more on the history of our own infancy than on which school of psychology educated us. A careful analysis is thus overdue. I shall make a beginning.

The question is not whether or not psychology is a science. In the tradition of chemistry, laboratory experiments on behaving beings break up behavior into manageable bits, to quantify, manipulate, change, and control it. In the tradition of astronomy, field studies gather data by observation in search of patterns, relationships, regularities, and recurrences. Both sets of activities are scientific in that they attempt to discover true facts about the objects of study and to bring order to otherwise miscellaneous phenomena. Both yield hypotheses that ultimately redirect human thinking and planning. In biology, both types of scientific activities have been used. Darwin on Galapagos and Mendel in his garden perhaps best exemplify the going out into the natural world to observe what is there as contrasted with manipulating bits within narrow confines to gain control over them. Darwin did not put the turtles through any set of tests, assigned them no tasks; astronomers cannot modify the behavior of the stars, but they are scientists nonetheless. To suggest that without manipulation, such as Mendel arranged in his pea rows, there is no science is patently absurd (Carlson, 1972, pp. 20–21). Psychology is indeed a science, and like biology it uses both observation in natural situations and testing in setups and reaches conclusions by both routes. As in all sciences where knowledge is thin and where conclusions contradict the folk traditions, informed hypotheses become best guesses. This is the position of psychology at the present time regarding corporal punishment of children in school.

In the 10 issues of *Psychological Abstracts* from January through October 1972, there are no entries under corporal punishment—nor have there ever been. It has never been a subject of sufficient concern to experimental psychologists to merit study except peripherally as one minor variable in studies of delinquency. Under "punishment" there are 108 entries, but 5 of these do not provide enough data to determine the nature of the material discussed. Of the remaining 103, 25 concern rats and 15 are about other animals such as monkeys, pigeons, turtles, and cockroaches, for a total of 40 animal punishment reports. Of the 63 studies concerned with human subjects, 20 used undergraduates, 19 used special adults (prisoners, alcoholics, stutterers), and 24 are about children. Of these 24, 5 were case studies ($N = 1$ or 2) in which electric-shock was applied to severely retarded and severely disturbed children with successful reduction of unacceptable behavior reported. Four reports used loud sound or white noise as the punishment of choice; 2 used symbolic punishments, 1 used the word "wrong!" as punishment, and in 1 the punishment was to read anti-Semitic literature.

All of the above were studies built on the laboratory manipulation paradigm. Most were concerned with timing, intensity, manner of delivery, intermittent scheduling, or other technical niceties rather than the immediate or delayed effects on the subjects. The definition of punishment was in all

cases—though not always stated—whatever was convenient, measurable, and successful. A few of the studies merit individual review for the light they shed on our topic.

"The Effective Use of Punishment to Modify Behavior in the Classroom" (R. V. Hall et al., 1971) is one of the few studies in which the kind of punishing behavior is the subject matter. After noting the "controversy among the proponents of behavior modification regarding the use of punishment," Hall et al. conducted four experiments using as punishments: finger pointing with a shouted, "No!"; withdrawal of name slips; staying after school; and after-school tutoring. Their finding was that "systematic punishment procedures which do not result in strong emotional behavior do have a general applicability to classroom problems [p. 3]." Quoted out of context, the title and conclusion of this article could, and probably will be, quoted in defense of corporal punishment, with "strong emotional behavior" assumed by nonpsychologists on the school board to mean violent screaming, frothing, and thrashing about of the limbs. Consulting psychologists need to be prepared to counter such assumptions with a fuller report of this particular study pointing out that after-school tutoring is being labeled as punishment in spite of valiant efforts of educators to present it as opportunity. Otherwise, laymen may placidly assume that their prejudice in favor of old-fashioned whacks has been confirmed by modern psychology. This enhances their approval of "Psychology" but does nothing to increase their understanding of the effects of painful punishment on schoolchildren.

Similar misunderstanding has been caused by rapid readers of Donald Baer's (1971) article "Let's Take Another Look at Punishment" in *Psychology Today*. Both Hall et al. and Baer begin by defining punishment as "any consequence of behavior that reduces the future probability of that behavior [Hall et al., 1971, p. 1]" or "any stimulus that reduces the frequency of the behavior that precedes it [Baer, 1971, p. 34]." Both assume that corporal punishment went out with the gay nineties: "Hitting misbehaving students with rulers or making students sit in a corner with a dunce cap on their heads were common practices in the *early days* of American education [Hall et al., 1971, p. 2, italics added]."

> I think that much of our revulsion regarding the use of punishment is based on a reaction against the truly inhumane conditions of *many years ago* that recur in literature—headmasters with canes, slave masters, prison turnkeys with whips, bullies, orphanage overseers, snakepit mental hospitals. We like to think of such practices as long past, so when a therapist speaks objectively about the uses of punishment, we react as if he were asking us to forget all the years of progress and reform [Baer, 1971, p. 36, italics added].

There are many Americas. In the kindly America in which these experimentalists must have grown up, children were taught without blows to behave. The assumption that there have been "years of progress and reform" is one that many successful Americans make. Nat Hentoff, who researched corporal punishment for the American Civil Liberties Union, began his November 1971 (Civil Liberties) article with this admission:

> If asked two years ago, I would have expressed doubt that corporal punishment was still an issue in our schools—except perhaps in a rural fastness or at the hands of a disruptive teacher who would be sternly warned by his principal not to beat a child again. Whatever the other failings of our schools, at least physical barbarism—and its festering psychic residue—were safely locked into the past. Or so I thought [p. 2].

"I was shocked out of my ignorance," he wrote as his research progressed. "Corporal punishment . . . is far from extinct. In a number of places, most egregiously Dallas, the brutalization of children appears to be a part of the core curriculum [p. 2]."

The documentation of the brutality there and elsewhere—slashings with tire chains, front teeth knocked out, strapping a 13-year-old on the testicles, dumping a boy head first into a garbage can and beating his buttocks while he struggled for breath, thrashing a girl so severely that her menstrual periods ceased for three months—school phobias of great severity have happened in California and Kansas, in Minnesota and New Mexico. Only 3 states, New Jersey, Massachusetts, and Maryland, forbid corporal punishment, and 17 states have passed laws expressly allowing physical punishment of pupils.

In the vocabulary of legislators, corporal punishment carries the dictionary and popular meaning: painful penalty as a consequence of the performance of a forbidden behavior. The behaviors so forbidden are defined by the teacher or principal of each school to suit his temperament and convenience. It may be anything from tripping a teacher to returning 30 seconds late after recess. Nor is it generally asserted that such paddling, punching, whipping, slapping, slamming against the wall, or other battering will "reduce the frequency of the

behavior that precedes it." The boy failed to say "Sir"; therefore, he has earned three blows.

In the vocabulary of the emotionally needy, primarily in the poverty culture, parents who have been identified as child batterers see punishment in the sense of pure retribution without consideration of future activities.

> This group has a very strong sense of good and evil, right and wrong. These individuals have a very, very definite pattern—when an evil has been committed, punishment must occur. The punishment wipes out [the evil] . . . and so the parents feel they are really doing this child a special favor by inflicting this punishment, and this really makes them very good or right [Fawcett, 1972].

Documenting the difference between the laboratory and the lay meaning and emotive load of the word punishment does not suggest that one or the other is correct. My purpose is to reiterate the warning that the word carries markedly different meanings depending on the context and the user. Phrases associated with punishment and child-rearing practices when modified or used symbolically in the confines of the laboratory must also be translated for general applicability with as much exactness as is used in the experiments themselves. An example of such a related phrase that is wide open to misquoting and misunderstanding can be cited in the study entitled, "Interactions between the Facilitative and Inhibitory Effects of a Punishing Stimulus in the Control of Children's Hitting Behavior" (Katz, 1971).

"Hitting behavior" is one of the most persistent problems encountered by school disciplinarians. A study that purports to shed some light on how to keep boys from hitting others on the playground is likely to gain a wide audience among those who look to the science of psychology to solve a ubiquitous practical problem in playground control. The claim of experimental psychology to the status of a science lies in the assumption that behavior can be, and is, reduced to manageable bits and studied in a simplified, "pure" form under controlled conditions, quantified so exactly that mathematical comparisons have as much predictive value as do measurements of mass and energy of inanimate matter. To study hitting behavior in this way, one must determine first if it is indeed a homogeneous behavioral unit. In hitting the keys of a typewriter, in hitting a nail on the head, in connecting a bat with a pitched ball, in swatting a housefly, in probing for a solution, and in many other activities I may describe what I am doing as hitting behavior. English is a rich, fulsome language, but it tends to make short words of Anglo-Saxon origin cover by analogy a wide range of meanings.

In the experiment referred to, the "hitting" of the title was directed at a toy clown that dispensed gumdrops or pennies and simultaneously or alternately an unpleasant sound. Subjects were given opportunity and approval for playing with the clown, and those who did not hit it for the dispensations were dropped from the study. The results of the study are not questioned; measurements were meticulous. But simulation "to avert many of the ethical problems associated with punishment operations" has its own special hazards. Striking a candy-dispensing inflated toy after this had been modeled by a male graduate student in a mobile unit situated on school grounds might by some stretching be considered a unit of behavior, but can it be equated with striking a peer who has emitted insulting remarks about one's mother, one's body build, or one's academic status? The element in common is the English word "hitting," a slippery concept at best, and the flexion and extension of some of the same muscles of the forearm. Such equating or extrapolating of results from one situation to the other is unjustified and dangerous. Nevertheless, the experimenter and his mentors in graduate school felt free to conclude that punishment, undiluted by subsequent or simultaneous reward, is more effective in extinguishing hitting behavior than is a combination thereof. He even went so far as to chide those parents who lavish affection on the punished child to reassure him of their continued love or to salve their own guilt at having struck a defenseless child or for some other subjective reason. The conclusion to be drawn in the applied situation in school that punishment for fighting on the playground should be "pure" and without incidental attentional or other compensations to facilitate reentry is an unfortunate non sequitur that cannot be justified scientifically or otherwise.

Among the impurities of units of behavior is the almost unstudied difference between prescriptive and proscriptive instructions. If rewards facilitate and punishments restrict behaviors, the way in which a behavior is described will determine which will be the stimulus of choice. Shall we punish a child for "hitting" or for not maintaining his cool? Shall we reward him for "not hitting" or for coming to report the provocation to the one in authority? Or to make it still more complex, shall we punish him for fighting or for tattling when these are the

only possible behaviors in a particular situation? How do children themselves see it? Do they consider any instance of punishment as the result of a sin of commission or one of omission? Or both? In a study of retrospective attitudes of adults concerning their own childhood (McKinney, 1971), it was found that they "consider the positive effects of their behavior to be the result of the right things they do, while they consider the negative effects to be the result both of the wrong things they do and the right things they fail to do [p. 78]." The well-known phenomenon that children rarely are able to remember or report what behavior or lack of performance preceded a particular punishment although the punishment itself is clearly imprinted on the memory may be a function of the mixed prescriptive or proscriptive description given to the child concerning his actions. "Results attributed to the relative efficiency of reward and punishment might take this differential into account [McKinney, 1971, p. 79]." It may be that "units of behavior" are a romantic fiction, unitary only semantically or in the verbal behavior of the experimenter and determined to need modifying or extinguishing without a certain analysis of alternatives.

"Out-of-seat behavior," an even more popular target for applied behavioral modification techniques in the classroom, demonstrates that the rigid rejection of so-called extraneous factors cannot be justified in the search for accuracy and effectiveness. Reward for remaining seated or penalty for standing up has been used to accomplish that most desirable of all conditions—to the teacher—that the child remain in his place rather than wandering about the room. But again, in-seat or out-of-seat is not a unit of behavior; it is necessarily imbedded in a situational matrix that is ignored at peril of ultimate failure.

Rising from a seated to a standing position is an act that is in itself neutral, neither good nor bad, dangerous nor safe, emotionally stable nor disturbed. It cannot possibly be eliminated from the repertoire of human activities without physically crippling the individual involved. In one situation, as when the bailiff so orders, it is legally required. In another, as when the national anthem is played, it is customary. In a third, as when dining with a group, it might be annoying. And in still another, as when the plane is taking off, it is prevented by a seat belt. In the classroom, it may partake of all of the above requirements at different times of the day.

The experimenter verbalizes that it is not important what the student "thinks" or "feels" but only what he observably does, but it is very doubtful if the student identifies his behavior as abstracted from the surround, or the rewards of tokens as other than a game that changes the emotional tone of the whole from "resentment of obedience requirements" to "enjoyment of chance possibilities." If the modifier refuses to admit this contamination of his clean "unit of behavior" manipulation, his lack of observation of the experimental field necessarily leads him to faulty conclusions.

The discovery that humans will perform allotted tasks for token rewards was made by the best minds of the Roman Empire and rediscovered postfeudally. The discovery that humans will concentrate on learning tasks for intrinsic rewards such as pleasure in good workmanship (Veblen) and joy in mastery and elation in discovery, the AHA! principle (Maslow), has yet to be considered by the manipulators. Small wonder that their successes are with the incompetent, the retarded, the disturbed, the young, and the powerless. No study to my knowledge has undertaken to discover the results of token rewards on gifted, creative people. Here, the results would be confused by the propensity to delay gratification, to sense both pleasure and pain in any reward, to feel embarrassment at fulsome praise and pleasure in being corrected because it signifies being taken seriously.[2]

A final critique of defining punishment as "any stimulus that reduced the frequency of the behavior that precedes it" must address itself to that word "any." We have been reassured that punishment does not have to be painful (Baer, 1971), nor necessarily result in a "strong emotional response" (Hall et al., 1971), but otherwise no limits have been placed on what it might be. This cavalier attitude about "any" contrasts with the paucity of punishing agents beyond electric shock and also with traditional deprivations used by schoolmasters since the profession was recognized in ancient Egypt. Let me suggest a few "punishments" that fit the approved definition but might not be so defined by the subjects. A hurt child's crying can be reduced with a kiss and a Band-Aid. An active child's jumping about can be reduced by placing

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[2] Research regarding the effects of rewards on normal and gifted children undertaken since 1970 by Mark R. Lepper and David Greene of Stanford University suggests that a token economy with these groups interferes with, rather than facilitates, learning (Lepper & Greene, 1974, in press).

a television set 10 feet in front of him. The future probability of leaving the school grounds at noon can be reduced by providing free pizzas in the lunchroom. Pouting and "I won't" behavior has been eliminated by assigning an underplaced child to a higher and, therefore, compatible grade level.

The choice of "punishment" or, more properly, the "corrective," depends on the reason for the behavior. Out-of-seat behavior, in one school district during a period of 10 years, has been caused by a fistula involving the coccyx, a recent whipping on the buttocks, pinworms about the anus, tight underwear, no underwear, a minor kidney infection, an undiscovered visual defect, boredom, inappropriate subject matter seen as insulting (a story about white gloves at a girl's birthday party assigned to a Klamath River Indian boy), hunger, overtutoring, anxiety about a home situation (divorce, fire, brother arrested, illness of mother), a continuing feud, and a mental set caused by overhearing much criticism of the school as being "too permissive; they let the kids do anything they want to."

Choice of "punishment" thus becomes the major task of the behavior modifier. The casual adoption of candy or tokens as reward in all cases and the infliction of such aversive stimuli as electric shock, withdrawal of tokens, "time out" in various combinations, or other simplistic, narrow reductionism of "any" is not enough. Across-the-board one-menu-for-all methods are insulting and doomed eventually to passing as a ridiculous fad. The search for an individual prescription to fit a particular child in a particular situation with a particular background and history does not need to be seen as "Freudian," nor indeed as an abandonment of learning principles. It does require a more careful observation of the whole field and an individuation of recommendations for corrective measures. Without such case study methods, "psychology" has merely substituted electric shock for the whip, and corporal punishment remains the fate of the powerless. No plea for "objectivity" will still the moral revulsion of a free people against this return to barbarism.

The use of the word "corrective" is highly recommended in place of both reward and punishment as more accurate, as eliminating the semantic interchangeability of reward and punishment for prescribed and proscribed behaviors, as more likely to solve the dilemma of the ethical reservations of those who have learned to handle interpersonal exchanges without the use of force and fear, and, most important of all, to prevent the kind of misquoting and misuse of learning principles that flowed between Harvard and the U.S. Supreme Court, thus affecting the cultural lag of Texas.

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Part 2: Punishment Theory

Among psychologists, Adler (1970) was perhaps the first to set up community clinics and to offer general advice about child rearing. He strongly emphasized the futility of corporal punishment. "Corporal punishment in childhood," he wrote, "leads to low courage in adulthood." His followers have continued to emphasize natural consequences and to advise avoidance of power struggles with the young. "The punished student will want to avoid school, to look for means of escape, not means of meeting the difficulty [Ansbacher, 1956, p. 401]." "The punished shrug it off as the fortunes of war." And, "Our classrooms are filled with acts of retaliation [Dreikurs, 1957]." These conclusions, laced in the telling with many examples culled from teachers' stories, are open to accusations of lack of rigor. Control groups are noticeably absent; populations observed are not described as to intelligence, ethnicity, socioeconomic status, or other variables. Reported improvements on application of these prescriptions were not subjected to confirmation by objective tests. Nor is there much evidence that Adlerians listen to or observe the objects of their concern with any degree of scientific curiosity. Such

practical advice as they offer results in successful changes of behavior according to well-documented claims, but is, of course, vulnerable to accusations of "anecdotalism."

Freudian analyses of the effects on children of physical punishment might have followed from the essay *A Child Is Being Beaten,* except that the patient's confession was treated as fantasy and no more credence given its actuality than if it were dream stuff. The concentration on childhood sexuality has centered Freudian thought on the trauma of observing intercourse, the fear of castration, and the emotional impact of the family drama rather than on survival or other ego tasks. Neo-Freudians, however, have attended to these other aspects and have sought to fit them into the general framework of psychoanalytic thought:

> The buttocks as the locus for the induction of pain (is considered) a safe locus. However, the anal region is also precisely the major erotic zone at the time at which the child is likely to be beaten there. Thus, it is aptly chosen to achieve deranged sexuality in adulthood [Bakan, 1971a, p. 113].

> Trauma in childhood, including corporal punishment, may be associated with an interruption of normal sexual development and lead to numerous other forms of disorder [Alexander & Ross, 1961, p. 104].

> Violent adults endured violence when they were young [Curtis, 1963, p. 386].

So widespread has Freudian thinking permeated the intellectual community that applications appear in the literature of other disciplines, notably anthropology. The cryptosexual aspects of paddling the anal area are described as a relic of primate behavior in *The Naked Ape* (Morris, 1967): "The female rump presentation posture as an appeasement gesture . . . with rhythmic whipping replacing the rhythmic pelvic thrusts of the dominant male [p. 137]." Not content with such a shocking accusation, he drives it home in specific detail: "It is doubtful whether schoolmasters would persist in this practice if they fully appreciated the fact that in reality they were performing an ancient primate form of ritual copulation with their pupils [p. 138]."

Credence is added to this analysis by an excerpt from a confession of a child beater:

> There is no pleasure so pleasurable as the pleasure in another's pain. But one cannot get away with it completely. Somewhere a great deal of guilt follows on this sadism, and I am frequently burdened by remorse. I feel especially guilty that I sometimes get an erection and even have a climax during the beating. This is vile and hurts me dreadfully [Recorded from an interview with a patient by E. J. Anthony, 1969].

Since the 1940s the largest amount of attention to the effects of physical hurt upon the educational process has been given by social psychologists (Bowlby & Burbin, 1950; Glueck & Glueck, 1940, 1950; Redl & Wattenberg, 1959; Redl & Wineman, 1962; Staub, 1962; Winder & Rau, 1962; Yates, 1962), by developmentalists (Bakan, 1971a, 1971b; Baldwin & Baldwin, 1970; Button, 1969, 1973; Eron, Walder, & Lefkowitz, 1970; Feshbach & Feshbach, 1973; Ilg & Ames, 1951; Kagan & Freeman, 1963; McCandless, 1967; McCord, McCord, & Howard, 1961; Munsinger, Sears, Maccoby, & Levin, 1957; Parke, 1970; Rutherford & Mussen, 1968; G. Watson, Douvan, & Adelson, 1966; Yarrow, Campbell, & Burton, 1968), by educational psychologists (Adams et al., 1971; Bernard, 1954, 1969; Bernhardt, 1964; Cutts & Mosely, 1941; Falk, 1941; Fish, 1970; Gnagey, 1968; Goodman, 1966; Gray, 1966; Haring & Phillips, 1962; Holt, 1967; Hymes, 1955; Kohlberg, 1963; Kounin, 1970; Kozel, 1967; Kvaraceus, 1945; Ladd, 1971a, 1971b, 1972a, 1972b; Larson & Karpas, 1955; Levy, 1969; Livarle & Seligson, 1970; Long, Morse, & Newman, 1965; Maurer, 1963, 1972a, 1972b, 1972–73, 1973; Morgan, 1938; Nash, 1963; Phillips, 1960; Richardson, 1969; Sheviakov & Redl, 1956; Spielberger, 1966; Tonningsen, 1972; Webster, 1968; Wolfe, 1953), and by members of other professions (Birmingham, 1971; Brooks, 1886; Grier & Cobbs, 1968; Hentoff, 1973; S. Holt, 1967; Illingworth, 1968; Kennedy, 1963; Morris, 1967; Radbill, 1968; Reitman, Follman, & Ladd, 1972; Vaigo, 1968; as well as the venerable Harriet Beecher Stowe, 1858). In addition to these writers are the behavior modifiers, carrot branch, whose findings in contrast to those of the stick branch emphasize the superior effectiveness of reinforcement schedules over those consisting of aversive stimuli (Bandura, 1969, 1973; Bandura & Walters, 1967; Buckley & Walker, 1972; Carter, 1972; Estes, 1944; Katz, 1971; Skinner, 1938, 1948; Solomon, 1964; Watson, 1924, etc.).

The verdict was unanimous: All were opposed. The flavor of these authors can best be absorbed by a small sample of culled quotations and short resumes:

> Physical punishment was the favored disciplinary method of both fathers and mothers of delinquent boys [Glueck & Glueck, 1950].

> Beating in child rearing actually has its roots in slavery [Grier & Cobbs, 1968, p. 138].

Corporal punishment escalates; discipline becomes harder to enforce. More energy is spent policing as children develop deviousness. The "bad boy" becomes the "hero"; gangs form to defeat the teacher (Bowly & Burbin, 1950).

Socially deviant behavior is common among boys subjected to restrictiveness and punitiveness (Winder & Rau, 1962).

Punishment may produce varieties of abnormal behavior such as rigidity, fixation, regression, aggression, displacement, primitivization, and resignation (Yates, 1962).

Among these studies are some that trace cruelty and punishment to its primitive origins: "Maltreatment of children has been justified for many centuries by the belief that severe physical punishment was necessary to maintain discipline, to transmit educational ideas, to please certain gods or to expel evil spirits [Radbill, 1968, p. 3]." From the schools of Sumer, 5,000 years ago, discipline oscillated between abandonment of the rod to its use to the point of savagery. We are entering a new phase of our history wherein one of our most serious concerns is the protection of our children (Radbill, 1968).

Basic texts, such as those by Falk (1941) and by Cutts and Mosely (1941), analyze the social structure and explicate the use of torture to maintain the power of one group over another. Both trace cruelty through the ages and conclude with reasoned, yet passionate, appeals for the abolition of corporal punishment in schools. These texts were published in 1941 during the height of World War II when the shock of the incredible cruelty of the Nazi concentration camps gripped the civilized world in a determination to eliminate official sadism for all time.

A more recent historical analysis of infanticide and child abuse was made in 1970 in which the phenomena were shown to be among the dominant themes in religious writings, in fairy tales, in Mother Goose rhymes, and in children's games (Bakan, 1971b). Current habits of spanking and slapping are seen as residual traces of the use of infanticide as a means of population control. During periods of famine, the numbers of children sacrificed to the gods increased markedly, thus providing symbolic relief but also "one less mouth to feed." During periods of comparative abundance or during wars or epidemics when the available food and the population reached an easier accord, the tendency toward substituting animals for humans softened the sacrificial rites. A related historical highlight is the story of the Sephardim who instigated the addition of a rider onto the Naval appropriation bill as it struggled through Congress in 1950 banning the use of the cat-o'-nine-tails to scourge sailors (Birmingham, 1971).

Developmental studies of parent–child relationships are unanimous in their findings that punitive parents produce poorly adjusted children:

> The child will do what the punitive parent demands only as long as he thinks that they will find out about his actions; he may not follow their prescriptions at all if he thinks he can "get away with it." The parent relying heavily upon punishment must continually be on guard, because his child is likely to rebel against his wishes [Berkowitz, 1964].

> We anticipated that punishment for aggression would lead to inhibition of aggression. . . . Field studies contradict these predictions. . . . Increased aggression was routinely found to be associated with increased punishment for this behavior [Eron, Walder, Lefkowitz, 1970].

Other developmentalists spell out strong suggestions: Spanking episodes should be analyzed; parents are told: "Think over each situation to see where we missed the warning signs and how we could have acted differently [Ilg & Ames, 1951, p. 366]."

> We must look carefully, thoroughly and rationally at the experiences of childhood and we must come out forcefully against those experiences that set the stage for later bigotry, brutality and violence—the symptoms of essential contempt for other people [Button, 1969, p. 121].

Among the educators we find the only apologists for corporal punishment (Bagley, 1923; Riebe, 1940; Stenhouse, 1967), but they tend to be quaint as well as dated. Most educators offer direction on how to avoid corporal punishment: "Regardless of state law . . . never touch a student in anger. *Never*. This means shoving, shaking, pinching, jerking at clothing, stepping on toes, and so on [Gray, 1966, p. 26]."

Exposé books describing in horrifying detail the happenings in junior high schools flooded the market in 1970 (Kozol, 1967; Levy, 1969; and many more). One of these books (Kozol, 1967) sparked the changing of the laws regarding children's rights in the state of Massachusetts; the other raised the specter of racism in describing the vicious methods of control, including karate chops, used in the classroom, and the counterculture of the children that this inspires and models. White teachers (in a black school) consciously treat with kid gloves all

those children whose parents are active (Levy, 1969).

Some educators address their attention to the effects on the individual child: "When we make children afraid, we stop learning dead in its tracks [Holt, 1967]." Some speak to teachers: "We need more discipline, not less, but self discipline [Hymes, 1949]."

"Punishment escalates when not controlled—even by men fundamentally well intentioned [Tonningsen, 1972]."

Gentle coaxing of administrators to try nonpunitive techniques on "kids accustomed to restrictions, threats and punishments" (Ladd, 1972b) offers a helping hand into new ways to even the most hard-bitten.

Some educators point stonily to the evil efforts of the raw exercise of primitive power on the undefended and powerless. Studies indicate that vandalism is the youth's attempt at retaliation for punishments and humiliations suffered in school (Kvaraceus, 1945), and strong demands are made for protective legislation for children. "Beating of children which is really an assault upon them should be abolished by law. Those who defy the law should be treated as any other law breaker [Adams et al., 1971, p. 172]."

The father of American behaviorism opposed whipping and beating for his own reasons. They are not advised, he wrote, because they cannot be regulated according to any scientific dosage. Punishment is either too mild to establish a conditioned negative response, or too severe, thus stirring up unnecessarily the whole visceral system of the child. "It is either too rare to meet scientific conditions or too frequent thus making for habituation and masochism [Watson, 1924, p. 184]."

The next wave of behavior-oriented psychologists adopted both of these hands-off policies. "We are now discovering at an untold cost in human suffering . . . that in the long run punishment doesn't reduce the probability that an act will occur [Skinner, 1938]." And again, "It works to the disadvantage of both the punished and the punisher [Skinner, 1938]." That he has been misquoted has been documented earlier, but it should be reiterated that there were no buttons to trigger electric shocks nor any attachment that emitted noxious stimuli in the Skinner box.

Most learning theorists, however, have used electric shock to motivate an animal as casually as most people flip a switch to start a motor. Their attention has then been free to consider the permutations of the experimental design. Rarely is the focus on the punishment itself. When it has been, as at the University of Pennsylvania, bewilderment seems to have been the chief result:

> I have not specifically considered the age factor in discussing the effects of punishment. . . . The data are a bit cloudy and in some cases seem to show that early punishment builds up an immunity to punishment, while in other cases the data seem to show that early punishment sensitizes animals to future punishment. Obviously there are factors at play that we do not understand [R. L. Solomon, personal communication, 1963].

Somewhat more murky to those engaged in out-of-laboratory behavior is the summing up: "I have argued that the emotional effects of punishment are felt through the Pavlovian conditioning of reaction and that the instrumental effects of punishment are noted through the reinforcement of instrumental acts through punishment termination [R. L. Solomon, personal communication, 1963]."

The interrelationship between corporal punishment of humans and ethical standards is of course the moot point:

> It is a complicated problem and the interrelationship between psychological efficiency and ethics seems evident. Clearly an inefficient use of punishment, one that has to be repeated over and over again, approaches depravity. The same punishment used once with excellent effects could be called wise kindness if the behavior that was discouraged by the punishment was of the type that could be harmful to the individual. We are in no position to make these ethical judgments because our psychological knowledge is so poor; however, someday our psychological knowledge will not be poor, and then ethical judgments must be made [R. L. Solomon, personal communication, 1963].

Since 1963 when the above was written, our psychological knowledge and our ethical judgments to be based thereon remain firmly embedded in ignorance. In pleading for a continued suspension of ethical judgments until we have had more time to experiment with punishment (Johnston, 1972), the experimentalists of this persuasion must begin to recognize that time has run out. Ethical judgments are being made. Furthermore, ethical judgments are changing rapidly from a willingness to live with primitive punitiveness to a recognition that permitting one child to be battered is to subject all of our children to the danger of victimization. When as an adult the erstwhile battered child shoots 18 people from a Texas tower, kills 14 nurses in a Chicago residence, or slays a movie colony party in Hollywood, the blood is on the hands of those who would give aid and comfort to the punishing parents who shaped these lives with-

out learning that corporal punishment is an ethical evil.

Children will soon become a comparatively scarce commodity because of the new freedom vouchsafed women to control their own reproductive processes and to bear only such children as they wish. Scarcity increases value. The winds of change are blowing toward greater valuing of fewer children. Under these conditions it behooves experimentalists to confront ethical judgments now, and social, developmental, and educational psychologists to subject their ethical judgments to the rigor of careful assessment.

Could we set aside our obsession with control? Could we, as the pediatricians have developed a symptomatology of the battered child, begin to develop a profile of the overpunished child?

Work in progress indicates a near perfect correlation between the amount and severity of physical punishment endured by a child from 2 to 12 and the amount and severity of antisocial aggressiveness that he displays during adolescence (Button, 1973; Welsh, 1974). But during the years that the beatings are a daily diet and before the child is strong enough to retaliate or defend himself, he is in school and we should be able to recognize and rescue him. Could hyperactivity be environmental rather than genetic? Could short attention span be a survival tactic developed by the child in a continuously punitive home? Are there glandular changes, such as an increase in adrenalin output that permanently changes the body chemistry in the young human under continual severe stress? Are there "soft signs" that point to overpunishment at least as clearly as signs point to neurological handicaps? What really is "minimal brain damage" and can these symptoms be induced by early brutality?

All of these have been advanced as possible correlates of severe and prolonged physical punishment. A first study of the "at risk" child sees him as one whose apparent brightness does not show up as more than a dull IQ. He is danger oriented, tense and guarded, visually hyperalert, lies blandly in defense of his parents, and seems precociously "mature" (Martin, 1972). Is this the whole picture? Here, again, control groups are needed, especially because many grown males will claim defensively that they were punished as children, suffered no ill effects, and certainly did not become criminal.

These unknowns, rather than schedules of reinforcement or even rehabilitation techniques, must be unraveled before we can begin to see any hope that the next generation will be less traumatized and less violence prone than our own.

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Current Status of Graduate Education in Psychology

PETER F. MERENDA *University of Rhode Island* [1]

In anticipation of resolutions endorsed by the Vail Conference participants relative to psychological academic programs, in general, and the professional training of psychologists, in particular, and in recognition of the fact that there is considerable controversy within the psychological discipline itself regarding graduate curriculum content, educational objectives, and models of training, the study reported here was undertaken. Throughout its recent history, academic psychology has been confronted with great and continuing divergence of opinion with respect to the nature of graduate training programs, generally, and with specific reference to the appropriate education and training model for graduate students in the applied areas of psychology, notably clinical psychology. Note the many national conferences held on this theme: the Boulder Conference, the Chicago Conference, and now the Vail Conference.

Method

INSTRUMENT

A two-page questionnaire was developed to elicit, on a national scale, both factual information and some opinions from the chairs of departments of psychology. The questionnaire was mailed out at the end of July 1973 to 60 chairs of departments of psychology in universities in the United States offering doctoral-level programs in both basic and applied psychology. The great majority of these departments are offering APA-approved doctoral programs in applied psychology, principally clinical psychology. By the end of August 1973, a total of 48 (80%) schools had responded.

SAMPLE

The chairs of psychology departments who responded were from 32 state universities and 9 private universities. The majority of these schools maintain rather large doctoral programs in psychology. Of the 48 departments that responded, 35 reported having more than 30 doctoral students matriculating at any one time; only 10 reported having between 11 and 30 students; and only 3 schools reported having fewer than 10 students pursuing the doctorate at any one time. With regard to the number of doctoral degrees conferred annually, the sample is comprised of 35 departments reporting more than 10; 11 departments reporting between 6 and 10; and only 2 departments reporting fewer than 5.

Results

NATURE OF DOCTORAL DEGREE

All departments responding to the questionnaire offered the PhD degree in psychology. Only two schools reported offering an additional doctoral degree through the department of psychology. They were the University of Illinois (PsyD) and Teachers College, Columbia University (EdD). No other doctoral degrees were reported.

CORE COURSES

Of the 48 departments in the sample, 30 reported requiring the successful completion of core courses of all doctoral students in psychology. The number and nature of these courses and proseminars varied among the departments claiming to hold to this requirement. The distribution of the number of credit hours involved in the core course requirement is presented in Table 1. The median number of required credit hours was 17.50 (based on $n = 28$).

The nature of these required courses is presented in Table 2. The courses in this table are listed in descending order of frequency of report.

The data of Table 2 show clearly that the 30 departments that maintained a core of required courses for their doctoral students considered the following content areas in psychology to be most important: statistics (29), social psychology (12), child/development psychology (13), learning (11), personality (10), and physiological psychology (8). These data also reveal the diversity in requirements.

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[1] A copy of the complete report, with supporting materials, may be obtained from the author.

Requests for reprints should be sent to Peter F. Merenda, Department of Psychology, University of Rhode Island, Kingston, Rhode Island 02881.

TABLE 1

Distribution of Credit Hours Reported for Core Courses

Credit hours	Frequency
42–46	1
38–41	0
34–37	1
29–33	1
25–28	5
21–24	3
17–20	3
13–16	4
9–12	5
5–8	3
<5	2
Variable	2
Total	30

A total of 32 separate courses were given by the 30 respondents. This level of diversity regarding the specific required core courses parallels the diversity reflected in Table 1, which reports the number of such courses required by each of the 30 departments. The typical requirement is approximately 6 courses, but the reported number of credits required ranged from 6 (2 courses) to 45 (15 courses).

The remaining 18 departments, which reported that they did not require doctoral students to take any specific core courses, offered a variety of reasons to explain why such courses were not demanded. These reasons may be broadly classified into the following categories: (*a*) Outside minor areas are substituted for core courses; (*b*) qualifying or special examinations are substituted; (*c*) the faculty simply could not agree as to what should constitute the core.

CONDUCT OF COMPREHENSIVE EXAMINATIONS

The 48 departments that responded to this survey reported the following categorical information regarding the committee that is responsible for the conduct of the PhD comprehensive examinations: student's program committee (19), departmental examining committee (19), special committees or other examining bodies (11). The 11 departments that reported examining procedures of the latter category submitted the following explanations of how these examinations are conducted:

1. Each student must pass an advanced, in-depth examination in his chosen area. This examination is graded by a committee of at least two faculty members. Ordinarily, a student must pass this test by the end of the second year.

2. Ad hoc committee conducts examinations depending on exact nature of student's specialization.

3. Performance on core courses constitutes preliminaries. Decisions are made at the end of the first year. Students also must prepare a publishable paper in a significant area.

4. Ad hoc committee conducts examinations depending on the student's goals.

5. Examinations are conducted by each area group, consisting of at least three staff members.

6. General comprehensives (departmental examining committee) and preliminaries (student's own program committee) are administered.

7. First-year exams are the responsibility of a committee, one half of which is comprised of departmental examining committee and one half by student's program committee.

Second-year exams are the responsibility of a student's program committee. As an alternative to taking a three-hour exam in major specialty, a three-hour exam in methodology, a two-hour exam in specialty area of student's choice, and another two-hour exam in a second specialty area outside the major, the student may negotiate a contract

TABLE 2

Specific Core Courses Reported

Course title or area	Frequency
Statistics (two courses)	17
Social	13
Child/developmental	13
Learning (one course)	12
Statistics (one course)	12
Personality	10
Physiology	9
Perception	5
Psychopathology	4
Sensory processes	3
Motivation	3
History and systems	3
Research methodology (one course)	3
Learning (two courses)	2
Advanced experimental	2
Cognition	2
Research issues/problems	2
Issues and ethics	2
Measurement application	2
Comparative	2
Psychometrics	1
Research methodology (two courses)	1
Contemporary problems	1
Biological foundations of behavior	1
Conceptual foundations	1
Instrumentation in psychology	1
Advanced general (two courses)	1
Human experimental psychology	1
Psychobiology	1
Applied behavioral sciences	1
Behavior assessment seminar for clinicians and internship	1
Differential psychology	1
Proseminar in general psychology	1
Laboratory courses (two)	1
Research practicum	1

consisting of a series of accomplishments, subject to faculty approval.

8. A divisional examination is administered.

9. Written examinations are given, to which entire faculty contributes in writing questions, grading, and overall evaluation.

10. Graduate Record Examinations are given by the college.

11. The department examines the candidate in history and systems and in a second elective area of psychology. The program committee examines the student in his major area. The oral comprehensive examination involves departmental (on occasion) and nondepartmental representatives.

These data were further analyzed from another standpoint, taking into account whether core courses were required. This two-dimensional breakdown is presented in Table 3. The figures in parentheses are the proportions of the row totals represented by each of the cell frequencies. They serve as a better description of the situation, as reported by the 48 respondents, than do the cell frequencies themselves. Departments requiring core courses for all of their doctoral students reported that these same students take comprehensive examinations given by their own program committees or by other arrangements in greater proportion than those students who are not required to enroll in (and pass) required core courses. On the other hand, these latter students are examined by departmental examining committees or special examining committees in greater proportion than the former.

SCOPE OF COMPREHENSIVE EXAMINATIONS

Of the 48 departments that responded to the questionnaire, 26 claimed that the comprehensive examinations administered to their doctoral candidates are broad and comprehensive in scope. Another 10 departments reported that the comprehensive examinations are, in part, broad and integrative and, in part, narrow and specific. Only 4 claimed that their comprehensive examinations were narrow and specific. The remaining 8 departments reported that the comprehensive examinations varied, essentially depending on the area involved.

TABLE 3

Conduct of Comprehensive Examinations by Core Course Requirements

Core courses	Departmental examination committee		Student's program committee		Special committee		Other		Total
	n	%	n	%	n	%	n	%	
Required	9	(30)	13	(44)	1	(3)	7	(23)	(30)
Not required	9	(50)	6	(33)	3	(17)	0	(0)	(18)
Total	18		19		4		7		48[a]

Note. Figures in parentheses are the proportions of the row totals represented by each of the cell frequencies.

[a] One department reported examinations by both department and program committees.

DISCUSSION AND IMPLICATIONS OF FINDINGS

From the data obtained in this survey (see Table 4) it becomes immediately obvious that at the present time, the PhD degree is, for practical purposes, the only doctoral degree that is being awarded in psychology, in both the basic and applied areas. Taking into consideration the fact that the Vail Conference endorsed the doctor of psychology degree as the *appropriate* degree for the professional training of psychologists at the doctoral level (Korman, 1974), it becomes more obvious that the conference is asking all but one university (University of Illinois) to deliberate the changes.

Another resolution passed by the conferees at Vail is that the nature of core academic and professional training requirements be broadened. It was further recommended that "nonfunctional" requirements be modified or deleted. With the exception of statistics and to a lesser extent with the exception of social, developmental, learning, personality, and physiological psychology, there appear to be substantial discrepancies as to what should constitute a "core" among those departments that require a set of core courses of their doctoral candidates. The determination of how to modify and broaden—or whether or not to modify or broaden—the "core" is anticipated to be most complex and difficult.

Considering the varied reasons given by the minority departments as to why they have eliminated the core requirements, confusion must also be anticipated in establishing graduate program requirements so that "these requirements . . . [contribute to] the assurance that students acquire competence in the performance of professional activities [Korman, 1973, B–I–1]."

The Vail Conference evidently did not address itself directly to the issue of the nature and scope of comprehensive examinations required of doctoral students. Perhaps this was due to the fact that requirements are university-wide and are established and monitored by the graduate faculty at

TABLE 4

Summary of Departmental Requirements and Procedures

Institution	Degree(s) offered			Core courses		Conduct of comprehensive exams			Content of comprehensive exams			Number of matriculating doctoral students			Number of doctoral degrees awarded annually		
	PhD	PsyD	EdD	Yes	No	Departmental exam committee	Program committee	Special committee/ other	Broad and integrative	Variable	Narrow and specific	Fewer than 10	11–30	More than 30	Fewer than 5	6–10	More than 10
University of Alabama	X			X		X			X					X		X	
University of Arkansas	X			X			X				X			X		X	
University of California, Berkeley	X			X		X			X					X			X
University of Cincinnati	X			X				X	X		X			X			X
University of Colorado	X				X	X				X				X			X
University of Connecticut	X				X			X	X		X			X			X
Duke University	X				X		X			X				X			X
University of Florida, Gainesville	X			X			X		X					X			X
University of Florida, Tallahassee	X			X			X		X					X			X
Fordham University	X			X				X						X			X
University of Georgia	X			X				X	X					X			X
George Washington University	X			X				X	X		X			X			X
Georgetown University	X			X		X			X					X		X	
University of Hawaii	X			X		X			X		X			X	X		
University of Illinois	X	X		X			X		X					X			X
Indiana University	X			X			X		X				X				X
Johns Hopkins University	X			X				X	X				X		X		
University of Kansas	X			X		X			X		X		X				X
Louisiana State University, Baton Rouge	X			X			X		X		X		X		X		
University of Maine	X				X	X			X					X			X
University of Massachusetts	X			X				X	X		X			X		X	
University of Miami, Coral Gables	X			X				X	X					X		X	
University of Minnesota	X				X	X				X				X			X
University of Missouri	X			X			X		X					X			X
University of Montana	X			X			X		X								X
New York University	X			X				X	X				X			X	
University of North Carolina, Chapel Hill	X				X		X		X					X			X
Northwestern University	X				X		X				X			X			X
University of Oregon	X			X			X			X			X				X
Pennsylvania State University	X				X		X		X		X		X				X
University of Pittsburgh	X			X			X			X			X				X
University of Rhode Island[a]	X			X			X		X					X	X		
University of Rochester	X				X	X			X		X			X			X
Rutgers University	X			X			X			X				X			X
University of South Dakota	X				X		X		X					X		X	
Southern Illinois University	X			X		X			X				X			X	
State University of New York at Albany	X			X		X			X					X		X	
Syracuse University	X				X	X			X		X			X			X
Teachers College, Columbia University	X		X	X				X	X					X			X
Temple University	X			X			X		X		X		X				X
University of Tennessee	X				X	X			X					X			X
University of Texas	X			X			X				X			X			X
University of Utah	X				X		X		X								X
University of Washington	X				X	X			X					X			X
Washington University, St. Louis	X				X	X			X					X			X
University of Wisconsin	X				X			X		X			X				X
Unknown A	X				X	X								X			X
Unknown B	X				X	X			X		X			X			X
Unknown C	X			X		X			X					X	X		

[a] Home department of author.

large. However, on the basis of the data arising from the 48 departments that responded to the survey questionnaire, the following conclusions are tenable: (*a*) The examinations are, in the main, broad and integrative in scope; (*b*) the examinations are administered primarily by either departmental examining committees or the student's own program committee. In departments not requiring specific core courses of the students, the data show a greater tendency to examine students through a departmental examining committee rather than through the student's own program committee. In those departments requiring core courses, however, arrangements other than examination by either committee are possible, except for students in departments that do not require core courses.

In summary, the results of this survey reveal that at the moment doctoral training in psychology leads, for all practical purposes, to the PhD degree; that the majority of departments offering advanced graduate work in psychology insist on a core of required courses that is quite variable; and that the comprehensive examinations taken by these students are, in the main, broad and integrative and more likely to be administered by either a departmental examining committee or the student's own program committee. Finally, for those departments in which a core of courses is not required of all doctoral students, there are, nevertheless, substitute requirements, for example, demonstration of competencies and knowledge in content areas through examinations, or the successful completion of a minor academic area.

REFERENCE

Korman, M. National Conference on Levels and Patterns of Professional Training in Psychology. August 17, 1973. (Mimeo) (*American Psychologist*, 1974, 29, 441–449.)

Membership Characteristics of the Division of School Psychology of the American Psychological Association

BARTELL W. CARDON *University of Pennsylvania* [1]

One of the specialties within psychology that has been receiving increasing attention is school psychology. Within the American Psychological Association, the Division of School Psychology (Division 16) has steadily gained in membership, activity, and influence. At the time this article was written (summer 1973), Division 16 was fifth in total membership among the divisions and second only to Division 12 (Clinical) in number of representatives in Council.

Despite its size and visibility, there seems to be relatively little awareness among members of APA as to the composition and characteristics of the Division. Indeed, it would appear that many members of the Division harbor misperceptions.

Size and Geographical Distribution

As of June 1973, the Division of School Psychology had 2,485 members (127 Fellows, 1,346 Members, 1,012 Associates). Table 1 provides a geographical breakdown of Division members by zip code area.

Academic Degrees

The distribution of Division membership by degree is found in Table 2. Approximately 49% of its membership have earned the doctorate and 51% hold the master's or bachelor's degree. Of those holding the doctorate (1,213), 70% have PhDs. Although complete information is not available, it can be said with confidence that the large majority of members holding the master's degree (1,260) either received the degree after completion of two or more years of graduate study or have completed at least one year of study beyond the master's or both. Likewise, it can be stated with confidence that the majority of members holding the bachelor's degree (12) have completed one or more years of advanced study.

The dramatic increase of members earning the doctorate during the 1961–1970 period (Table 3) very likely represents the cumulative effects of the creation of formal school psychology programs during and slightly preceding the 1961–1970 period. More than 75% of all currently existing programs originated after 1955 (Cardon & French, 1968–1969). The first doctoral graduates of those programs would have received degrees in 1958–1960, with the number increasing thereafter.

Approximately half as many students in school psychology are prepared at the doctoral level as at the master's and intermediate levels (Cardon & French, 1968–1969). Of Division members obtaining their highest degree since 1961, more hold the doctorate than do not. It may be inferred that the Division is more attractive to school psychologists who hold the doctorate than it is to those who do not.

Primary Employment

For purposes of this study, employment is defined in terms of university versus nonuniversity settings. Approximately 75% of members of the Division are employed principally by an employer in a nonuniversity setting (Table 1). They work in public and private schools and clinics, for test publishers, and so forth.

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[1] The author was, at the writing of this article, Secretary of Division 16 and a Representative to Council. All information has been taken from membership records of the Division, the *American Psychological Association 1972 Membership Register,* and a short questionnaire sent to approximately 150 members for whom adequate information was not available. Appreciation is expressed to Judy Stephenson who compiled much of the data.

Requests for reprints should be sent to Bartell W. Cardon, Graduate School of Education, University of Pennsylvania, Philadelphia, Pennsylvania 19174.

TABLE 1

Division 16 Membership by Geographical Location and Type of Employment: 1973

Zip code area[a]	Nonuniversity		University		Total	
	N	%	N	%	N	%
00	20	1.1	16	2.2	36	1.4
0	274	14.9	56	8.9	332	13.3
1	459	24.8	146	23.1	605	24.3
2	139	7.4	49	7.7	187	7.5
3	85	4.6	54	8.5	139	5.6
4	201	10.8	83	13.1	284	11.4
5	138	7.4	41	6.5	179	7.2
6	169	9.1	54	8.5	223	9.0
7	74	4.0	40	6.3	114	4.6
8	66	3.6	28	4.4	94	3.8
9	227	12.2	65	10.3	292	11.7
Total	1,853		632		2,485	

[a] 00 = foreign;
0 = Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, Vermont, Rhode Island;
1 = New York, Pennsylvania, Delaware;
2 = Maryland, North Carolina, South Carolina, Virginia, West Virginia, Washington, D.C.;
3 = Alabama, Florida, Georgia, Mississippi, Tennessee;
4 = Indiana, Kentucky, Michigan, Ohio;
5 = Iowa, Minnesota, Montana, North Dakota, South Dakota, Wisconsin;
6 = Illinois, Kansas, Missouri, Nebraska;
7 = Arkansas, Louisiana, Oklahoma, Texas;
8 = Arizona, Colorado, Idaho, New Mexico, Nevada, Utah, Wyoming;
9 = Alaska, California, Hawaii, Washington, Oregon.

The distribution picture is interesting. Those areas with the fewest members have lower practitioner (nonuniversity)–academic (university) ratios than do areas with more affiliated school psychologists. That is, proportionally more members living in areas with few affiliated school psychologists are associated with universities than in areas with many affiliated school psychologists. The highest proportions (approximately 35:65) of university school psychologists are found in Areas 3, 7, and 8 (see Table 1). These areas represent a band extending across from Florida, west to Arizona, and north through the Rocky Mountain region. The lowest proportions (approximately 20:80) are found in the northeastern and far western states.

Sex

The current interest in and concern for the professional status of women recommends an in-depth evaluation of sex status within the Division of School Psychology, as reflected in membership. Some data reflecting sex have already been presented.

TABLE 2

Division Membership by Highest Degree and Sex: 1973

Degree	Males		Females		Total	
	N	%	N	%	N	%
PhD	607	39.6	239	25.0	846	34.0
EdD	283	18.5	84	8.8	367	14.8
Master's	633	41.3	627	65.7	1,260	50.7
Bachelor's	8	.5	4	.4	12	.5
Total	1,531		954		2,485	

There are 954 women (38.14% of the membership) in the Division (see Table 2), 323 (33.18%) of whom hold the doctorate. Slightly over 61% (1,531) of the membership is male, with 58.1% holding the doctorate. Although half (49.6%) of the Division members not holding the doctorate are female, only one in four (26.6%) of the doctoral members is female.

A look at Table 3 is informative. Female members with the doctorate are consistently outnumbered by female members not holding the doctorate when compared across years of last degree. Males with the doctorate outnumber those without at all years of degree except the 1941–1950 period. The influence of World War II may be seen in these data.

Of the males, 68.2% (1,046) are employed in nonuniversity settings. They represent 56.4% of the total nonuniversity school psychologists within the Division. Of the females, 84.6% (807) are employed in nonuniversity settings.

TABLE 3

Division 16 Membership by Sex and Degree: 1973

Year	Male			Female		
	Doctorate	Nondoctorate	Total	Doctorate	Nondoctorate	Total
1971	28	15	43[a]	9	0	9[a]
1961–1970	543	346	889	187	252	439
1951–1960	244	199	443	72	162	234
1941–1950	42	61	103	33	135	168
1931–1940	22	11	33	13	65	78
1921–1930	9	9	18	1	16	24
1911–1920	2	0	2	1	1	2
1901–1910	0	0	0	0	0	0
Total	890	641	1,531	323	631	954

[a] Applicants obtaining degrees in 1971 were generally not yet eligible for Division membership at the time of this study.

There are 632 Division members whose primary employment is university related, 148 (23.4%) of whom are women. The female–male ratio of university school psychologists varies greatly across the nation. Women are best represented in Area 0 (28.6% versus 71.4%), Area 7 (27.5% versus 85.4%), and Area 3 (14.8% versus 85.2%).

Of the female members, 47.8% (456) are Associates, 48.1% (459) are Members, and 4.1% (39) are Fellows. Of the male members, 36.3% (556) are Associates, 57.9% (887) are Members, and 5.7% (88) are Fellows. Although a larger proportion of the total male membership holds the status of Fellow than the females, it is noteworthy that a larger proportion of female members holding the doctorate are Fellows (12.1%) than males (9.9%).

It would be difficult to judge if there is a difference by sex in level of Division activity. One cannot overlook, however, an interesting fact. Although women represent less than 40% of Division membership, they presently hold more than half of the seats on the Executive Committee.

REFERENCE

Cardon, B. W., & French, J. L. Organization and content of graduate programs in school psychology. *Journal of School Psychology*, 1968–1969, 7, 28–32.

Erratum

In the Report of the Executive Officer: 1973, in the June issue, there are two errors. On page 368, the second sentence of the paragraph headed Publications should read: "In 1973 the journals contained 15,000 text pages, each of which had been carefully redacted for accuracy and format by one of the Association's 14 journal technical editors." The other 3 technical editors are employed on other publications of the Association, not the journals. On page 374, the second sentence of the paragraph headed Manpower Resources should read: "In 1973 the 46,000 questionnaire records received from the 1972 Survey were processed and converted into a computerized data file."

Audited Financial Statements of the American Psychological Association for the Year Ended December 31, 1973

It has not been the practice in the past to publish to the membership the audited financial statements for a given year immediately after their availability. Rather, the financial reports have been incorporated into the Treasurer's annual report. The cycle of events being what it is, the Treasurer makes his report at the annual business meeting of the Council of Representatives. By the time the Treasurer's report is then published to the membership, many months have elapsed.

Accordingly, and with the Treasurer's consent and approval, we are publishing the audited financial statements for 1973 independent of the Treasurer's annual report and will plan to follow this practice annually hereafter. The chief advantage is that the information will be made available to members of the Association as soon as possible. These reports will normally be published without interpretive comments. Such interpretation will be reserved for the Treasurer's annual report when a critical analysis is made of past experience, current operations, and future prospects.

Having said the above, it nevertheless seems appropriate at least to invite members' attention to the fact that APA sustained a deficit of $72,940 in 1973 which followed a $120,338 deficit in 1972. These figures can be seen in the accompanying "Statement of Operations and Net Worth." Not seen is the fact that these deficits in 1972 and 1973 followed a $21,987 deficit in 1971—making three consecutive years of deficits. Clearly, deficits cannot be allowed to continue, and the matter of stabilizing APA finances is receiving the close attention of the governance structure.

Board of Directors
American Psychological Association, Inc.
Washington, D. C.

We have examined the financial statements of the American Psychological Association, Inc., for the years ended December 31, 1973 and 1972. Our examinations were made in accordance with generally accepted auditing standards, and accordingly included such tests of the accounting records and such other auditing procedures as we considered necessary in the circumstances.

In our opinion, the accompanying balance sheet and statements of operations and net worth and changes in financial position present fairly the financial position of the American Psychological Association, Inc., at December 31, 1973 and 1972, and the results of its operations and changes in financial position for the years then ended, in conformity with generally accepted accounting principles applied on a consistent basis.

Ernst & Ernst

Washington, D. C.
April 8, 1974

TABLE 1

Balance Sheet of the American Psychological Association, Inc.

	December 31	
	1973	1972
ASSETS		
Current assets		
Cash (1973—$48,708; 1972—$142,427) and short-term investments:		
Unrestricted	$1,945,353	$2,113,748
Restricted for:		
Unexpended grant advances	83,085	91,498
Special purpose funds	13,415	13,163
	2,041,853	2,218,409
Marketable securities—at cost (market value 1973—$721,742; 1972—$631,585)	698,373	537,341
Accounts receivable, less allowance for doubtful accounts of $10,000	347,823	344,579
Prepaid expenses, deposits, and other	72,992	148,279
Total current assets	3,161,041	3,248,608
Notes receivable		
From officer—Note B	94,704	95,973
From a division of American Psychological Association	2,763	3,263
	97,467	99,236
Property and equipment, on the basis of cost—Note C		
Land	1,104,142	1,104,142
National Headquarters building	3,202,896	3,150,648
Furniture, fixtures, and equipment	314,440	281,955
Less allowances for depreciation	(1,158,567)	(1,033,903)
	3,462,911	3,502,842
	$6,721,419	$6,850,686
LIABILITIES AND NET WORTH		
Current liabilities		
Accounts payable and accrued expenses	$ 401,580	$ 395,299
Amounts collected for divisions	195,329	197,841
Advances on grants	83,085	91,498
Special purpose funds	13,415	13,163
Current portion of mortgage note payable—Note C	105,950	80,798
Total current liabilities	799,359	778,599
Mortgage note payable—Noncurrent portion—Note C	2,087,163	2,172,498
Unearned dues and subscriptions	2,791,165	2,782,917
Total liabilities	5,677,687	5,734,014
Net worth	1,043,732	1,116,672
Commitments—Note E		
	$6,721,419	$6,850,686

Note. Certain items in 1972 have been reclassified for comparative purposes. See notes to financial statements.

TABLE 2

Statement of Operations and Net Worth of the American Psychological Association, Inc.

	Year ended December 31	
	1973	1972
Revenue:		
Member dues and fees	$1,098,335	$1,033,794
Journal subscriptions	2,750,551	2,131,036
Grants and contracts	314,791	717,743
Advertising	254,907	200,816
Other journal revenue	216,623	223,304
Sales of other publications and services	835,815	415,257
Building revenue, non-APA tenants	286,637	299,552
Registration fees	126,587	64,975
Exhibit space rental	58,650	33,718
Accreditation fees	90,025	69,300
Investment income	106,816	89,441
Other revenue	135,253	108,955
Total revenue	6,274,990	5,387,891
Expenses:		
Employee salaries and benefits	2,415,982	2,068,591
Printing and mailing of publications	1,646,075	1,454,925
Editors' stipends and office expense	233,536	216,044
Professional, consulting, and contractual services	356,549	334,125
Travel, other than boards and committees	96,882	142,998
Boards and committees	216,048	192,779
Convention and placement local site expense	69,760	43,246
Supplies and other office expense	535,635	340,307
Building expense	561,391	552,779
Other expenses	216,072	162,435
Total expenses	6,347,930	5,508,229
Excess of expenses over revenue	(72,940)	(120,338)
Net worth at beginning of year	1,116,672	1,237,010
Net worth at end of year	$1,043,732	$1,116,672

Note. See notes to financial statements.

(*See notes to financial statements on page 638.*)

TABLE 3

Statement of Changes in Financial Position of the American Psychological Association, Inc.

	Year ended December 31	
	1973	1972
Source of funds		
Excess of expenses over revenue	$ (72,940)	$ (120,338)
Provision for depreciation	126,181	120,240
Total from operations	53,241	(98)
Increase in current accounts payable and accrued expenses	6,281	67,912
Increase in amounts collected for divisions	—	17,754
Increase in advances on grants and special purpose funds	—	60,785
Increase in unearned dues and subscriptions	8,248	492,154
Decrease in prepaid expenses, deposits, and other	75,287	—
Decrease in notes receivable	1,769	—
	144,826	638,507
Application of funds		
Increase in marketable securities	161,032	219,668
Increase in accounts receivable	3,244	72,695
Increase in prepaid expenses, deposits, and other	—	85,107
Increase in notes receivable	—	1,962
Additions to National Headquarters building	52,248	60,505
Additions to furniture, fixtures, and equipment, net of disposals (1973—$1,882; 1972—$1,181)	34,002	11,766
Reduction in mortgage note payable	60,183	76,503
Decrease in amounts collected for divisions	2,512	—
Decrease in advances on grants and special purpose funds	8,161	—
	321,382	528,206
Increase (decrease) in cash and short-term investments	(176,556)	110,301
Cash and short-term investments at beginning of year	2,218,409	2,108,108
Cash and short-term investments at end of year	$2,041,853	$2,218,409

Note. See notes to financial statements.

NOTES TO FINANCIAL STATEMENTS

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

December 31, 1973

Note A—SUMMARY OF SIGNIFICANT ACCOUNTING POLICIES

The Association is a tax-exempt organization dedicated to the advancement of psychology as a science and as a means of promoting social welfare.

Method of Accounting

The records of the Association are maintained on the accrual basis of accounting. Consequently revenue is recorded when earned and expenses as incurred.

Grants

The Association receives grant funding from federal agencies and private foundations, with amounts received restricted for use in fulfilling the terms of the grants. Revenue is recognized only to the extent of Association expenditures under the grants. Overexpenditures are borne by the Association and unexpended funds are returned to the grantors.

Property and Equipment

The National Headquarters building and furniture and fixtures are carried on the balance sheet at cost and are being depreciated on a straight-line basis over the useful lives of the assets as follows:

National Headquarters building, initial cost	40 years
Building additions	10 years
Furniture, fixtures, and equipment	10 years

Divisions

The Association has 33 autonomous divisions of special psychological interest which members may elect to join. As a service to the divisions, the Association will, upon request, act as collection agent for assessments paid by division members ($199,000 in 1973). The accompanying financial statements exclude the accounts of these autonomous divisions.

Note B—NOTE RECEIVABLE FROM OFFICER

The note receivable from officer bears interest at 6½%, is secured by a deed of trust, and is payable in equal monthly installments, including interest, through 1999. The note is callable at the Association's option upon 90 days' written notice. The Association has given notice to call the note due and payable as of April 18, 1974.

Note C—MORTGAGE NOTE PAYABLE

The mortgage note is payable to a bank, bears interest at 5½%, and is secured by the land and building occupied by the Association. Quarterly installments, including interest, are $50,770 payable through 1990.

Note D—RETIREMENT PLAN

The Association has a pension plan through which it makes contributions on behalf of eligible employees for the purchase of annuities. Contributions under the plan were approximately $86,000 in 1973 and $76,000 in 1972.

Note E—COMMITMENTS

During the year the Association entered into a 5-year lease for data processing equipment for the period January 1, 1974, to December 31, 1978. Minimum annual lease commitments are approximately $102,000 for the years 1974 through 1978. The lease is cancelable during the term of the lease upon 90 days' notice with retroactive price adjustments.

Rental expenses incurred during 1973 and 1972 were as follows:

	1973	1972
Data processing equipment	$74,729	$35,287
Office equipment	16,979	11,506
Other	5,963	2,074
	$97,671	$48,867

Comment

A Real Dilemma

Regarding Nolan's (March 1974) article, I agree that *ought* does not follow effortlessly from *is*, and that Skinner may have his head in the sand by encouraging scientifically minded psychologists, all nice fellows, to set up a perfect culture. But are we not also being proverbial ostriches if we reject science when questions of value arise? There is a real dilemma here. If values are not bestowed by supernatural forces, they must develop from within the natural system in biological and, particularly, cultural evolution (e.g., Campbell, 1972). The latter includes complex, social signaling processes known as *rational discussion*, for example, scientific interchange. There is no assurance that *any* of the paths we follow, such as those blazed with the help of science, will be healthy for us in the long run, yet we find ourselves pursuing science. On what basis do we decide to stop when questions of value arise?

REFERENCES

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Nolan, J. D. Freedom and dignity: A "functional" analysis. *American Psychologist*, 1974, **29**, 157–160.

ROBERT B. GLASSMAN
Lake Forest College

What *Is* Psychology About?

D. O. Hebb's (February 1974) article was unfortunately another example of dichotomous thinking that continues to fragment *people* into fictional parts under the guise of defining "psychology." In his attempt to espouse "scientific" fictions over a variety of other "nonscientific" fictions, Hebb made several unscientific conclusions:

> Science is the servant of humanism, not part of it. Combining the two ruins both [p. 74].

> Mind then is the capacity for thought, and thought is the integrative activity of the brain . . . [p. 75].

> Free will thus has a physiological basis, in the relative autonomy of the activity of the cerebrum [p. 75].

No scientific evidence was given for these conclusions in the article or in the Sperry (1968) article that he touted so highly.

Now I do not wish to argue for any of the so-called nonscientific approaches that Hebb used as straw men to demolish; nor do I wish to argue against Hebb's scientific position. My concern, however, is with this kind of either–or thinking—the "I'm right, they're wrong" attitude in psychology. I am very much involved in this issue of "what psychology is about" both as a teacher in the newly opened Psychological Studies Institute in Palo Alto, California, and as a practicing psychotherapist. An alternative to the "I'm right, they're wrong" attitude has been presented in my book, *The Nightmare of Success*. Psychologists are just as prone to get trapped in the "success syndrome" as are the business executives and professionals described in the book.

Being very "right" in one's position can effectively prevent one from being "wrong," in other words, different from what *has been* successful. Hebb mentioned both Claude Bernard's dictum that "those who have an excessive faith in their own ideas are not well fitted to make discoveries," as well as Barber's (1961) article, "Resistance by Scientists to Scientific Discovery." But Hebb apparently forgot their admonitions in his "right" answer to "what psychology is about."

Both Hebb's comment about "surprises" (not "rightness") being found "in the truly creative act," and Carl Rogers' comment about "serendipity" in the act of making discoveries, reported in Rogers' article (February 1974), could serve as an introduction to the approach that I presented in my book as an alternative to being right. In fact, I call the process by which highly successful and right individuals become real and human again "serendipity psychotherapy." Like Hebb, I heartily recommend it to those who wish to form opinions on this issue, and even to those who do not.

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WILLIAM J. RUZICKA
Psychological Studies Institute
Palo Alto, California

-Fem-ale or -Mal-e Authorship?

Immediately after reading "The History of Psychology Revisited: Or, Up with Our Foremothers" (Bernstein-[fem] & Russo[fem], February 1974), which decried the inability of readers to determine whether a reference is authored by a male or a female, I read in the same issue of the *American Psychologist* "What

Psychology Is About" (Hebb[mal], February 1974). The latter article mentioned the early Russian psychologist Shenger-Krestovnikova, of whom I have never heard. I am nevertheless sure that this psychologist was female because the Russian language distinguishes between male and female last-name endings. This suggests a much-needed repair to our method of citation. We should add, at the end of each name, a suffix to indicate sex. The suffixes that immediately come to mind are *-ma*, for male, and *-fe*, for female. But perhaps my male chauvinism is showing, for the female suffix is quite similar to the Yiddish expletive for dirt. Alternatively, we could use *-mal* and *-fem*, although the male suffix would suffer from "bad" connotations. The second set of suffixes seems preferable, and I urge their adoption.

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Bernstein, M. D., & Russo, N. F. The history of psychology revisited: Or, up with our foremothers. *American Psychologist*, 1974, 29, 130–134.

Hebb, D. O. What psychology is about. *American Psychologist*, 1974, 29, 71–79.

E. J. Kay[mal]
Lehigh University

Comment on Rogers

I was surprised to hear Carl Rogers (February 1974) say that I have "cheated the profession" by refusing "to permit the nine-hour confrontation we held at the University of Minnesota to be released." As I remember, it was not my permission but my advice that was asked for, and I said that the quality of the recording was so wretched that I did not feel it fair to offer the tapes for rental or sale. I have no objection whatsoever to the tapes being made available if anyone is willing to listen to them. I have heard a recording made by a member of the audience which is much clearer and might be a better candidate for release.

I do not, however, relish the prospect of editing my share of a nine-hour transcription, and I should not want a transcription circulated without editing, particularly considering the bad recording. That was certainly never part of our original agreement.

REFERENCE

Rogers, C. R. In retrospect: Forty-six years. *American Psychologist*, 1974, 29, 115–123.

B. F. Skinner
Harvard University

Some Ambiguities in the Escape from the Ambiguous

This note is a comment on Sheehan's (November 1973) article. This comment is divided into two parts: The first part is based on what would ostensibly appear to be a reasonable interpretation of Sheehan's account; the second part is based on a consideration of the experimental techniques employed by both Barber (1969) and Sheehan. Predominantly, our concern is with Sheehan's critique of what he called the operational paradigm or model. His critique was essentially an analysis of Barber's (1969) experimental procedure. Sheehan maintained that Barber's model contains an artifact; we maintain that the term *artifact* was used too loosely by Sheehan. Artifact refers to a specific kind of error, and, although it was evident that Sheehan had legitimately found a fault in Barber, this fault is of a different nature.

First, artifact arises typically in the following situation: An experiment is conducted in which there is an experimental group and a control group. These groups by definition differ only with respect to the presence of the experimental factor in the experimental group. All other factors in the two groups are supposed to be the same. If experimentation demonstrates a difference on the dependent variable between the two groups, the difference is attributed to the experimental factor. If it turns out that the experimenter, in exemplifying (Campbell, 1969, p. 358) the experimental factor, unwittingly introduced some aberrant or unspecified independent variable such that the difference in the dependent variable can be shown to be produced by this ignored factor, we say that the original result was *artifactitious*. Thus, experimenter bias, or *demand characteristics*, or the *Hawthorne effect* are elements unintentionally introduced into experimental stituations and have subsequently been shown responsible for differences in dependent variables.

Heuristically, one might view Barber's recent studies (for a summary, see Barber, 1969) as purporting to expose an artifact. Barber pointed out that results that appeared to be due to hypnotic trance were artifactitious. Among the factors prerequisite to the production of a trance, one finds something called *task motivation*. In numerous experiments, Barber presumed to show that when task motivation was added to the control condition, the superiority of the hypnotized subjects disappeared.

Orne's real–simulating model discussed by Sheehan (1973) is a typical example of a situation in which artifact arises. Although both Sheehan and Orne referred to the simulators as "quasi-controls," this seems an unnecessary qualification once it is made clear that the quasi-control is really the experimental group. What makes the situation look different is that Orne wanted to show a *faking* instruction to be an inhibiting factor so that the "experimental" simulators would do "worse." The simulators were treated in every way like the hypnosis subjects except for the additional instruction to fake. The instructions to fake can be regarded as the experimental factor. Furthermore, the artifact, the so-called simulation effect (Sheehan, 1973, p. 984), was found to be an element unwittingly introduced with the instruction to fake, again fulfilling the desideratum for normal artifact situations.

The objection raised by Sheehan against Barber, however, was not of the typical kind. Task motivation may be visualized as the control condition, and standard hypnotic induction as the experimental condition (containing both task motivational and hypnosis variables). Seen in this fashion, Sheehan was not trying to show that the experimental factor contained some other unwittingly introduced variable; rather, he attempted to show that the control condition possessed something that the experimental situation lacked, namely, more social pressure or constraint. In order to grasp the importance of the distinction between the Sheehan-type artifact and the usual artifact, it is necessary to examine the fundamental purpose of the control group. When doing an experiment, why have a control group? Why not have just an experimental condition? The answer is that the purpose of the experiment is to determine whether a hypothesized relationship exists between one factor and another. It is not always possible, and frequently not even meaningful, to isolate this factor. Other factors may also be present, yet by the introduction of a control group the experimenter seeks to decrease the likelihood that these factors are influential in producing the result. Whatever factors are common to both experimental and control groups are thought to be eliminable in accounting for the differences obtained between these groups. Sheehan contended that the control group does not necessarily serve this function because the control factors may play different roles when the experimental factor is absent than they do when it is present.

Sheehan's position struck at the very heart of the use of control groups. Specifically in the case of Barber, the objection was that task motivation introduces, or at least increases, a variable of constraint when presented without the rest of the induction. If this objection is valid in the Barber case, then it is a potential objection to all studies involving experimental and control groups. Indeed Sheehan's claim was logically valid and potentially applicable to the numerous studies that employ control groups. Sheehan's objection was tantamount to saying that a given factor may have a particular effect in one context and a different effect in another context. In other words, an experimental factor may interact with a factor in the control condition, and such a possibility is certainly real.

The fact that such interactions are possible does not mean that every experiment employing a control group contains a Sheehan-type artifact. On the other hand, because so many of the variables in experiments are unspecified—situational, instructional, subject—the possibility of such an artifact in a control group looms large. Of course, the burden of uncovering such interactions lies with the one who proposes such an interaction, and the question arises of how such an interaction can be demonstrated.

To understand how an interaction may be demonstrated empirically, it is necessary to discover what factors are presumed to be interacting. In the general analysis of the control group that we are proposing, a Sheehan-type artifact assumes that an experimental factor is interacting with some factor in the control situation. But as we have already indicated, task motivation, because it is represented as lacking something that standard induction does not, is in reality the control group for the experimental condition (i.e., standard hypnotic induction). Sheehan did not seem to recognize this as the true empirical problem that he posed, but it seems clear that the politics behind his efforts were to save the non-task-motivating part of the induction from Barber's experimental onslaught.

Accordingly, the proper experiment seems to be one that will show an interaction between the non-task-motivating parts of the induction and the task-motivating portion. Such an experiment would include four conditions: (*a*) the usual "control" (i.e., no special instructions, just the suggestibility tasks), (*b*) task motivation alone, (*c*) non-task-motivating instructions alone (suggestions of drowsiness, etc.), and (*d*) instructions of both kinds, that is, the "standard hypnotic induction." This is a familiar factorial design, and the results of an analysis of variance should make clear whether a Sheehan-type artifact is present.

The actual experiment that Sheehan performed (Sheehan & Dolby, 1974) was quite different from the one outlined above. Why is this? The answer within the framework presented here is that Sheehan skipped a step. What do experimenters do when they discover an interaction? They attempt to explain it, to postulate a mechanism that would account for it. This is what Sheehan did. He assumed that the interaction existed and endeavored to explain it as acting through the variable of "social constraint." The interaction occurred *because* the non-motivating parts of the induction inhibited the constraint created by the task-motivating instructions in the control condition. This was what Sheehan sought to show.

Second, the analysis to this point is logically coherent and makes important and valid methodological points. It is based on the assumption, however, that in the Barber procedure there is some one thing, called task motivation, present in both the experimental (standard hypnotic) and the control (task-motivating) conditions. As Barber (1969, p. 71) observed, standard induction consists of (*a*) hypnotic variables (the subjects being told that they are to be hypnotized and then given suggestions to relax, become drowsy, and go to sleep) and (*b*) task motivational variables (cooperate–try statements and statements that it is easy to respond to suggestions and to experience the suggested effects). One might have conceived of the experimental situation as consisting of actual pieces of magnetic tape presented to each set of subjects. The piece of tape that contained the task-motivating instructions (*b*, above) would be played to both groups; another piece that contained the rest of

the induction procedure (*a*, above) would be played only to the experimental subjects. Neither Barber (1969) nor Sheehan (Sheehan & Dolby, 1974)[1] followed such a procedure. The task-motivating instructions are presumed to be interwoven into the standard hypnotic induction; however, there was a blatant incongruity between the wording of the instructions given to the task motivational subjects and to the standard induction subjects.

This materially alters the situation. The obvious differences in what is meant by *task motivation* in the two conditions violates a rule of methodological procedure and therefore makes it impossible to apply the analysis discussed in the first part of this comment. The rule is that except for the experimental factor, the control and experimental groups should be treated in as like a manner as possible. This was not done, and there appears to be no reason for this failure. Since Sheehan's instructions followed those of Barber (1969) verbatim, the responsibility for this gross neglect seems to be the latter's.

The burden to show comparability between the two groups should be Barber's. The claim that the differences in what we call *task motivation* in the two groups make no difference because there is no difference in the dependent variable (suggestibility scores) begs the question and is therefore clearly invalid. At the most fundamental level a vast difference exists between experimental and control groups; hence one should be fully justified in concluding with Sheehan (1973) that "task motivational subjects could be responding because of their condition and hypnotic subjects because of their condition, both involving different reasons [p. 987]." At any rate, it seems to have fallen Sheehan's lot to show empirically that the obvious differences in Barber's two groups do make a difference in suggestibility scores. Sheehan provided us with a variable of constraint in terms of which these two sets of instructions differ, but calling this operation *artifact finding* is erroneous. To call every error an artifact would be misleading.

By assuming the burden of proof, Sheehan put himeslf on the defensive. His actual empirical work seemed less convincing than a preexperimental examination of the instructions given each group. In submitting the three sets of instructions (those which comprise task motivation, standard hypnotic induction, and modified hypnotic induction) to the same set of raters to assess the dimension of constraint, he also created an occasion for the introduction of artifact. Asking the same raters to rate these similar instructions carries a demand or expectation that differences exist. A more rigorous technique would be to employ independent randomly selected groups to respond to each set of instructions rather than have one group rate all three. Also, his actual results with the three sets of instructions showed *no* difference in the dependent variable. To get significance he had to use the "change scores." There seems to be little precedent for this and, as Sheehan himself admitted, it is an ad hoc procedure. Furthermore, this "change score" confounds difficulty and order.

Finally, it should be pointed out that the concept "social constraint" is very vague. Task motivation is similarly vague. If one explores the differences between these two concepts, it is difficult to distinguish them clearly. Indeed, is there a way to define task motivation so that it does not imply some social pressure or constraint? What if subjects were asked simply to rate the sets of instructions for their degree of task motivation instead of social constraint? Would the ratings have been different? Perhaps Sheehan emphasized that the two sets of instructions are not identical in task motivational and hypnotic settings, but, as we argued, one has every reason to expect this from the obvious differences in their wording.

[1] Professor Sheehan was kind enough to send a preprint of the Sheehan and Dolby (1974) study and the instructions used in the experiments.

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Lewis R. Lieberman
John T. Dunlap
Columbus College

Psychology in Action

THE APA MINORITY CONSULTATION PROGRAM

FRED STRASSBURGER *American Psychological Association* [1]

Over the past several years, the American Psychological Association's Committee on Equality of Opportunity in Psychology (CEOP) has been interested in and concerned with the numbers of minority students entering and remaining in the field of psychology. The desire to do something concrete in this area led to the development of Project Impact. Through a program of consultations with graduate departments of psychology, this proposal was designed to help establish models for the recruitment, training, and practice of psychology among minority group members.

A memo from CEOP, summarizing ideas for a grant proposal designed to increase the representation and meaningful participation of minority group members in the profession of psychology, was submitted to the Board of Directors in May 1971. The Board approved the plan in principle and authorized continued development of a grant proposal. Project Impact was eventually put into its final form by an ad hoc committee that met on January 15–16, 1973; this group consisted of Carl Eisdorfer, James Jackson, Glendora Patterson, and Jacquelyne Jackson, the Chair of CEOP.

While Project Impact was being debated and developed, the Board of Directors at its September 1972 meeting had the idea of starting something like a miniature pilot project along similar lines within the already ongoing activities of APA. The minutes of that meeting say: "Because the Board felt that work in this area should not be delayed until outside funds are available, it directed that consultation on minority affairs should be a focus of the 1972–1973 APA consultation program with the hope that experience from early consultations can be used in strengthening the proposal." Actually, some members of CEOP were inclined to reject this pilot project on the basis that it might be seen as a substitute for Project Impact and would thereby reduce the pressure to seek funding for Project Impact. There was also a feeling that a project like this, on such a small scale, was bound to fail and that negative implications for Project Impact would be drawn from such a failure. Finally, however, CEOP decided to take advantage of this opportunity, after establishing that the two projects were really independent and that work on Project Impact was not contingent on the success or failure of the minority consultation program. The purpose of this report is to present some results of the consultation program.

A certain amount of funds allotted to the Visiting Scientist Program was set aside to initiate a Minority Consultation Program. At this point, a letter from the APA Executive Officer was sent to about 300 colleges and universities calling attention to this special focus and asking for the institutions' expression of interest in having minority affairs consultants visit their campuses. Thirteen schools did express a positive interest in the program; two schools sent letters saying they would be willing to supply consultants to such a program. Letters of interest varied considerably, from the obviously lukewarm to the enthusiastic and insightful.

It might be noted that, although only 13 of 300 responded to the offer of a minority consultant visit, some of the psychology departments responding were at very large institutions, and a few would probably be classed as belonging in the upper ranks of psychology departments; however, the so-called "top 10" were not among the respondents.

The Ad hoc Committee on the Visiting Scientist Program, at the suggestion of the Education and Training Board, decided it would be able to fund the visits (two visitors per school) to all the institutions requesting them. The members of CEOP met and drew up a list of consultants who were geographically near to the requesting institutions and who seemed appropriate to the particular needs, expressed or perceived, of the institution. Letters were sent and calls were made to the selected consultants, who expressed a surprising 90% interest in working with this project, despite the fact that they would receive no stipend for doing so, except for room and board, and transportation. After the consultants had agreed to serve, the institutions were notified and further arrangements were worked out between the institutions and the consultants.

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[1] The author would like to thank Dorothy Thomas for her help in preparing this report.

Requests for reprints should be sent to Fred Strassburger, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

At the time of this writing, eight visits had been completed and evaluative reports had been received from all eight of the institutions and ten of the consultants. A reading of these reports reveals that at least four of the institutional reports could be rated clearly as "positive," while three might be classified as "mixed"; none is clearly "negative." Of the consultants' reports, seven are clearly "positive," two could probably best be described as "mixed," while one could definitely be called "negative."

More instructive, however, than these figures, and more interesting, are some actual quotations from these reports. The following remarks were selected to point out consistencies that appeared to emerge from the reports.

In answer to the question, "Indicate what you regard as the major, special problems in dealing with minorities the host institution faces," the visitors' report of a well-known southern university stated:

> It appeared obvious that _____ has difficulties in understanding the particular needs of its minority students to combine traditional psychological training with more relevant pursuits. Second, because minority students are new to _____, communication is a definite issue. Third, the lack of minority (including women) faculty appears to be a drawback in allowing for nonacademic supportive relationships between faculty and minority students. Fourth, and perhaps most persuasive, is the traditional nature of the department. Unlike other large traditional programs that are flexible, due to their size, the smallness of the department maintains certain norms, values, etc., which appear to be threatened by the desire for certain different types of training by minority students. This is a problem shared by departments across the country and it is in no way peculiar to _____.

Other comments from this report included:

> A disadvantage from the black students' point of view is the small percentage of black students in other departments and in the undergraduate college with whom to form a mutual support base. The typical student is seen as either indifferent or unreceptive to black students. The student body is seen as primarily either ultraconservative or apathetic, politically. Greekdom is still very big there (à la 1950s in the North). All students interviewed are single, which no doubt enhances their feelings of isolation.
>
> The students, by and large, feel that the faculty views them as less than adequate students. There was doubt expressed as to whether they would in fact graduate. The stereotype under which they perceive themselves to be laboring was actually reflected in some interviews with faculty members. This stereotype had to do with black students thinking more concretely, having more than the average difficulty with writing and statistical skills.
>
> They [the students] feel that the Department is a bastion of white males with prejudice toward both females and blacks. They expressed a need for black and female faculty. The female students tend to address the faculty person by title, and the male students tend to use first names in addressing faculty members.
>
> The proximity of black colleges was pointed out to them. Their response was to the effect of feeling that they are discouraged from working with persons at other institutions.

From the same report, the following are observations about the faculty:

> The faculty extended as warm a welcome to the visiting consultants as did the students.
>
> The faculty . . . were genuinely interested in the problems of recruiting and retaining black students and were readily available to us for interviews or providing information.
>
> There were parallels and contradictions between the faculty's perceptions of the black students and the perceptions attributed to them by the students themselves. . . . The terms *deficiencies* and *remediation* arose spontaneously in every interview with faculty members. There seemed to be among the faculty almost a "set" for remediation that may be in the nature of a "self-fulfilling prophecy."
>
> A striking difference in perception was noted in treatment of black students. The students felt there were some discriminatory practices on the part of some of the faculty, although this was not cited as of major significance. In contrast, the faculty seemed to feel that black students were given above-average or "special" treatment. One faculty member described the assignments of the black students as "more plush" (allowing for more free time than the average graduate student had), to make up deficiencies.
>
> One faculty member stressed an important factor, that not enough feedback was given to the students nor was it given to the entering black student early enough. Some faculty, it was felt, were hypersensitive in their responses to black students. Interestingly, the faculty member that came across, to this observer, as being the most comfortable in discussing the black students, both their strengths and weaknesses, was the faculty member most admired by the black students.

The institution involved in this case also submitted an evaluation of the visit, of which the following comment is a brief sample:

> Not only the consultants' advice but also the internal dialogue stimulated by the conferences should be most helpful. The consultants approached the task in a very probing, frank discussion of the department's problems relevant to minority group members. It seems most likely that these conferences will have a constructive effect on future departmental actions. The consultants' role in setting the stage for this probable development cannot be overestimated.

Although positive responses such as the foregoing were more typical, it is of interest to quote from the one clearly negative visitor's report. This visit was to a small eastern college. She described the program as follows:

> The consultation consisted of an invitation to make a presentation of our own ideas to the faculty. . . . There was no opportunity to talk with other campus community members. . . . The advice offered consisted of the following . . . [here followed a list of "imperatives" offered to the faculty].

This consultant, in response to a question on whether a consultation program could be helpful in aiding the host institution resolve its special problems, replied "Consultation on a regular basis could help if with group work; however, it must be requested by the institution and still would have low probability of per-

ceivable success." Additional comments made by this consultant are as follows:

> There was poor communication of the purpose of the visit between APA, the institution, and the consultants. This must be cleared up. . . . The visit should be with smaller groups of concerned persons at a time and others beside faculty should be available. . . . At this point, I would not like to be considered as a visitor unless the program is reorganized to be more effective.

Of obvious interest here is the report written by the institution about this particular visit. Following are relevant comments:

> Due to poor communication, only one conference was held at which our staff of 36 faculty were present. Only one member of our staff is black and one third are women. . . . Advice was offered to the faculty primarily in the area of community psychology regarding power sources. . . . Although some hostility was engendered in the beginning, in the final analysis the thought-provoking ideas came through.

In response to the question, "What is your single, most important problem related to minority students?" this department's representative answered, "Motivating some black students to attend classes and keep appointments." To the question, "Could such a program aid you in the future?" he responded, "Perhaps, but I am not sure how because we have a viable college affirmative action committee that has been established this year on a college-wide basis." Regarding suggestions for improvement he commented, "Keeping lines of communication open through responding to phone calls and letters is important. Because of problems in this area we eliminated other uses to which the consultants might have been put if scheduling did not have to occur at the end of the semester."

Perhaps it should not be surprising, but it is certainly instructive to note how much overlap there seems to be in problems perceived by and around minority students at the campuses visited. One typical report, made by a visitor to a university in the Rocky Mountain region, stated:

> The university offers a very attractive program that is of potential benefit to Chicano and black students. Their emphasis on school guidance and school psychology appears very desirable to the minority students I have had the opportunity to speak to. The department has a minority affairs committee which impressed me as being conscious of its responsibilities and, as a result of this committee, has approved a curriculum change adding a number of black and Chicano culture courses which they strongly recommend to their students. . . . Nevertheless, despite attractive professional training programs, the addition of minority culture courses, and the willingness of the faculty to take some steps toward improving minority education, extremely serious problems exist with regard to minority student affairs:
>
> 1. The percentage of minority students is extremely small, despite attractive course offerings.
>
> 2. Financial support levels at the university, for students of any race or color are pitiful . . .
>
> 3. The faculty currently have no black and no Chicano on appointment as instructor or higher. This creates problems of relating among students who feel that the Anglo instructors do not adapt their teaching or understanding of human behavior to specific minority concerns when these are appropriate. The relative lack of role models on the faculty was unanimously decried by the black and Chicano minority students I spoke with.
>
> 4. The administration (from the highest authority down) is perceived by blacks and Chicanos as not committed to improvement of minority student relations. The perception here is that money exists at the higher levels for supporting minority education programs but the administration persistently blocks these attempts at releasing funds.
>
> 5. The administration's failure to meet with me in conjunction with clear evidence of large financial expenditures for construction landscaping etc. does possibly call into question their priorities for financial allocations.
>
> 6. The faculty teaching load is nothing short of back breaking. . . . A faculty member's commitment to improvement of the plight of minorities can at present only come at the expense of his or her family or other free time. . . . One last bone of contention between students and administration concerns admissions standards. Again, students complained of "institutional racism" in the department's depending on the GRE [Graduate Record Examination] scores for admission. I discussed with faculty our department's brief experience with GRE scores and minority students, in which it appears that a minimal GRE verbal score was associated with success in our program. We also discussed alternatives (e.g., tutorials, special tracks) for the minorities. We arrived at no real solution to this problem, other than an agreement that standards of high professional quality must be met in professional programs regardless of standards used to admitting students.

This consultant made some recommendations to APA on how to improve the minority consultants program. One recommendation was that APA should catalog resources available to departments eager to improve their minority student programs. APA perhaps could funnel information through the minority consultants about federal, state, and private agencies that might fund programs aimed to assist in the recruitment and retention of minority students. Another recommendation was that APA might better train its consultants to obtain important information from the university authorities in a position to support (or withhold support) from minority programs. This particular consultant favored specific action on target areas (such as financial aid for minority students) rather than attempts to change attitudes, this feeling was by no means universal in the reports.

It seems fairly evident that some programs are more able than others to benefit from a project such as this and that some types of consultants have more to offer than others. Some programs appear more able to admit shortcomings and lacks without becoming overly defensive about them. At the same time some consultants, while able to confront the issues directly and honestly, do not go to a program with their minds made up that whatever they find will be bad and that there can be no convincing evidence of good and sincere intentions.

The more receptive programs made a great deal of time available to the consultants, but did not necessarily feel the need to plan each minute that the consultants were on campus. They did see that a maximum number of students, faculty, and administrators were available to the consultants. Receptive consultants recognized the realistic difficulties in scheduling these various groups in a one- or two-day period and bent over backward to adapt their own schedules to suit. They were also more likely to want to engage in open give-and-take discussions as opposed to giving lectures, advice, or prepared formulas. It appears to me that the consultants who expressed no concern about the lack of fees are likely to do a better job; likewise, receptive departments are more likely to come up with the funds to pay for the visits.

The reports give the impression that the greatest benefit that may come from a visit is when it acts as a catalyst for a campus dialogue that continues after the consultants leave, or it reopens a dialogue that, for various reasons, had died before the consultants arrived. A closed-minded consultant is not likely to start such a dialogue, nor is one who is terribly uncomfortable with an unstructured give-and-take situation or who is too unaggressive to ask to see the relevant people on campus. A more interesting question is whether departments that ask for minority consultants, but with a clear lack of enthusiasm, can still benefit from the visits. Not surprisingly, one gets the impression that the most positively perceived visits are to departments that looked forward eagerly to them and did a good amount of advance preparation.

Nevertheless, it appears that some of the departments that had initial reluctance about the program still expressed positive feelings about the results of the visits, as did the consultants who made the visits. Indeed, in discussions within CEOP about the potential of Project Impact, it was generally acknowledged that programs which felt under some duress to "volunteer" for the program might in the long run benefit the most. Timely support to the factions within a campus that created the request in the first place could conceivably have a positive long-range impact. It is true, however, that where reluctance exists, it is incumbent upon the consultants to do as much of the advance preparation as feasible. In any case, the results of the minority consultants program, judging from the reports submitted, would indicate that there is a real need for an expanded program of this type, such as is envisaged in Project Impact.

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Pittsburgh, Pennsylvania 15260

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Czechoslovak Medical Society
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Czechoslovakia

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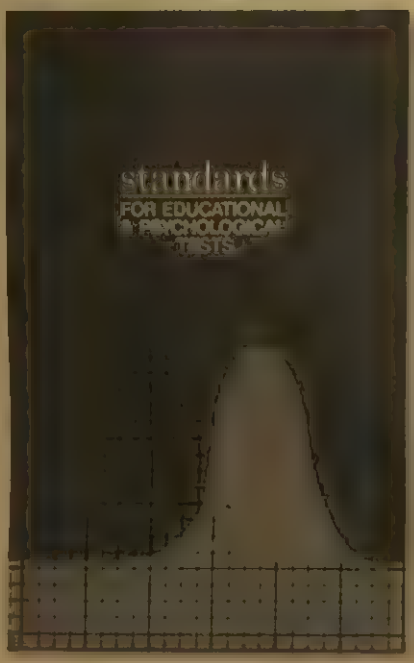

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Psychology and the Lie Detector Industry

DAVID T. LYKKEN *University of Minnesota* [1]

Polygraphic interrogation—the use of the so-called "lie detector"—is already a thriving industry in the United States, and it is growing rapidly. Several *million* polygraphic examinations are conducted annually by more than 3,000 professional polygraphers, most of whom are engaged in the private practice of their art, and some dozen schools, including one operated by the U.S. Army, are spawning graduates. This great crescendo in what for years had been a rather muted theme resulted, like most such booms, from newly discovered possibilities for profit. Estimates of losses due to employee theft in American business are running as high as $6 billion annually; thus, a rich and eager market beckons the entrepreneur who claims to have a quick and relatively inexpensive method of detecting peculators. During the 1960s, the federal government invested large sums of money in a search for a *covert* lie detector, that is, a method of measuring physiological arousal in a subject who is unaware that his autonomic reactions are being monitored. This hope has now been realized with the development of the Psychological Stress Evaluator (Dector Corporation), a device that can detect emotional modulations of the speaking voice and thus can be applied even to tape recordings of telephone conversations and other "bugs." Movements are afoot in most of the 50 states to license polygraphers, legitimating the profession by statutory fiat. Should the hopes of some of the leaders of this profession come to pass, polygraphic interrogation may soon become the most important area of applied psychology, both economically and in terms of social impact.

It is clear that polygraphic interrogation *is* an area of applied psychology because the lie detector clearly is a psychological test. If persons who administer and evaluate Stanford-Binets or Rorschachs or MMPIs are psychometrists, then the polygrapher is a psychometrist also, and one basic science that should underlie his art is the science of psychological assessment. Since the polygraph test involves the study of autonomic (rather than verbal or other operant) responses to psychological stimuli, a second basic science area which is directly relevant to polygraphy is psychophysiology. One might therefore suppose that most polygraphers are trained psychologists, or at least that most polygraphers are trained *by* psychologists, or at the very least that a sufficient number of psychologists keep in touch with developments in the polygraphy business to provide expert guidance, for example, to courts or to legislative bodies. In fact, however, the field of polygraphic interrogation is about as well integrated into psychology as is the field of chiropractic into medicine; few professional polygraphers have any significant psychological training, and nearly all have received their training in polygraphy from teachers whose own prior professional background, if any, was in law enforcement. If bills were to be introduced tomorrow simultaneously in the legislatures of the 50 states limiting, regulating, or expanding the use of the lie detector, it is doubtful that more than some half dozen of those legislatures could find within the borders of the state a certified psychologist competent to offer expert testimony on the matter. This article is motivated by the conviction that such a situation is a kind of scandal, that psychologists *ought* to know about the lie detector because its use or misuse has social consequences potentially more important than anything most psychologists *do* know about *qua* psychologists, and that the "truth about the lie detector" is a matter of considerable intrinsic interest anyway. (For background information, the reader is referred to the historical survey by Trovillo, 1939, and two representative texts by Lee, 1953, and Reid & Inbau, 1966.)

[1] This work was supported in part by U.S. Public Health Service Grant MH18856-04 and by research funds from the State of Minnesota.

Requests for reprints should be sent to David T. Lykken, Box 392, Mayo Hospitals, University of Minnesota, Minneapolis, Minnesota 55455.

Methods of Polygraphic Interrogation

There are two fundamentally different methods of polygraphic interrogation, one which is properly called *lie detection* and another which I shall refer to as the *guilty knowledge test*. Since this basic and important distinction is for some reason not generally recognized, it will be elaborated with some care at the outset. There is no great mystery about the first category; any interrogation technique that involves asking the subject one or more relevant questions ("Did you kill Cock Robin?") and then attempting to determine by some method or combination of methods whether his answer to that question is or is not deceptive can be classified as a method of lie detection. There are a number of lie detection methods that differ in the nature of the questions used, the manner in which the questions are put to the subject, the sorts of data included in the analysis, and the methods of analysis employed. It is probable that more than 90% of all polygraphic examinations given in the field employ lie detection methods exclusively. Guilty knowledge detection, as the name implies, is intended to determine whether the subject is aware of certain information; the guilty knowledge test might be used whenever the demonstration of such awareness might, for example, differentiate between a guilty and an innocent suspect.

To illustrate both of these methods in the context of criminal investigation, I shall use a notorious case that occurred some years ago in New York City. The bass player in the band at the Stork Club was arrested on the charge of armed robbery, accused of having held up a number of Manhattan liquor stores and a loan company. The prosecution's case was based entirely on the fact that each of the victims positively identified the musician as the man who had threatened them with a gun and taken their money. After the usual protracted delays, the case went to trial, but before that ordeal was finished the sudden death of the judge resulted in a mistrial. After further delay and a second trial, the musician was found guilty as charged. Just before he was to be transferred to the state prison, another man, remarkably similar to the bass player in physical appearance, was arrested for armed robbery and volunteered a confession which exonerated the musician. By this time, more than two years after his initial arrest, that unfortunate victim of circumstance had lost his job, his home, his life savings, and even his wife, who had succumbed to the strain and been committed to a state hospital.

Let us suppose that our protagonist had been given a polygraphic interrogation by the New York City police soon after his arrest. Had this happened, the method employed would almost certainly have been some form of lie detector test; that is, he would have been asked "critical" questions of the form, "Did you rob the Friendly Loan Company?" and the autonomic responses accompanying his "No" answer would have been compared with his responses to other "irrelevant" or "emotional control" questions. Based on this comparison *and* on the examiner's observation of the suspect during the examination *and* on whatever the examiner happened to know about the facts of the case, the polygrapher would have arrived at a global assessment: "deception indicated" or "deception not indicated" or "indeterminate." We shall examine these lie detection methods in more detail later, including the assumptions on which they are based and the evidence that exists concerning their validity. For the moment, let us assume that the polygrapher happened to arrive at the correct diagnosis in this instance, that the musician was telling the truth and was in fact innocent. It is possible that such a finding might have led the investigating officers to reexamine their evidence, perhaps to make a greater effort to corroborate whatever alibies the suspect might have been able to produce for the times in question. But in this particular case, with such an array of positive eyewitness identifications, it seems probable that the bass player's fate would not have been greatly altered by the lie detector test.

The Guilty Knowledge Test

Suppose, however, that the polygraphic examiner had been more sophisticated than most and had recognized that this case was one that lent itself nicely to the use of the guilty knowledge test. At the cost of some initial effort and preparation, and with the cooperation of the investigating officers, the examiner might have prepared a test along the following lines:

Item 1. The man we're looking for held up a loan office in Manhattan. If you're the guilty party, you will recognize the name of that loan company. I'm going to name a few loan companies that have offices in the vicinity; you just sit there quietly and repeat the names after me as you hear them. Was it the Ideal Loan Company? . . . Was it the Continental Loan Company? . . . Was it the Guarantee Loan Company? . . . Was it the Friendly Loan Company? . . . Was it the Fidelity Loan Company?

If the subject is in fact guilty of the robbery and if he did notice the name of the company he robbed, then it is a safe assumption that he will show a greater autonomic response to the guilty alternative ("Friendly Loan Company?") than he would if he did not possess this guilty knowledge. If we can further assume that he cannot deliberately but covertly produce an augmented response to one of the other alternatives in order to mislead the examiner and also that he has no other reason to respond selectively to one of these others, then we can expect that his response to the guilty alternative will at least be stronger than his mean response to the array of alternatives and probably that it will be strongest of all. On the other hand, if the subject is in fact innocent and if he has no indirect means of knowing or inferring which alternative is "correct," then we can safely suppose that there is only about one chance in five that his largest autonomic response will be to the guilty alternative.

Item 2. Before showing his gun, the robber pretended that he wanted to take out a loan for a certain purpose. If you're the guilty man, you will know whether that purpose was to buy a car, to pay doctor bills, to pay for a vacation trip, to buy a color TV, or to get a present for his wife. I'm going to name each of these five possibilities in order and I want you to sit quietly and just repeat what I say. Was it—a car? . . . [etc.].

If the subject had been guilty, it is very likely that he would have remembered talking to the woman at the loan office about needing money to pay his "doctor bills" and therefore that his autonomic response to this one of the five alternatives would be augmented by this guilty knowledge. On the other hand, the probability that an innocent suspect would give his largest response to this alternative is only about .2. Similarly, the probability that an innocent suspect might happen to respond most strongly to the "correct" alternative in *both* Item 1 and Item 2 would be equal to the product of the individual item probabilities, or about .04.

Item 3. If you did rob this loan company, you will recognize the woman you talked to, the one who gave you the money. I have some photographs here of women tellers in five different loan offices, just as they look to a customer. I will show these pictures to you one at a time. Just sit there quietly and look at each photograph.

The probability that an innocent suspect might chance to "hit" on all three items would be approximately $(1/5)^3 = .008$. Because subjects tend to respond more strongly to the first alternative in any list, the examiner might make a practice of using an extra "incorrect" alternative at the start of each set and then ignoring the response to that alternative. With only 10 such guilty knowledge items, each with five scorable alternatives, there would be only about 1 chance in 10 million that a subject without guilty knowledge would give his largest response to the "correct" alternatives in all 10 items. With a 10-item test, the actual culprit would only have to "hit" on 6 of the 10 items to permit us to say that the chances are less than 1 in 1,000 that he is innocent.

In the case of the misidentified musician, it would have been a relatively simple matter to construct several tests of 10 items each, taking advantage of the fact that in this situation one could be sure that the guilty suspect would possess a great deal of guilty knowledge about the details of the several crimes of which he was accused, details that would not be known to an innocent suspect but that could be determined with a little effort and competent investigation and planning prior to the examination. Moreover, of course, each set of items could be used more than once; the probability that the "correct" alternative would elicit the largest response from an innocent subject on both of two separate testings is doubtless greater than $(1/5)^2$ because that alternative might have been especially provocative for him on both occasions even without guilty knowledge, but the probability that the first test *and* the retest will *both* be false-positive is certainly smaller than .2. Therefore, after testing our unfortunate bass player, the examiner would have been able to inform the prosecution that the chances of this man's being guilty were vanishingly small and that the police should be deployed again to seek some other suspect fitting his general description.

Assumptions and Limitations of the Guilty Knowledge Test

The guilty knowledge method requires that the examiner be able to determine a number of facts that only a guilty subject will be able to recognize (these facts can be perfectly trivial matters which would not appear in newspaper accounts) and that he can present these facts in the form of multiple-choice items, embedded in a set of three or four or five alternatives that would seem equally plausible to an innocent subject without guilty knowledge. The basic assumption of the guilty knowledge test is that the guilty subject will show stronger autonomic response to what he recognizes as the significant alternative than he would have shown

without such guilty knowledge. The amplitude of the involuntary autonomic responses to the significant alternative has little meaning by itself; a hyperreactive subject might respond strongly to that alternative without knowing that it was the "correct" one, while a hyporeactive suspect might give a small response even though he does have guilty knowledge. But the same subject's responses to the other plausible but incorrect alternatives of the guilty knowledge test provide a nearly ideal control against which to evaluate his response to the significant alternative. In the language of psychophysiology, all of the guilty knowledge test alternatives can be expected to produce orienting reflexes that will vary in amplitude from subject to subject for a number of reasons, of which guilt is only one. However, for the guilty subject only, the "correct" alternative will have a special significance, an added "signal value" (Berlyne, 1960), which will tend to produce a stronger orienting reflex than that subject will show to the other alternatives. Whether he is high or low in reactivity, whether he has confidence in the test or not, whether he is frightened and aroused or calm and indifferent, we can still expect that his response to this significant alternative will be stronger than to the other alternatives as long as he recognizes which alternative is "correct." Similarly, if he has no guilty knowledge, neither his reactivity, his present emotional state, nor his confidence in the validity of the test can act to influence his guilty knowledge test score. For *all* subjects who are without guilty knowledge, the probability of the "correct" alternative producing the largest response on each of the N test items will be $(1/K)^N$, where K is the number of alternatives per item (assuming sequential independence of the items and well-chosen item alternatives).

Finally, since the guilty knowledge method does not require that the items be in the form of questions to which the subject must give answers, that is, since he can be asked merely to repeat the alternative or, even, to simply sit and listen, the interesting question arises as to whether requiring a criminal suspect to submit to a guilty knowledge test would involve a violation of his Fifth Amendment rights any more than would requiring him to show his face to a witness or to submit to fingerprinting.

On the other hand, the guilty knowledge method simply cannot be used in many situations in which the lie detector is now used, and it almost always will require much more careful preparation and preinvestigation than does a lie detector test. Moreover, even where it is appropriate, it is obvious that the construction of a good multiple-choice guilty knowledge test requires considerable ingenuity and psychological sensitivity. Where it is applicable—where the requisite guilty knowledge information is available to a competent examiner—there can be little doubt that the guilty knowledge method could be a powerful tool in criminal investigation and that its greatest benefit might be to protect innocent suspects from prolonged police harrassment and even false imprisonment.

But it must be emphasized that the guilty knowledge method is *not* in fact employed by professional polygraphers, partly because the distinctive character of the method has not yet been understood. Even the two recent reviews written by psychophysiologists (Barland & Raskin, 1973; Orne, Thackray, & Paskewitz, 1972) failed to distinguish between lie detection and guilty knowledge methods with adequate clarity. One problem here is a tendency to regard the guilty knowledge method as just a variant of something known to polygraphers as the *peak of tension test*. As it is commonly described in polygraphy literature (e.g., Reid & Inbau, 1966), the peak of tension test involves presenting a series of questions, only one of which is expected to elicit a lie from a guilty subject, after first showing the subject what the sequence of questions is to be. The test gets its name from the notion that a guilty suspect will show increasing autonomic arousal in anticipation of the critical question, a peak of tension when that question is answered deceptively, and a decline thereafter. But this is nothing more than a standard lie detector test embellished by the opportunity to look for a predictable trend or peak in tonic level of autonomic function in addition to whatever information is provided by the phasic autonomic responses to the individual questions.

As it is described by other authorities (e.g., Barland & Raskin, 1973), the peak of tension test resembles a single-item guilty knowledge test. However, there seems to be no clear awareness that it is a very different thing to use the polygraph to determine whether the subject can identify the significant alternative, than to use autonomic arousal or "tension" as evidence that the subject is lying.

With a very few exceptions (e.g., Ben Shankhar, Lieblich, & Kugelmas, 1970; Davidson, 1968; Lykken, 1959, 1960), the literature on polygraphic

interrogation deals exclusively with lie detection methods. The professional practice of polygraphic interrogation employs lie detection methods almost without exception, and it should be clearly understood that the guilty knowledge method *could not* be used in the vast majority of situations where the lie detector *is* now used—many criminal investigations and *all* employee screening applications—because of a lack of the guilty knowledge information necessary to construct the item set. Therefore, the several virtues of the guilty knowledge method—the fact that it rests on reasonable assumptions, that it can produce an objective, quantitative estimate of the probability of guilt, and that in certain situations it is capable of yielding near-perfect validity—cannot be marshaled in support of current practices.

The Lie Detector Test

The usual field polygraph makes continuous recordings of three channels of physiological data. The electrodermal channel displays changes in palmar skin resistance or galvanic skin response (GSR), usually by means of a capacity-coupled bridge circuit which results in the loss of any information about tonic resistance levels. The "cardio" channel is actually an arm plethysmograph which records changes in upper-arm volume associated with the cardiac cycle. From this channel, one can determine heart rate and some indication of changes in pulse volume. The third channel is driven pneumatically or electrically from an expansible belt around the subject's chest and records respiration. The field polygraph as manufactured for the lie detector trade is approximately 20 years behind the state of the art as represented by the modern psychophysiological laboratory. While not very important in itself because the problems with lie detection devolve from logical and psychological considerations rather than from questions of psychophysiological technique, this obsolescent character of field polygraph design is symptomatic of the fact that polygraphic interrogation has developed in virtual isolation from the original parent discipline of psychophysiology.

The Psychological Stress Evaluator, mentioned earlier, adds a fourth channel that records the presence of a low-frequency component present in normal speech sounds but said to diminish under emotional stress. Assuming that these claims can be corroborated independently, the Psychological Stress Evaluator represents an ingenious technological advance having the unique advantage of being usable without the subject's knowledge. But the questions to be raised here about the logic and validity of the lie detector are not concerned with the nature of the response being monitored and would seem to apply equally to the Psychological Stress Evaluator as to the traditional polygraph.

The standard lie detector test is normally preceded by a pretest interview in which the list of questions to be used is gone over with the subject to be sure that he understands the wording and feels he can answer them truthfully and unambiguously with a simple yes or no. Another purpose of the pretest interview is to indirectly convince the subject that the lie detector really works, that he has nothing to fear if he is innocent and truthful, but that any attempt at deception will be detected easily. After the preliminary conversation, the subject is seated in the examining room and the transducers are attached to his hand, arm, and chest. The agreed-upon list of questions is presented, usually two or three times, and the examiner may engage in some discussion with the subject between testings in order to insure that the questions are unambiguous and that, for example, the subject is not responding to a critical question for some unsuspected but irrelevant reason. Although I have not seen it recommended in published manuals, one may suppose that professional examiners frequently suggest to subjects that their attempts to deceive have in fact been detected (whether they have or have not) with the intent of precipitating a confession from a guilty subject. It is a fact that the lie detector functions effectively as a "painless third degree" and that the confessions of guilt elicited in this situation might themselves provide sufficient economic justification for its use by the police, even if the technique were wholly invalid and the polygraph merely a stage prop.

The question list used would consist of from 5 to 10 questions that can be answered yes or no. It would include one or more "critical" or "relevant" questions of the form, "Did you fire the bullet that hit Jones?" and one or more "irrelevant" questions pertaining to unrelated and unexciting matters, for example, "Are you sitting down?" Most modern polygraphers also include several control questions which are intended to serve as emotional standards. The control question should be unrelated to the matter under investigation, and it is expected that the subject will answer it truthfully; however, the

control question is chosen with the intention that it will elicit an emotional response from the subject, preferably a response involving an attitude of guilt, for example, "Can you remember ever stealing anything before you were 18 years old?" Finally, some examiners try to include a "guilt complex" question, for example, a question relating to some other real or imaginary crime of which the subject is innocent.

Methods of evaluating lie detector tests to determine whether deception is or is not indicated—that is, whether the test has been "passed" or "failed"—vary from one examiner to another. Some of the early workers maintained the concept of a specific lie response, that is, the notion that some unique pattern of autonomic response exists that is manifested by all individuals when they are deliberately lying but not when they are answering truthfully, even though fearfully. Thus, Benussi (1914) claimed that the respiratory ratio of expiration to inspiration increases after lying but decreases after a truthful answer. Marston (1938) believed that an increase in systolic blood pressure was certain evidence of lying. Summers (1939) contended that the arousal produced by the critical questions diminishes (habituates) with repetition if the subject is innocent but not if the subject is lying. All that we know about individual differences in both psychological and physiological response tendencies argues against the possibility that there is such a specific lie response, and since no acceptable evidence has been forthcoming in support of these early claims, such methods of analysis have fallen into disuse.

The modern polygrapher simply looks for evidence of autonomic disturbance associated with the answer to the critical questions, disturbance that is more intense or persistent than that associated with the irrelevant questions, and, especially, the emotional control questions. Most examiners make a global evaluation, without specific measurement or scoring. At least one of the schools now training polygraphic examiners teaches the *zone of comparison technique* (Barland & Raskin, 1973) in which a numerical score is derived from specified comparisons of responses to the several types of question. A high score results from a record in which the autonomic perturbation associated with the critical questions is much more persistent and intense than that associated with the control questions, and the examiner is advised to classify tests that give intermediate scores as "inconclusive."

A point that must be emphasized, however, is that the professional polygrapher almost never arrives at his final diagnosis on the basis of the polygraphic records alone; the examiner, rather than the polygraph, is the actual "lie detector." Laboratory studies often employ "blind reading" of charts by judges who know nothing else about the subject or the circumstances, and a similar practice is occasionally followed in the field. But in the vast majority of field examinations, the final diagnosis results from a subjective blending in the mind of the examiner of what he has observed in the charts, in the demeanor of the subject during the test, and in the preexamination interview, what he knows of the evidence against the suspect and what he may infer from the suspect's prior history, and even any prejudices he may hold about the subject's race, age, appearance, and the like. Thus, it is a fact that evidence which might be collected relating to the validity of diagnoses arrived at in this standard manner can only be interpreted as evidence relating to the performance of the particular examiner studied and is almost completely uninformative concerning the objective contribution of the polygraphic data.

Assumptions of the (Autonomic) Lie Detector Test

As we have seen, the basic assumption of the guilty knowledge test is straightforward and reasonable; given an array of stimuli, X, Y, and Z, a guilty suspect who recognizes that Z is related to the crime in question will tend to show a different autonomic response—a stronger orienting reflex—to Z than to X or Y. The assumptions underlying the lie detector test are considerably less straightforward and not nearly so easily accepted. First of all, most professionally administered lie tests are influenced to an unknown degree by the examiner's impressions of the subject and the available evidence, interacting with his evaluation of the actual polygraph record. For present purposes, we shall confine our attention to the latter, to what might be called the autonomic lie detector test, in order to evaluate the assumptions involved in a lie test scored by someone unfamiliar with the accused or with the case against him, working from the polygraphic data solely.

It is a characteristic of the lie test that the "critical question" (e.g., "Did you do it?") is obviously "relevant" to all subjects, guilty and inno-

cent alike. It will therefore tend to elicit an emotional response and the attendant autonomic reaction in the guilty and innocent alike. It is perfectly reasonable to suppose that being guilty and attempting to deceive will tend to add an increment to the autonomic response of guilty, as compared to innocent, subjects. Thus, one might readily imagine that the response to the critical question of 1,000 guilty suspects will tend, on the average, to be larger than the mean response of 1,000 innocent suspects. However, it is also clear that the magnitude of this response will not be determined solely by the fact of guilt or innocence. Other things being equal, a highly reactive or labile subject will respond more strongly to the same stimulus than will a subject whose lability is low; the critical response magnitude depends partly on individual lability, L, independently of guilt or innocence. Similarly, individuals will differ in their emotional attitude toward the same situation, in their *fear* of the consequences of being found guilty. Thus, for some people, the prospect of being tried, convicted, and punished for the crime in question may seem catastrophic, while for others, for example, those who have less to lose in such an eventuality, finding themselves in the role of a criminal suspect may be much less fear inducing. Individual differences in fear of the consequences, F, will therefore also help to determine the critical response, independently of guilt or innocence. Finally, another important determinant will be the subject's confidence in the validity of the test procedure. If he is perfectly confident that the test will yield the correct result (i.e., if $C = 1.0$), then the guilty suspect will tend to show his maximum response because he "knows" that his guilt is about to be demonstrated. An innocent subject with perfect confidence, on the other hand, will tend to show a minimum response; no matter how labile he may be or how abhorrent conviction and punishment might seem to him to be, he "knows" that he is about to be exonerated, and he responds to the critical question calmly and without great apprehension. The preexamination interview of the standard lie test is designed in part to inspire such confidence in the subject; the extent to which this works—the extent to which C approaches unity at the time the critical question is presented—will obviously also vary independently of the fact of guilt or innocence.

These three variables, extraneous to guilt or innocence, which however also interact to determine the magnitude of response to the "critical" lie test question, can be conveniently summarized in a formula. For clarity, we shall use separate formulas to represent the response of Mr. I, who is innocent and telling the truth, and the response of Mr. G, who is guilty and lying.

$$R_i = L_i F_i \left(\frac{1 - C_i}{2}\right) + M_i \qquad [1]$$

$$R_g = L_g F_g \left(\frac{1 + C_g}{2}\right) + M_g \qquad [2]$$

where R_i is the critical response of an innocent suspect, Mr. I; R_g is the critical response of a guilty suspect, Mr. G; L is the subject's autonomic reactivity or lability; F is the subject's fear of the consequences of being found guilty; C is the subject's confidence in the validity of the lie test $(-1 < C < +1)$; and M is the minimum orienting response which any stimulus might elicit in that subject. (While these formulas will be helpful in summarizing the ensuing argument, readers who tend to go faint at the sight of an equation will find that they can ignore the algebra and still be able to follow the discussion.) Note that in the two equations above, the variables L and F function multiplicatively; if either the subject's lability or his fear of the consequences happens to be zero, then his response to the critical question will be a minimum (M), irrespective of his degree of confidence in the test or of whether he is guilty. Note also that C, the measure of degree of confidence, functions differently for a guilty than for an innocent subject. If $C = 1.0$, an innocent suspect will give a minimum response ($R_i = M_i$), while a guilty suspect, under these conditions, will give a *maximum* response ($R_g = L_g F_g + M_g$). That is, Formulas 1 and 2 merely summarize the commonsense ideas already advanced and contain no appreciable "surplus meaning" deriving from the algebra.

Since a subject's response to the critical question in the lie test can be affected by (is a function of) several factors which will vary from subject to subject independently of who is guilty and who is not, it is clearly not possible to say with any confidence that a critical response larger than some specified magnitude is certain evidence of guilt. A truthful, innocent suspect who happens to be unusually labile and who feels that being prosecuted for the crime in question might ruin his career could obviously give a stronger emotional and autonomic response to the question, "Are you guilty?" than a psychopathic, guilty suspect whose lability is low and who does not really care whether he "fails"

the test or not. The absolute magnitude of the critical response could provide a basis for a high-validity lie test *only* if one could be sure that *all* subjects had nearly perfect confidence ($C = 1.0$) that the test *was* valid (because, for example, truthful subjects would then all show just the minimum response). On the other hand, when confidence is zero—that is, when subjects believe that their passing or failing will be determined as by flipping a coin—then the formulas agree with common sense in indicating that the actual validity of the test will then *be* zero.

Assuming that the best one could hope for would be that *most* subjects could be led to have at least *some* confidence in the lie test, the remaining problem is to find a way to control for individual differences in the variables L and F which will act to influence the critical response independently of guilt or innocence. It will be recalled that, in the guilty knowledge test, the response to the "correct" alternative will also be influenced by these same variables, autonomic lability and fear of the consequences of "failing" the test. But these same variables also affect the responses to the "incorrect" alternatives which therefore provide the necessary control; by subtracting the mean response to the "incorrect" alternatives from the "correct" response, one can estimate the increment added by the guilty subject's recognition of the "correct" alternative. In other words, in the case of the guilty knowledge test, the responses to the "incorrect" alternatives provide an excellent estimate of the response which *that subject* would be expected to make to the "correct" alternative *if he is in fact innocent*.

How might one devise an equally acceptable control question for the lie detector situation, a question that could be expected to elicit a response which is a good estimate of the response which *that subject* could be expected to make to the "critical" question *if he is in fact innocent?* An ideal situation could be imagined along the following lines. Suppose that our subject is suspected of being guilty of crime X. As far as he knows, he might equally well be suspected of crime Y, which carries penalties of the same degree of severity as crime X. However, unknown to the subject, we happen to have certain evidence that he is *not* guilty of crime Y. Under these rather special circumstances, the subject's response to the control question, "Did you commit crime Y?" provides us with a good estimate of how that subject might be expected to respond to the critical question, "Did you commit crime X?" if he is innocent of crime X. In terms of the foregoing formulas, we can express the difference between these two responses as follows:

Expected "lie score" if innocent

$$= R_x - R_y = \left[LF\left(\frac{1-C}{2}\right) + M\right] - \left[LF\left(\frac{1-C}{2}\right) + M\right] = 0. \quad [3]$$

Expected "lie score" if guilty

$$= \left[LF\left(\frac{1+C}{2}\right) + M\right] - \left[LF\left(\frac{1-C}{2}\right) + M\right] = CLF. \quad [4]$$

That is, the difference in magnitude of the critical and control responses would be expected to average about zero for an innocent subject, but for a subject who is guilty this difference would be expected to vary directly with the product of L, F, and C, that is, with his autonomic lability, his fear of being prosecuted, and his confidence in the validity of the lie test. As long as none of these individual difference variables is zero, the guilty might be expected to be well differentiated from the innocent with this form of lie test.

But the lie detector test actually employed in the field does not use the sort of control question we have been supposing here. Instead, the critical response is compared with that subject's response to the so-called "emotional control" question, of the form, "Did you ever steal anything before you were 18 years old?" The subject is expected to reply truthfully to this question, and all that is required of the question is that it should produce for this subject some sort of emotional response. But clearly this response is not an adequate "control" at all. In the terms of our formulas, although the autonomic lability term L should apply equally to the "control" as to the critical response, it would obviously be nonsensical to imagine that the fear variable F could be equilibrated for the two questions. How could one undertake to ask Jones a question, to which he will answer truthfully, that would be neither more nor less fearsome or disturbing to Jones than will be the critical question, "Did you rape Miss Fisbee?" so that his response to the former question will serve as a reasonable estimate of how he would respond to the latter *if innocent?* It is equally difficult to imagine how one might design such an "emotional control" question in response to which the confidence variable C would play the same role that it plays in the critical re-

sponse. In short, the standard lie test in which the "lie score" is a function of the difference between the responses to the critical and "emotional control" questions can only be said to provide some degree of control for individual differences in autonomic lability; the "lie score" will continue to vary, not only with respect to differences among subjects in their confidence in the procedure, but also with individual differences in the relative emotionality or fearfulness of the critical and control questions.

As was indicated in the preceding section, some professional polygraphers do make use of what is called a "guilt complex" question as an additional control. The rationale for this procedure in the polygraphy literature is somewhat ambiguous or vague, but it does appear that at least some field polygraphers do sometimes try to formulate a question relating to a real or imaginary crime of which the subject is known to be innocent. On its face, the guilt complex question obviously comes closer to the sort of control recommended earlier for the lie detector test, but there are several reasons for being dubious about this. First, the guilt complex question always seems to be used together with emotional control questions as discussed above and treated in the scoring procedure as if the two were equivalent, a practice that would not seem to inspire confidence. Second, one does not find in the literature any discussion of the rather formidable problems that must be dealt with if the guilt complex question is to serve its control function, that is, to produce a response that is a reasonable estimate of what the critical response should be if the subject is innocent and answering truthfully. It will be recalled from my earlier example that the control question, concerning crime Y, will provide an adequate control only (*a*) if the subject regards crimes X and Y as similar and prosecution for either as equally serious, and (*b*) if the subject believes that he is equally and independently suspected of both crimes and that he is therefore truly in jeopardy of being prosecuted for Y even though innocent of that crime. Now, it is possible to imagine a field situation in which such a control might be established, but it is abundantly clear that this would *not* be possible in most situations in which the lie detector is used (the emotional control question is always feasible but has the important defect that it cannot be expected to work very well). Moreover, if it were to become known that the standard lie detector examination had the crime X versus crime Y format, criminal suspects could be expected quickly to identify which question is "critical" and which is the "control," and the effectiveness of the procedure will be forfeited as soon as that happens.

To summarize this section, we have seen that the autonomic response to the critical question will always be influenced by individual difference variables which are not a function of the subject's guilt or innocence. Therefore, if it is to have any hope of high validity, the lie detector test must provide a means of comparing the actual critical response to some reasonable estimate of what that subject's critical response would be if he were innocent. Such a control is an integral part of the guilty knowledge test, but that test can only be used in a limited proportion of criminal cases and is apparently never used by field polygraphers. We have seen that the emotional control question commonly used in the field does not provide the reasonable estimate required. Under very special circumstances, in which the subject can be persuaded that he is also and independently suspected of a second crime of which he is actually innocent, then his response to a question about this crime might provide the needed estimate. However, this specific method is seldom if ever used by field polygraphers, it could be used properly only in a limited proportion of cases, and it would quickly become invalidated if it were to come into standard use.

The purpose of the foregoing analysis has been to provide a basis for estimating the prior probability of the claim that conventional lie detection methods have extremely high validities, for example, 92% (Bersh, 1969), 99% (Arther, 1965), or 100% (Kubis, 1950). There can be little doubt that professional polygraphy has a validity significantly, even substantially, better than chance, but for reasons that will become apparent later, the important question about the lie detector is not whether its diagnoses are more valid than could be achieved by flipping coins but rather whether they are very nearly infallible, as the professionals claim. If the decision to amputate a leg is to depend entirely on the result of a test for osteosarcoma and if only 10% of those tested actually have the disease, then the difference between a test that is 80% valid and one that is 92% or 99% valid may be all the difference in the world. The true validity of the lie detector is, of course, an empirical question, but it would be naive to suppose that such empirical questions are easily answered. If the a priori likelihood were high that the validity of the

lie detector was .90 or better, then one or two studies yielding such an estimate empirically might be considered adequate even though each study might be found to be defective in some way. What I have attempted to demonstrate is that the prior probability of such high validities for lie detection methods is in fact vanishingly small, and therefore, as in the case of ESP, for example, we are entitled to require unusually strong, clear, well-replicated evidence before accepting the fact in the face of the theory. As we shall discover in the next section, such evidence is wholly lacking.

Empirical Evidence of Lie Detector Validity

Estimates given by professional polygraphers on the basis of their own experience are essentially worthless. This dogmatic pronouncement is not intended to disparage the integrity of these professionals, most of whom in my experience are honest and able people. But when one considers what would be required to support an accurate estimate of this sort, one can see at once that these conditions are simply never met in practice. One of my acquaintances was trained by Leonard Keeler himself and has given thousands of polygraph examinations in a police setting over a span of more than 30 years. His "conservative" estimate of the cumulative validity of the lie test in his hands is 95%. But how could he know which of those thousands of tests produced correct results and which did not? In many instances, a court may eventually adjudicate guilt, but a courtroom verdict is hardly an infallible criterion, and, in any event, final legal disposition of such cases will typically occur months after the suspect has moved beyond the ken of the polygrapher. I know of no professional who even pretends to have kept up the elaborate bookkeeping system that would be required to follow up on all subjects interrogated. In a high proportion of cases, there will be no final disposition made at all; the vast majority of employee-screening interrogations are of this nature.

In a significant proportion of police interrogations, the guilty suspect will confess. When the confession is made after the lie test has been definitely evaluated (but not so long after that the polygrapher never hears about it), then one might generate an adequate estimate of validity for this very limited subset of cases, but subjects who will later confess are hardly representative of subjects in general, especially those who have *nothing to confess*. Often, the confession will occur during the lie test or even in anticipation of it. Clearly, since no diagnosis has been made, none can be tested for validity in such instances, and yet one may be permitted to suspect that each such experience adds a bit to the examiner's subjective certainty that he is dealing with a useful and "valid" technique. One highly experienced polygrapher testified before the 1973 session of the Minnesota State Legislature that he had conducted more than 20,000 polygraphic interrogations and that he had "never once been proven to have made a mistake"; such testimonials should not be taken very seriously.

Laboratory studies cannot provide adequate validity estimates. This may be the only value judgment in the present article with which the majority of professional polygraphers would agree. Laboratory experiments involving mock crimes or other contrivances of even less ecological validity have commonly achieved a hit rate of from 70% to 85%, usually against a chance expectancy of from 20% to 50%. Professional examiners dismiss such studies as irrelevant parlor games and insist, reasonably, that in a police investigation, with real crimes and real punishments, the results might be altogether different and the hit rates much higher. Since this coin has two sides, one should also point out that moving from the laboratory to the field situation might also serve to *lower* hit rates. While a guilty suspect may indeed be more apprehensive and hence more reactive when interrogated in the jail than will a mock-guilty college sophomore in the laboratory, it is also true that the innocent suspect will be more reactive in the real-life situation and, thus, more likely to become a false-positive.

Adequate criteria against which to measure lie test validity are next to impossible to obtain in the field. Although slightly less dogmatic than the previous two, this proposition is stated strongly enough to emphasize that, while field investigations are the only trustworthy source of estimates of lie detector validity, it is exceedingly difficult in the field to establish an adequate criterion of whether the lie test diagnosis was in fact correct. For this reason, only a single field study has so far been published in which this criterion problem seems to have been handled well enough for the data to be taken seriously. Bersh (1969) obtained records on a fairly large sample of criminal investigations conducted by the military in which a standard lie detector examination had been given to the (serviceman) suspect. Each complete case file (minus

only the polygraph findings) was evaluated independently by four attorneys from the office of the Judge Advocate General. These attorneys were instructed to "disregard all legal technicalities and to judge each case solely on the evidence . . . ." Each judge was asked to eliminate cases in which he felt the evidence was insufficient and then to arrive at a positive determination of guilt or innocence on the remainder.

Using the unanimous verdict of the four-judge panel as a criterion, it was found that the polygraphers' diagnosis agreed with the criterion on 92.4% of 157 cases. On 59 additional cases for which only three of the four judges were in agreement, the hit rate was lower, 74.6%, possibly because the majority-agreement criterion was less valid than the unanimous-agreement criterion. As Bersh (1969) pointed out, polygraphic examiners in the military are undoubtedly better and more uniformly trained on the average than polygraphers generally, so that these findings probably represent the state of the art as favorably as possible.

This study is an important contribution to the literature in this field but, as Bersh himself indicated, the findings must be interpreted with caution. First of all, the 157 cases on which the polygraphers achieved the hit rate of 92.4% were a highly selected sample and, presumably, not representative even of the general run of polygraphic investigations done in a military setting. We know that another 59 cases produced a 3:1 vote of the panel (and only a 75% hit rate against a chance expectancy of about 50%), but we are not told in how many cases the judges split 2:2 nor, especially, how many of the original cases were discarded because one or more judges found the file data to be inadequate to yield a positive judgment. Most important, however, is the fact that *none* of these findings tell us anything definite about the validity of the polygraphic lie test itself because, as a matter of routine, the examiners had complete access to the case file—the evidence against the suspect—as well as any information that they could descry in the pretest interview, the subject's appearance and demeanor and the like, prior to the examination, and there is no way of determining the relative weights that were subjectively allotted to this information, as opposed to the polygraph records themselves, as the examiner arrived at his diagnosis. Since the validity criterion was a judgment based on the file records, a jaundiced appraisal of Bersh's findings might be that he has shown merely that when four judges agree that the evidence indicates guilt or innocence, a fifth judge is very likely also to agree on the basis of the same data. Such an appraisal is probably unduly harsh: Bersh pointed out that the files were often less complete at the time of the lie test than when evaluated by the attorneys. Still, however, it has to be admitted that one cannot say with certainty that the polygraph charts contributed anything at all to the accuracy of the original diagnoses.

Summarizing this examination of the evidence for the validity of the lie detector, after eliminating from consideration the testimonials of experienced polygraphers and after eliminating also the results of laboratory studies which, while obviously useful in extending our understanding of these phenomena, cannot be safely used to estimate the validity of field testing, we find that we are left with exactly *one* good field study. And that one study, while it indicates that Army-trained examiners can apparently do very well indeed in determining the guilt or innocence of at least a subset of criminal suspects, unfortunately sheds no clear light on the validity of the autonomic-response portion of the lie detector test. It was contended in the previous section that the prior probability of the *autonomic* lie detector as having .90+ validity was so low that one might reasonably insist on especially clear, well-replicated evidence before accepting such a proposition. We see now that no such evidence exists.

Before continuing, however, it would be only fair to emphasize the important positive conclusion that can be drawn from Bersh's data concerning the validity of the *human* lie detector. In a criminal investigation situation, where the evidence against a suspect is suggestive but not yet overwhelming, and where that suspect still maintains his innocence, a well-trained polygraphic interrogator can in about one hour's time arrive at a diagnosis of guilt or innocence that is likely to be correct 9 times out of 10. It would be interesting to know whether that hit rate would be greatly reduced if the polygraph pens were to be activated by random-noise generators rather than by transducers connected to the subject. It would also be interesting to compare the polygrapher's hit rate against the validity of judgments made by an experienced policeman who had interviewed the suspect at about the same time, without benefit of the polygraphic props and ritual. But, in any case, it should be apparent that a method which can achieve such a degree of accuracy at so little cost could be of great benefit

both in increasing the efficiency of investigation and in protecting the innocent. When the polygrapher says "Guilty," all efforts can be concentrated on finding the physical evidence that will prove the suspect's guilt; when the polygrapher says "Innocent," those same efforts can be redeployed in another direction. As long as the polygrapher's diagnosis is regarded as tentative and advisory rather than as conclusive and the investigating officers remember that the polygrapher will be wrong perhaps 10% of the time, the use of this (human) lie detector can be of real social benefit.

The Lie Detector in Employment Screening

We turn now to a consideration of an area in which the guilty knowledge method *cannot* be used but where the use of the lie detector has grown so rapidly in recent years as to threaten to become a major industry—and a major influence on our society. This is the area of employee screening: preemployment screening of job applicants to determine who should not be hired and routine post-employment screening to find out who has been pilfering from the company during the preceding year. Just as the guilty knowledge method is fundamentally different from the lie detector, so too is the use of the lie detector in employee screening different in certain basic ways from the use of that same technique in criminal investigation by the police.

For purposes of comparison, consider the police investigation situation first. A crime has been committed and one or more likely suspects are to be examined. In the police situation, it might be reasonable for us to assume that the base rates for lying may be fairly high, perhaps approaching 50%, because the police will not bother to examine anyone who is not a "likely" suspect. In what follows, I am also going to make the unproven assumption that the overall validity of the conventional lie detector method is as high as 90%. Now if, over many police examinations of the type we are considering, 50% of the suspects do in fact try to lie, and if the lie detector is 90% valid, then we can expect to identify 45 out of every 50 liars, while only 5 of every 50 truth tellers will be incorrectly classified as liars, that is, will be false-positives. Such a result would obviously be very useful to the police and it could be achieved at relatively minor cost, assuming that lie detector results are never tolerated as evidence in court, because the only harm done to the five false-positives will be that they will continue for a time to be the subject of intensive police interest and investigation. If we can assume textbook police work, with no beatings and no manufacturing of evidence, then we can suppose that "intensive police interest" will eventually flag after it has been unable to produce admissible physical evidence that these suspects are in fact guilty.

The first basic difference between police lie detection and the employment screening situation has to do with the question of the consequences of being a false-positive. The great impetus behind the growing use of the lie detector with employees is the enormous annual losses attributed to employee theft, so there is an understandable attraction in a relatively quick and cheap procedure which claims to be able to weed out potential thieves, drug addicts, and the like, before they are hired or to identify light-fingered employees in the annual screening. Since the main point of using the lie detector in this application is to save money, to save both the losses that would occur with no screening or the considerable costs of conventional private detective work or individual background investigations, one can see that these savings will be realized only if the lie detector is permitted to make the final decisions; if the examinee fails the lie test then he is not hired. Although one cannot send a man to prison for flunking a lie detector test, there are no constitutional guarantees which prevent him from being deprived of a job for the same reason.

Let me illustrate this particular problem with a recent real-life occurrence. An advertising agency in Minneapolis kept a $6,000 movie camera in a locked cabinet. The camera was stolen without forcible entry and a local private detective agency urged that the four employees who had keys to this cabinet should be asked to take a polygraph test. The employees, who felt of course that they could hardly claim to be innocent and yet refuse to take the test, agreed to these plans; one of them clearly "failed" the lie test. This apparent culprit happened to be a young, black account executive who was doing so well with the company that they were reluctant to accept this verdict. The young man was sent back for a second test by a different examiner; again he "failed." A third test was conducted by the most experienced of the polygraphers and this one too resulted in a verdict of "probable deception." Just at the moment when the company

president had regretfully determined to fire this apparent thief, a fortuitous set of circumstances led a *fifth* individual, who had never taken the lie test, to confess that he had stolen the camera, completely exonerating the other suspect. Except for this lucky accident, that first young man would not now be holding a well-paying job, making good use of his considerable talents, but instead he would be out on the street, saddled with a history of having been fired from his previous employment for having failed a lie detector test, and with negligible hope of ever again finding work in his chosen profession.

But I want to emphasize a second respect in which the employee-screening situation differs fundamentally from that of criminal investigation. If 1,000 employees are given a routine annual polygraph examination and are asked whether they have stolen anything from the company during the preceding year, it would clearly be unreasonable to expect that as many as half of them will in fact lie. Nobody knows what the base rates for lying are in such a situation but I should think that 5% would be a reasonable figure. Now let us continue to assume that the lie detector can be as good as 90% valid on the average, even in this application. Does that then mean that we can expect to detect 90% of the liars and also that only 1 in 10 of those who "fail" the test are actually telling the truth? *It doesn't mean anything of the kind, not with these base rates.*

In what follows, I shall make use of the analysis presented in a classic paper by Meehl and Rosen (1955) which illuminates the relationship between the base rates of a condition (such as lying) in a population of persons to be tested, with the psychometric efficiency of the test that will be used to detect that condition (such as a lie detector). If the lie detector could produce an objective, numerical score, like a standardized aptitude or personality scale, then we might establish by preliminary research an optimum cutting score such that, if we called everyone scoring above the cut "liars," we would maximize the overall hit rate. Under such conditions, the best we could expect to do would be to identify 45 of the 50 liars in our group of 1,000 examinees. But, at the same time, 95 of the innocent truth tellers would also be expected to "fail," which means that $(95)/(45 + 95) = 68\%$ of the people who "fail" the test will actually be innocent!

However, even this rather forlorn result is better than we can actually hope to achieve in practice because in fact the lie detector does *not* yield an objective, numerical score. As in the Rorschach or the TAT, the examiner's brain is an integral part of the machinery of the test, and it would obviously be very difficult to train an examiner to set his subjective cutting score in such a way as to produce an optimum validity for a given base-rate situation. For the case under consideration, if he set his cutting score too low, "failing" 40% of the cases, for example, then the *best* he could do would be to "fail" all of the liars but at the cost of "failing" more than one third of the truth tellers, in which case 88% of those identified as "liars" would in fact be innocent. To properly train operators for this situation we would need the same nearly unobtainable facilities that we would need to make meaningful estimates of the actual validity of the lie detector in the employment screening situation, namely, an independent criterion which would tell us which few subjects were in fact lying. Since this is almost certainly impossible to achieve because validity estimates obtained from a very different situation with very different base rates are a most uncertain guide when one is working in this new situation, and since whatever validity one *can* achieve here will be at the cost of an extremely high proportion of false-positives—innocent subjects who "fail" the test and therefore lose their jobs or suffer other unwarranted consequences as a result—it seems plain to me that the use of the lie detector in this lucrative but highly dubious application should not be condoned.

Any professional polygrapher who may read these words will be gnashing his teeth at this point because these statistical arguments will seem to conflict sharply with his actual experience in this lie detector application. He will be thinking of all those shame-faced individuals who have been led to confess assorted peculations under the pressure of the lie detector examination. This undeniable ability of the polygraph to function as a sort of "painless third degree" was mentioned briefly earlier. If it were possible to make use only of these elicited confessions, discarding from further consideration all tests which did not produce such a result, then these questions of validity would be dealt with very differently. But, of course, once the population to be tested gets wind of the fact that no one is going to "fail" unless he confesses,

there are not likely to be any more valid confessions. A reasonable proportion of subjects will *have* to be "failed" on the basis of the test alone, and, under the circumstances I have been assuming, we must expect that most of those who "fail" in this way will actually be innocent.

The conclusion from all of this seems to me to be obvious and compelling. It is most unlikely that we shall ever have an objective lie detector procedure that can routinely claim as high as 90% validity; there is no reason whatever to imagine that any known lie detector, human or polygraphic, can achieve validities close to 100%. Lacking near-perfect validity, both of the previous considerations militate against the use of the lie detector *at all* in employment screening, that is, both the fact that decisions here tend to be based on the lie test alone and the fact that, with low base rates for lying, the majority of those who fail the test are going to be innocent. For this reason, I testified before the 1973 session of the Minnesota Legislature in support of a bill (Senate File 612) that specifically prohibits any employer from requiring or even requesting any employee or prospective employee to take a polygraph examination. The single exception authorized by this act is the use of the lie detector in the investigation by a police chief of possible malfeasance on the part of a police officer. I am happy to report that this bill was passed and is now the law in Minnesota.

Conclusion

The polygraphic interrogation industry is rapidly expanding and is already having considerable social impact. Polygraphic interrogation is a form of psychological testing; psychometric theory and psychophysiology are—or should be—its basic sciences. In fact, however, only a handful of psychologists know enough about the lie detection business to evaluate its claims and to provide expert testimony to courts and legislative bodies.

A polygraphic method known as the Guilty Knowledge Technique appears to have the potential for very high validity in the restricted number of criminal investigations where it is applicable. But the GKT seems to be unknown to professional polygraphers and there have been no studies either of its range of applicability or of its validity in field situations. It is certain that the GKT could not be usefully adapted to employee screening, the application which is currently providing the strong economic impetus to growth of the polygraphy industry.

The techniques used by professional polygraphers can be appropriately characterized as methods of lie detection. Analysis of the assumptions underlying lie detection indicates that, while these methods might be expected to have significant validity (better, indeed, than many other commonly used psychological tests), the a priori probability is exceedingly small that the lie detector could be nearly as dependable as the professionals claim. Since these claims of 95%, 98%, and even 100% validity are so implausible, they should be taken seriously only if accompanied by unusually clear, well-replicated empirical evidence. Such evidence is wholly lacking.

In the preemployment screening situation, where the base rate for lying can be expected to be low, a high proportion—probably the majority—of those who "fail" the lie detector test will be false-positives. For example, if 5% of those tested are liars, then even with a test having 90% validity, fully 68% of those who "fail" will be innocent truth tellers. For this reason, and because in the employee screening application the lie test typically determines disposition of the case—if you fail the lie test, you do not get the job—widespread use of these methods in the private sector must inevitably work an unjust hardship on many innocent but autonomically reactive individuals. In certain sensitive occupations such as police work, where social considerations require attaching greater importance to false-negative than to false-positive predictions, the use of the lie detector as a selection device can be justified. The general use of the lie detector in employee screening cannot be justified, however, and psychologists have a professional responsibility to oppose this growing practice.

The use of polygraphic interrogation in criminal investigation is fundamentally different. Here the base rate for lying is undoubtedly higher, and, most important, the penalty imposed on the unlucky false-positive is less severe: One cannot be sent to prison for "failing" the lie test. Judicious use of the polygraph in the criminal investigation context not only can improve the efficiency of police work but could also serve as a bulwark to protect the innocent from false prosecution. It is argued that the apparent potential of the Guilty Knowledge Method should begin to be exploited in this application.

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Scientific Awards Announcement

The Committee on Scientific Awards is accepting nominations for its award program. The Committee selects up to three persons as recipients of the **Distinguished Scientific Contribution Award** who, in its opinion, have made the most distinguished theoretical or empirical contributions to scientific psychology in recent years.

A new award, the **Distinguished Contribution for Applications in Psychology,** has been authorized by the Board of Directors and will be given for the third time this year. This is presented to an individual who, in the Committee's opinion, has engaged in a program of research which is systematic and applied in character.

The awards are subject to the following limitations: (*a*) members of the Committee, former recipients of the awards, the President and the President-elect of the APA shall be ineligible. (*b*) The Committee shall seek diversity in selecting recipients, avoiding as far as possible the selection of more than one person representing a specialized topic, a specific material, a given method, or a particular application.

Names and appropriate information which will guide the Committee on Scientific Awards in conducting an intensive career review and evaluation should be forwarded to Office of Scientific Affairs, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. *Deadline for nominations is January 15, 1975.*

Learning Theory and Intelligence

W. K. ESTES *Rockefeller University* [1]

All scientific measurements of intelligence that we have at present are measures of some product produced by the person or animal in question, or of the way in which some product is produced. A is rated as more intelligent than B because he produces a better product, essay written, answer found, choice made, completion supplied or the like, or produces an equally good product in a better way, more quickly, or by inference rather than by rote memory, or by more ingenious use of the material at hand. . . .

Psychologists would of course assume that differences in intelligence are due to differences histological or physiological, or both, and would expect these physical bases of intelligence to be measurable. . . . Even if one aimed at discovering the physiological basis of intellect and measuring it in physiological units, one would have to begin by measuring the intellectual products produced by it. For our only means of discovering physiological bases is search for the physiological factors which correspond to intellectual production [Thorndike, 1926, pp. 11–12].

Such was the status of this problem when Thorndike was writing *Measurement of Intelligence* nearly 50 years ago.

Thorndike was well aware that in measuring intelligence we do go further than sampling an individual's current performance in order to predict his potentialities for various types of tasks. To the extent that we can sample performance for different individuals following equal opportunities to learn, we also infer relative rates of gain in performance in the future. In fact,

an obvious hypothesis, often advanced, is that intellect is the ability to learn, and that our estimates of it are or should be estimates of ability to learn. To be able to learn harder things or to be able to learn the same things more quickly would then be the single basis of evaluation. . . . If greater ability to learn means in part ability to learn harder things, we have excluded the vague general valuation of certain products and ways of producing only to include it again. . . . If greater ability to learn means only the ability to learn more things or to learn the same things more quickly, we have a view that has certain advantages of clearness and approximate fitness to many facts. Even less than in the case of truth-getting [insight into reality], however, do our present actual instruments for measuring intelligence measure directly a person's ability to learn more things than another person can, or to learn the same things more quickly. . . . Much evidence will therefore be required before we can wisely replace our present multifarious empirical valuations by the formula that intellect is the ability to learn more things or to learn the same things more quickly [Thorndike, 1926, pp. 17–18].

It seems to me that the preceding paragraphs could almost as well have been written yesterday as several decades ago. Little has changed with respect either to the basic method of measuring intelligence by the sampling of performance or to our inability to improve on this procedure by more direct measurement of learning abilities.

One reason why the great amounts of effort expended on problems of intelligence and its measurement in the interim have produced so little change in basic conceptions may have to do with the fact that from the time of Binet, the primary criterion for measuring intellect has been success in predicting performance in school and other situations requiring intellectual effort. Long ago, efforts to increase the predictive power of intelligence tests began to run into sharply diminishing returns, and it now seems quite possible that we are near the maximum attainable, given the usual limitations on time and expense of testing.

Thus, in raising again the possibility of replacing the characterization of intelligence in terms of performance with a characterization in terms of learning processes, I am not motivated by any expectation that appreciable gains in predictive power could be achieved. One of the motivations for a reappraisal is almost purely intellectual, and, again, I find that I can improve but little on Thorndike:

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[1] The substance of this article was included in an invited address presented at the meeting of the Toronto Psychological Association, February 1972, and at a symposium on intelligence held at the University of California, Los Angeles, June 1971.

Requests for reprints should be sent to William K. Estes, Rockefeller University, New York, New York 10021.

> We have learned to think of intellect as the ability to succeed with intellectual tasks, and to measure it by making an inventory of a fair sampling from these tasks, arranging these in levels of intellectual difficulty, and observing how many the intellect in question succeeds with at each level. . . . Such a definition in terms of tasks accomplishable, and such a measurement in terms of the contents of a graded inventory is sound and useful, but is not entirely satisfying. One cherishes the hope that some simpler, more unitary fact exists as the cause of intellect and that variations in the magnitude of this fact may provide a single fundamental scale which will account for levels and range and surface. Moreover, one realizes the desirability of search for the physiological cause of intellect, regardless of whether that cause be single and simple or manifold and complex [Thorndike, 1926, p. 412].

Thorndike went on to offer a specific interpretation of intelligence in terms of the learning theory of his day:

> The standard orthodox view of the surface nature of intellect has been that it is divided rather sharply into a lower half, mere connection-forming or the association of ideas, which acquires information and specialized habits of thinking; and a higher half characterized by abstraction, generalization, the perception and use of relations and the selection and control of habits in inference or reasoning, and ability to manage novel or original tasks. . . . The hypothesis which we present and shall defend admits the distinction in respect of surface behavior, but asserts that in their deeper nature the higher forms of intellectual operation are identical with mere association or connection forming, depending upon the same sort of physiological connections but requiring *many more of them*. By the same argument the person whose intellect is greater or higher or better than that of another person differs from him in the last analysis in having, not a new sort of physiological process, but simply a larger number of connections of the ordinary sort [Thorndike, 1926, pp. 414–415].

According to this conception, an individual's effective intelligence at any time was thought to be determined jointly by his original intellectual capacity, identified with the number of potential connections available in his brain, and by the number of connections actually formed as a result of training and experience.

Thorndike claimed "almost crucial" evidence for his hypothesis in a collection of correlational data from tests he believed to measure the higher abilities of relational thinking and abstraction (sentence completion, arithmetical problems, analogies) and tests believed to measure lower, "associative" abilities (vocabulary, numerical calculation, information tests). From the finding that intercorrelations of the higher tests among themselves, of the lower tests among themselves, and of the higher tests with the lower tests were all of the same order of magnitude, Thorndike concluded that both the associative and the higher abilities must have "in the main the same cause [p. 470]." And the common cause must lie at the associative level because the lower, associative abilities can be found without the higher (as in the mentally deficient), but the higher abilities are never manifest in the absence of the associative abilities.

Thorndike's conclusion rests on an assumption that we shall find in the sequel to be open to serious question, namely, that one can find subtests of intelligence scales which are relatively pure measures of either the associative or the higher abilities. Nonetheless, the number-of-connections hypothesis has continued to represent for several generations of psychologists and educators the way in which learning theorists would interpret intelligence.

Because of its conceptual meagerness, the hypothesis has never aroused among psychologists much enthusiasm for following up this line of inquiry. Consequently, it remains essentially without a competitor—never a healthy state of affairs in any scientific discipline. It seems most timely, therefore, to ask what form would be taken by a similar effort today, utilizing the richer body of theory that has grown out of several decades of research on learning.

Beyond utilizing conceptual resources that were not available to earlier investigators, I propose to take a somewhat different basic orientation. Thorndike's approach was to look at the learning theory of his day in order to see what concepts or ideas embodied in it might serve to account for differences in intelligence. Like many other investigators in the psychometric tradition, Thorndike seems to have viewed intelligence as a trait or characteristic of the individual that sets the limits on his level of intellectual functioning. An interpretation of intelligence in terms of learning would, then, entail finding some theoretically meaningful quantity with which variations in intelligence might prove to be correlated; his choice, as we have seen, was the number of available associative connections.

In the course of a rather extensive review of the literature on learning theory and intelligence (Estes, 1970), I have come to think that a new effort toward establishing a more significant working relationship between the two research traditions should begin by turning the problem around. Rather than looking to learning or physiological theory for some correlate of intelligence, I should like to focus attention on intellectual activity itself. By bringing the concepts and methods of other disciplines to bear on the analysis of intellectual behavior we may come to understand how the conditions responsible for the development of its

constituent processes and the manner of their organization lead to variations in effectiveness of intellectual functioning.

If this approach has appeal in principle, we need next to consider just what behaviors to analyze in order to be sure that the activity we are dealing with is closely related to that involved in measurement of intelligence. The simplest and most direct approach, it seems, is to begin with the specific behaviors involved in responding to items on intelligence tests.

The idea of relating performance on subtests of intelligence scales to various psychological categories or functions is by no means new. In his detailed analysis of the Stanford revision of the Binet scale, McNemar (1942) considered the idea that the various subtests of the Binet that seemed to have something to do with memory (e.g., digit span, repeating sentences, copying bead strings from memory) are alternative measures of a single memory function that might represent a purer manifestation of intelligence than the variegated tests of the scale as a whole. McNemar found the reliability of a pooled "memory" test to be relatively low and took this finding as rather negative evidence. However, he was not entirely satisfied with this basis for a conclusion and added:

> The final answer as to whether variance in measured intelligence is more dependent upon retentivity than upon original learning, or as to the extent of each as a contributor, must be sought in the laboratory. We hazard to guess that securing the answer will involve experimentation rather than wholesale correlational analysis [McNemar, 1942, p. 151].

The experimental analysis that McNemar found so sorely wanting 30 years ago did not follow with unseemly speed. Consideration of the few examples of relevant research that are now available will, I think, make clear some of the reasons for the long delay in their appearance. I suggest that we will find a dominant factor to be changes in the state of theory concerning learning and memory over this period.

In order to give some idea of the kinds of analyses that are immediately within our reach, I would first like to discuss two cases in which the identical task that constitutes a subtest of an intelligence scale has been subjected to laboratory investigation, and then two cases in which only rather modest extrapolation from closely relevant research is required.

Analyses of Test Behavior

THE DIGIT SPAN TEST

One of the simplest appearing subtests of the Stanford-Binet is the digit span test. As it occurs in the scales for Year 10, for example, the subject's task is to repeat a sequence of random digits immediately after they have been read aloud by the examiner. Successful recall of a string of six digits in the correct order is scored as passing performance at this level. The test correlates satisfactorily with total score on the Binet scale and with usual validation criteria, so it evidently samples some aspect or aspects of intellectual performance. But if an individual scores low on this test, what measures should we expect to be useful in improving his performance in this kind of task? An obvious possibility would be practice at recalling sequences of digits. But this seems rather narrow and most unlikely to transfer to any other situation. A better plan might be to have the individual practice recalling sequences of items of different types, digits, letters, words, etc.

These suggestions, together with an admonition to carry out the practice in a variety of contexts, would seem to be about all that could be derived from the learning theory available to Thorndike (1926) when he was writing *Measurement of Intelligence.*

If any subtest of the Stanford-Binet measures only the lower, associative abilities, the digit span test must surely qualify. The basis for successful performance would be conceived in classical association theory to be the establishment, or at least the strengthening, of interitem associations so that if, for example, the digit string 691472 were presented, the resulting structure in memory might be represented as 6–9–1–4–7–2. The request to recall would be the stimulus for remembering the first digit of the string, this in turn would lead to recall of the next digit and so on. If an individual failed the test, the interpretation would be either that he was inefficient in forming the required new associations, or that the strength of these associations in memory decayed too rapidly to permit successful recall.

A substantial body of research and theory dealing with short-term memory for sequences of items shows quite clearly that this interpretation in terms of interitem associations is inadequate and is in fact quite possibly entirely wrong.

Perhaps the most basic difficulty is presented by rather direct evidence that the item in position n of a string is not a necessary and sufficient stimulus, or even necessarily part of the stimulus, for recall of the item at position $n + 1$. For example, Jahnke (1970) showed that for most serial positions in a string, an individual's recall of the item at position n is better if he is simply asked to recall the item at that serial position than if he is given the item at position $n - 1$ and asked to recall its successor. This result was obtained with young adults who, it appears, must have established in long-term memory a conception of the sequence of ordinal numbers and must also have available a strategy of relating incoming items of a sequence to ordinal position.

There is considerable evidence to suggest that no effective associations at all may form between adjacent items in a sequence unless the individual has been instructed that he will be required to use individual items as retrieval cues in a subsequent test. Lesgold and Bower (1970) found no transfer whatever from learning of a serial list of items to a subsequent paired-associate task in which the pairs were adjacent items from the serial list unless the subjects had been informed in advance of the relationship between the serial and paired-associate tasks.

Numerous studies that have appeared since Miller's (1956) introduction of the concept of *chunking* have shown that, for strings greater than three or four items, recall is greatly facilitated if the individual groups the items into subgroups of approximately three items each. This procedure of grouping, or chunking, whether done by the experimenter as he presents the material or spontaneously by the subject as he attempts to organize it in his memory, is a powerful determiner of what is and is not learned in a short-term memory situation. Bower and Winzenz (1969), for example, showed that when the material is grouped by the experimenter at input, repetition of the string of digits leads to improvement in recall only if the particular mode of grouping is the same upon subsequent repetitions of the string.

Further, grouping is as important a factor with children as with adults, and with mentally deficient or brain-injured children as with normals. Spitz (1966), utilizing a modified digit span test with visual presentations, showed a substantial facilitatory effect of grouping of the digits upon performance of retarded children with an average IQ of 60; the effect was present but smaller with equal mental age normals, perhaps because the grouping (by twos rather than by threes) was not optimal. Notably, it was found that when the retarded children who had been given the grouped presentation were subsequently tested with ungrouped material, their performance declined, whereas that of normal children was unaffected, indicating that the retarded children had not developed the habit of spontaneously grouping material in order to aid recall.

These and related lines of evidence have suggested an interpretation of short-term memory for strings of verbal units in terms of organization and coding. Johnson (1970) developed in detail the idea that grouping facilitates recall because the individual assigns a code to each subgroup of a sequence and maintains in memory only the smaller set of codes until the time of recall, when he decodes these in order to reconstruct the original sequence. When these ideas are taken into account, the simple schema of classical connectionism becomes amplified into a hierarchical structure of representations in memory:

	LIST	
	CHUNK 1	
Position 1	Position 2	Position 3
6	9	1
	CHUNK 2	
Position 1	Position 2	Position 3
4	7	2

On presentation of the digit sequence 691472, the individual is conceived to subgroup the sequence into two chunks, assigning a code to each which he maintains in memory, and within each chunk relating the items of the sequence to the ordinal numbers 1, 2, and 3. On a request to recall the string, the individual brings into memory his coded representations of the two chunks; each of these in turn activates recall of the individual digits and their associated serial positions. While this process goes on, the individual must hold the partially reconstructed sequence in an output response buffer by an inhibitory process until the decoding is complete and then emit the digits in the proper order. This schema may seem complex, and certainly in details it is by no means beyond controversy, but numerous lines of solid evidence in addition to those reviewed briefly in the preceding paragraphs indicate that something not far from this theoretical picture is required (Estes, 1972).

With the revised schema in mind, we can readily see that there must be many different ways in which major individual differences can arise with respect to performance on a digit span test. In the young or the mentally retarded child, we may find failures occurring because the individual has not developed sufficient familiarity with the sequence of ordinal numbers, or enough experience in using the number sequence to order other materials. An individual of any age may fall short of standard performance if he has not developed the strategy of grouping to the extent characteristic of his age group, but may perform normally when led to utilize grouping by the experimenter or examiner. Another individual may not be able to accomplish the coding process necessary to take advantage of chunking even when material is presented to him in subgroups. Still another may be able to recall well enough but may lack the capacity for selective inhibition necessary to order his output properly.

Clearly it would be possible with the advantage of added theoretical insight to augment the standard digit span test in such a way as to localize the source of difficulty for an individual who fails under the standard procedure. This augmentation would quite likely do little to improve the predictive value of the test, but it might be of considerable help in indicating how deficient performance in this and related tasks might be remedied.

DIGIT SYMBOL SUBSTITUTION

Although various aspects of the digit span task have been studied in the laboratory, the fact that it also appears in intelligence scales is largely coincidental. Motivation and direction for these studies have arisen from previous theory and research on learning and memory. I have been able to find very few instances in which a task has been taken directly from an intelligence scale and subjected to experimental investigation with a view to elucidating the basis of its clinical usefulness. Of these, the most substantial is the recently reported investigation of the digit symbol task by Royer (1971).

The point of departure for Royer's study was the digit symbol subtest of the Wechsler Adult Intelligence Scale, which has been of special clinical interest because of its steep decrement in performance with age and its sensitivity to various neurological defects. In the standard form of the test, the individual being examined is given a table relating nine symbols to the digits 1–9, as illustrated in the

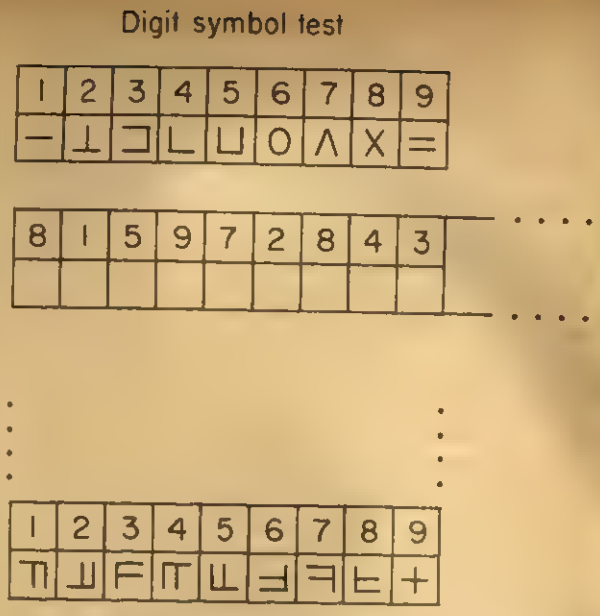

Figure 1. Assignments of symbols to digits in Wechsler digit symbol scale (first two rows) and in set 5 of Royer's (1971) study (last two rows), together with sample from an answer sheet (third and fourth rows).

upper two rows of Figure 1, and an answer sheet with rows of boxes above which numerals appear in a haphazard order. His task is to place the appropriate symbols in the boxes, and his score is the number of substitutions completed in 90 seconds. The task can just as well be presented in reverse, with numerals being substituted for symbols, and in fact Royer studied both variations.

Royer (1971) noted that the test can be considered to be a measure of information-processing capacity because an index of rate of information processing derived from information theory proves to be linearly related to standardized score on the test. Further, within the set of symbols two types of information which have been significant in perceptual theory may be distinguished. One of these, which for brevity may be termed *spatial information,* serves only to distinguish members of a subset of symbols which belong to a rotational equivalence set, that is, which may be superposed following an appropriate rotation (e.g., the third and fifth figures in the second row of Figure 1). The other type of information distinguishes figures that cannot be superposed following any possible rotation.

In his first experiment Royer (1971) varied the distribution of digits on the answer form—balanced (rectangular) versus unbalanced—the latter being

the condition obtaining on the Wechsler scale, and relative proportion of content and spatial information in the symbol set. The symbol set from the Wechsler scale was utilized, together with four others which included increasing proportions of symbols that differed only on the spatial dimension. The fifth set, in which all but one of the symbols belonged to the same rotation-equivalence set, is shown in the bottom row of Figure 1. Administration of this set of variations to a large group of college student subjects yielded no significant effect of the distributional variable but a substantial and virtually linear function relating score to the similarity condition.

One might be tempted immediately to seize on this function as indicating the basis for clinical significance of the test because difficulties in handling the spatial information component are known to characterize young children and individuals with various neurological disorders. However, we should first examine the task more closely in order to see whether other aspects of performance might covary with the nature of the symbol set. At each step in the task the subject must inspect the next digit, go to the proper location in the table, code the information distinguishing the symbol found, and carry this information in short-term memory long enough to reproduce the symbol in the proper answer box.

Looking first at the purely motor aspects of the task, one must inquire whether one relevant factor might be differences in difficulty in reproducing the symbols in the various sets. Royer (1971) investigated this possibility in a second experiment in which subjects were required simply to copy haphazard sequences of symbols drawn from the various sets. He found a significant trend in the same direction as that obtained for test performance, but concluded that the variation was relatively small compared to that in the substitution task itself.

Probably of considerably more importance are factors having to do with the coding of the information in the symbols. Excluding individuals who can maintain fully adequate visual images, probably a rarity in the adult population, efficient performance must depend on the individual's having available or being able quickly to produce distinctive codes to represent each of the symbols in memory. On this assumption, the task would be the easiest for the Wechsler symbols. Many of these have distinctive labels readily available, for example, a minus sign for the first symbol, an inverted T for the second, and an L for the fourth (second row of Figure 1). In contrast, the symbols of Set 5 (bottom row of Figure 1) would require the creation of an adequate labeling system in the course of the substitution task.

Once accomplished, coding will obviate the necessity of looking more than once at the symbol required for a given substitution. Further, it is likely that even within the short time involved in the test some individuals will be able to learn some of the digit code associations and thus on many occasions not need to look from the digit to the table at all in order to make the appropriate substitution. It is clear from examining the sets of symbols used by Royer that suitable codes would be least readily available for the set containing the largest number of members of a single rotational equivalence group.

A satisfactory interpretation of Royer's principal results awaits further research which may serve to differentiate the role of relative proportions of content and spatial information from that of availability of distinctive codes. Evidence now available seems rather clearly to point to the latter interpretation. I have in mind, particularly, the extensive data concerning sex differences in performance on subtests of intelligence scales. Generally, girls score higher on verbal subtests, and boys, on those involving spatial manipulations. Digit symbol substitution has usually been assigned to the nonverbal category, and consequently the superior performance by girls (McNemar, 1942) has seemed an anomaly. The explanation, I suggest, is that skill in digit symbol substitution involves a verbal-encoding process as a major component.

VOCABULARY

The ubiquitous vocabulary test, appearing, for example, at all of the higher age levels in the Stanford-Binet, is similar to the digit span test both in the deceptively simple appearance of the task and in the substantial growth of relevant theory over the years in which the test has been used, but differs in the lack of directly relevant experimental analysis in the learning laboratory.

Considering how inextricably the vocabulary test has been bound up with the assessment of intelligence over nearly three quarters of a century, it seems remarkable how little is known as to just what aspects of intellectual performance are being measured. In particular, we are largely ignorant concerning the extent to which the test taps vocabu-

lary, in the sense of the stock of words an individual uses or can recognize, as distinguished from the processes involved in constructing a definition or an explanation of the meaning of a word.

Vocabulary tests have been validated extensively, but evidently always against other measures or criteria of intelligence, not against alternative measures of vocabulary. What might these other measures be? One, in particular, is suggested by recent studies of Howes (1971) on the direct estimation of an individual's vocabulary via time-sampling procedures. Howes' method is to record several thousand words of informal discourse from an individual, then, utilizing a statistical model relating word samples to populations, to obtain an estimate of the individual's vocabulary which is quite independent of the other processes involved in answering the usual vocabulary test items.

In terms of contemporary learning theory, what should we expect to be the principal determinants of performance when an individual is presented with a series of words and asked to give definitions or explain their meanings? Most basic, in a sense, must be the long-term memory structures established by previous experience with the words in question. In classical association theory, this result of past learning was conceived of simply as a set of associations between a particular word and others which might be used to convey its meaning. In contemporary thinking, the status of a word in memory is characterized by its values with respect to a set of features or attributes (Anglin, 1970; Underwood, 1969). Accessibility to this representation in memory is determined by the presence of retrieval cues (Tulving, 1968), which may be conceived as learned associations between the cluster of features and the contexts in which the word has been or might be used. But the existence of these associations is not enough; actual performance at any time depends on their availability, which in turn is a function of recency and frequency of usage (Allen, Mahler, & Estes, 1969; Horowitz, Norman, & Day, 1966).

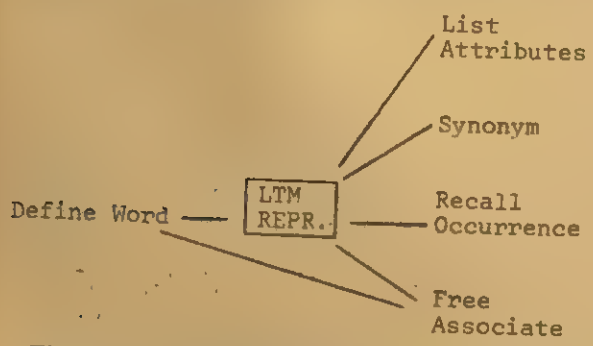

Figure 2. Levels of response to a request for definition of a word, all except the lowest requiring activation of a representation of the word in long-term memory (LTM).

Depending on the specific retrieval strategies that he has learned to use and the availability of retrieval cues, an individual who is asked to give the meaning of a word on a vocabulary test may function at a number of levels, as illustrated in Figure 2. The most primitive answer in a sense, and the least efficient, is that of simply giving some other words which are aroused by association. This type of answer deserves, and in practice receives, only partial credit because it does not tap the organization of word meaning in memory. If the coded representation of the word in long-term memory is activated, the individual can attempt to produce an acceptable answer by recalling specific occurrences of the word and their circumstances, by recalling a dictionary definition or its equivalent, or by actually expressing the features and attributes that serve both to relate and to differentiate the word from others in his long-term memory system.

Finally, though by no means least important, production of an acceptable answer in this as in any other test situation requires that the individual have in mind a conception of the characteristics of an acceptable answer, in this instance of an acceptable definition or explanation of meaning of a word. For a child who has grown up in a more or less average home environment with numerous opportunities and occasions to inquire concerning the meanings of words and to have these explained to him by parents and siblings, this requirement is so automatically satisfied that it may tend to be taken for granted. Whenever he is able to achieve the necessary information, the child will be able to generate an acceptable answer by comparing his own partially formed utterances with the properties he knows from his own experience to characterize a satisfactory definition. A child who has not had these opportunities will lack the basis for corrective feedback of his production.

Clearly an individual's successful passing of a vocabulary test provides valuable information as to the extent to which the numerous prerequisites for successful performance have been simultaneously satisfied by his combination of inherent capacities and past experiences. But failure gives little diagnostic information, for it can come about in many

qualitatively different ways. Inability to explain or define a word on request may occur because the necessary memory structure has never been established, because of a lack of retrieval cues for an intact memory structure, because words required to express the definition are at low availability owing to disuse, or because the individual lacks a general conception of the required solution to this type of problem and thus gives an answer which is meaningful within his own frame of reference but not within that of the examiner.

I think we must concede that, in comparison to digit span, present learning theory is not so clearly in a position to prescribe directly ways in which conventional tests of vocabulary might be augmented in order to provide more information concerning the different processes involved in test performance. However, it does appear that the line of analysis we have illustrated may go beyond the classical psychometric approach in generating specific kinds of research which might provide bases for remodeling the vocabulary test from a pure instrument for prediction of performance to one more useful in diagnoses of processes.

WORD NAMING

One's first impression of this task, a subtest of Year 10 of the Stanford-Binet scale, may well be that it scarcely seems to constitute an intellectual task at all. The individual is simply requested to name as many different words as he can in one minute, excluding counting or the use of sentences. Since, however, correlations of this subtest with total test score and with criteria are of the same order of magnitude as those for subtests with more obvious intellectual content, closer examination is evidently called for.

Consideration of the performance required of the individual immediately points to two of the processes intimately involved in the vocabulary and digit span tests, namely, the availability of words as responses and short-term memory, respectively. However, it appears that neither of these can be a major determinant of individual differences in performance. All but the most severely retarded individuals to whom this test might be given must have many more words in a relatively high state of availability than the 28 required as a passing score at Year 10. Short-term memory must be involved to some extent because the individual must keep track of the words he has already given in order to avoid repetitions during output. However, the bulk of available evidence (see Belmont & Butterfield, 1969; Estes, 1970) indicates that rate of short-term forgetting does not vary substantially with either age or intelligence over a wide range.

A more central factor in the word-naming test is manifest if we note that it is in a sense a limiting case of the free verbal recall experiment, in which the subject is read a list of words and then asked immediately to recall as many as possible in any order. A very substantial amount of recent research shows clearly that performance in free recall is largely determined by the organization of an individual's long-term memory system and the way in which he makes use of this organization to guide performance in the testing situation. Data obtained by administration of free-recall tests to various populations show that amount recalled from a given list length increases systematically with age, education, and intelligence and that the tendency of the individual to organize his output in terms of clusters of meaningfully related words exhibits parallel trends. More importantly, the performance of young or mentally retarded subjects can be brought up to substantially higher levels if the individuals are led by the examiner to make use of meaningful categories in organizing their responses (for a review of this literature, see Estes, 1970).

A sharp distinction needs to be made between the extent to which an individual's memory is organized in terms of categories and the extent to which he utilizes these categories in retrieving material in a test situation. For example, on examination with the usual procedures, children belonging to a nonliterate African community appeared substantially inferior to American schoolchildren of similar age in free recall (Cole, Gay, Glick, & Sharp, 1971), and it would be easy to dispose of this not particularly surprising finding as an example of the effects of cultural deprivation on the African children. By means of a painstaking series of researches involving close examination of the subjects' performance and of its relation to their cultural background, these investigators were able to show that when the African children were led by special techniques to respond in terms of meaningful categories their recall performance improved sharply to a level similar to that of American schoolchildren.

Mandler (1967) reviewed evidence suggesting that an individual's total vocabulary may be organized into a hierarchy of clusters of meaningfully related words in much the same fashion as the

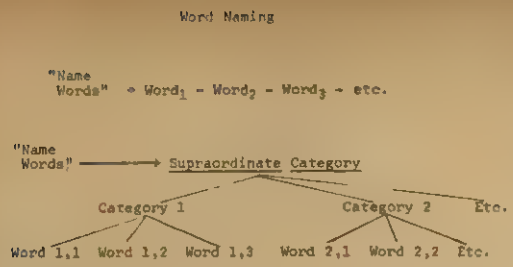

Figure 3. Schemata for processes of word naming by chain association (upper portion) and by utilizing a categorical organization of memory (lower portion).

smaller sample of material presented in a free-recall experiment and that efficient performance in a word-naming test depends importantly on the extent to which the individual utilizes this organization. Both the time required to retrieve words and the demands on short-term memory are greatly reduced to the extent that one runs through a set of categories in an orderly fashion, enumerating as many as possible of the words belonging to each category in turn. In Figure 3, this strategy (lower panel) is contrasted with the less efficient one of chain association.

The data of Cole et al. (1971) indicate that the habit of retrieving material from long-term memory by means of categories depends strongly on amount of schooling, but when amount of schooling is held constant, retrieval is only slightly related to either age per se or other aspects of cultural background. There is clearly a pressing need for still more analytical research to determine just what aspects of school experience are responsible for developing this type of intellectual strategy.

Conceptions of Intelligence and Learning Ability: Concluding Remarks

Now, where do we stand at a theoretical level with respect to the interpretation of intelligence in terms of concepts of learning theory? First, referring back to our starting point, we might ask how, in the light of ensuing developments, we would now evaluate Thorndike's proposal that differences in intellect are manifestations of differences in the total number of available associative connections. Research and logical analyses within the framework of modern learning theory have certainly done nothing to dampen the skepticism that most of us doubtless felt concerning the plausibility of Thorndike's conjecture. In particular, Thorndike's correlational evidence is now seen to be distinctly less than crucial, for our analyses yield no reason to believe that any of the tests he used provide pure measures of either associative or higher cognitive abilities. But, although subsequent events have not supported Thorndike's hypothesis, neither can they be said to have disproved it. What does become clear upon consideration of the complex interaction of biological and environmental events and processes over a prolonged period which are necessary for the development of effective intellectual behavior is that a model of the type Thorndike proposed is incapable of refutation by any methods now at hand or even foreseeable.

To be sure, some contemporary investigators, especially among those seeking to ameliorate mental deficiency by biochemical or psychopharmacological techniques, appear to operate on the tacit assumption that something akin to Thorndike's model is correct. Thus, one sees studies reported in which groups of intellectually deficient animals or children are subjected to new diets or new drugs and then tested to see whether their performance has improved relative to normals. But this strategy seems doomed to be as fruitless in the future as it has been in the past. For even if Thorndike's conjecture were correct, and if someone were to discover a drug that produced a substantial increase in the supply of available connections, the individual who was treated could not be expected to manifest any sudden increase in intellectual performance. The increased potentiality might be there, but a substantial program of reeducation and training would be required to make up for the deficiencies in past learning and to lay the groundwork for the organization of processes needed for efficient intellectual performance. I suggest that to have any rational basis for expectations of fruitful results, research directed toward the amelioration of intellectual deficiencies by physiological and chemical therapy must be conducted in close conjunction with the intensive application of the most sophisticated resources of learning theory to help trace the way through the no-man's-land between capacity and performance.

With regard to continuing theory construction, we can begin to see that theories of the form "differences in intelligence are manifestations of differences in x" or "differences between retardates and normals are produced by deficiencies in x" are the wrong kinds of theories to be trying to construct. The urgent need now is not for better means of classifying people with respect to intellectual func-

tioning and correlating these classifications with other variables, but for understanding what brings about specific kinds of competence and incompetence in intellectual activity.

An additional source of motivation for exploring the possibility of characterizing intelligence in terms of learning processes, going beyond the purely intellectual challenge, has to do with the changing mood of society. We are seeing increased resistance to the very conception of testing and measuring intelligence solely for purposes of prediction. If the trend that we see reflected daily not only in the newspapers but in the more erudite journals continues, we may find that not far in the future it will become impossible to measure intelligence, except in white, middle-class children, unless we can demonstrate some direct value to the individual being tested. This demand might be met if, for example, it came to be recognized (and in fact to be the case) that the primary purpose of intelligence testing had become that, not of predicting intellectual performance, but rather of indicating and guiding measures that can be taken to improve intellectual performance.

Little can be accomplished in this direction with present instruments for measuring intelligence because these operate primarily by sampling performance, and in every type of intellectual task any given level of performance can arise in many different ways. One's first thought may be that our present instruments for measuring intelligence need to be replaced by new ones better suited for diagnostic purposes. Aside from the perhaps transient problem that we have no specific idea at present how to proceed in this direction, we face the major difficulty that almost inevitably new tests of quite different design will be less valid predictors of intellectual performance than those now in use.

An alternative route involves not replacing or revising, but rather augmenting present methods of measuring intellect. The desired goal may be achieved if we can interpret the processes involved in test behavior in terms of concepts drawn from learning theory and utilize these interpretations as a basis for developing techniques to localize the sources of the deficits in performance revealed by test scores. This is not to imply that one should expect practical benefits to follow quickly and easily from laboratory analyses of test behavior. At the very least, it will be necessary to go on to the analysis of situations outside the laboratory which call for various types of intellectual activity. However, it does seem that research on intelligence within the framework proposed here might prove as fruitful for theory as that confined to the psychometric tradition and might lead to additional long-term social gains.

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Applied Behavioral Analysis of Societal Problems

Population Change, A Case in Point

STEVEN M. ZIFFERBLATT *Stanford University* [1]
CARROLL G. HENDRICKS *Stanford University*

We face problems that challenge our survival. Not only the quality of life but life itself is at stake. Whether or not we continue to inhabit this planet depends on our ability to find solutions to critical societal and ecological problems such as population control and pollution. Psychologists are becoming increasingly involved with seeking solutions to societal problems because the concerns people bring to psychologists are often traced to problems at a societal level.

The challenge at hand for behavioral science, more specifically psychology, is assisting people to behave in ways that will insure our survival. New strategies must be developed that change societal problems by changing human behaviors.

WHY PSYCHOLOGY?

Medical science and engineering are among those disciplines that humanity traditionally relies on for solutions. A substantial technology now exists; the difficulty lies in its implementation. A number of effective sanitation, birth control, nutrition, and antipollution devices and products now exist, yet their employment is somewhat unsystematic and thus ineffective. Quite often these problems relate to the behavior of people employing the technology. Scientific tools are used to create a physical technology. Similar scientific tools must be used in delivering, implementing, and maintaining the use of this technology. Developing an effective birth control device is one thing, but designing an environment ensuring its proper use is quite another.

The problem of designing environments to insure proper use of technology is far too critical to the world to remain unexamined by the tools of science. Psychologists are scientists skilled in dealing with the problems of behavior and behavior change. Societal problems are ultimately problems in changing human behavior.

A PROBLEM OF PROBLEM DEFINITION

Approaches to societal problems in the past have proved so inadequate that even politicians are careful to talk of solutions in only the vaguest terms. A major obstacle is that problems are conceptualized in ways that do not facilitate solutions. What is done about a problem depends on how it is defined. The way a societal problem is defined determines the focus, the techniques of intervention, and the criteria for evaluation. The way a problem is defined also determines not only what is done about it but what is not done about it (Caplan & Nelson, 1973). If attitudes and values are argued to be the basis for problem definition in population change and family planning (Cordero, 1968), then a host of everyday behaviors and their motivational factors also quite close to the problem are possibly overlooked as the foci of change.

Social critics often provide perceptive analyses, but their criticisms are usually at a global, descriptive level. An analogue may be found in traditional diagnostic procedures in psychotherapy, in which a client is classified with a global label such as *schizophrenic* on the basis of diagnostic tests that have limited validity and reliability (Mischel, 1968; Stuart, 1970). The major criticism of these diagnostic procedures is that the label contributes little toward solving the client's problems. Similarly, current diagnoses of societal problems ("impending

[1] Requests for reprints should be sent to Steven M. Zifferblatt, Stanford Heart Disease Prevention Program-Behavioral Studies, 730 Welch Road, Palo Alto, California 94305.

crisis," "overpopulation," "lack of motivation") do not readily point to solutions (Ehrlich, 1970; Slater, 1969; Toffler, 1970). For example, Toffler summarized the major emphasis in his book as "diagnosis":

> Yet the basic thrust of this book is diagnosis. For diagnosis precedes cure, and we cannot begin to help ourselves until we become sensitively conscious of the problem.
>
> These pages will have served their purpose if, in some measure, they help create the consciousness needed by man to undertake the control of change, the guidance of his evolution. For, by making use of change to channel change, we cannot only spare ourselves the trauma of future shock, we can reach out and humanize distant tomorrows [p. 430].

What Toffler and others have not emphasized is *how* we are to promote critically needed behavior change. Scientifically based methods of behavior change exist, *but* we must learn how to use them.

Family Planning and Population Change: A Case in Point

A common thread weaving its way through psychological research in population change and family planning is the search for underlying and common causal variables that transcend individual differences within a culture and account for the problem phenomena (Carlaw, 1971; Cordero, 1968; Fischer, 1972; Rosario, 1971). Once these variables are identified through correlational and factorial research then hypotheses may be established and experimental research conducted. Finally, these hypotheses will hopefully lead to useful treatment strategies that eventuate in behavior change. When behavior change or control over behaviors related to problem areas are used as criteria, these attempts have not borne fruit. Studies have also revealed the prevalence of certain attitudes or values across a given subculture. However, it is one thing to reveal a value across a given subculture, but it is quite another to design intervention or change strategies for the same subculture. For example, psychoanalytically oriented psychologists maintain that the dynamics of childbearing, sexual drives, and generational continuity are a function of early childhood wishes (Benedek, 1952; Erickson, 1964; Kestenberg, 1968; Wyatt, 1967). Even if the validity of this information is accepted, the usefulness of this information, in relation to how it bears on changing the problem situation, is questionable.

Many attitudinal studies across cultures have appeared in the area of family planning and population change. These studies relate to (*a*) childbearing motivations; (*b*) parental attitudes toward existing children; (*c*) intelligence as a function of family size and child rearing; (*d*) population attitudes of students; and (*e*) individual meanings of sex, marriage, contraception, menstruation, and children (Fawcett, 1969).

Substantive findings leading to the development of change strategies that resolve the problem rarely emerge as consequences of these research activities.

One reason for the lack of action-oriented findings is the "state of behavioral science technology." Fawcett (1969) stated that "most attitude research in the population field is lacking in methodological sophistication." Others argue that there is a need for improved measurement of psychological variables in population studies (Radel, 1972; Stephens, 1962). Other social scientists have called for improved research designs; better statistical control over sociocultural factors and personal variables; better inventory or baseline studies; and better generation, testing, and evaluation of hypotheses (Bogue, 1966; Yaukey, Griffiths, & Roberts, 1967). These responses, which cite the lack of relevant and useful procedures to solve family planning and population change problems, are perhaps problems inherent in all psychological research. Typically, psychological research in these areas abounds with calls for "new role definitions," "better measurement procedures," "more sophisticated experimentation," or "a theory of family planning and population change."

These practices reflect a rather limited view of psychological research—that of hypothetico-deductive and nomological inquiry—a view that is currently accepted by psychologists in the area of family planning and population change. Nomological inquiry, simply stated, is based indirectly or directly on theory building—a search for relevant hypotheses, common factors, and causal variables ("building blocks") with the hope of eventually "putting it all together." However, the potential usefulness of this approach to understanding human behavior, as of this moment, is questionable. Elaborate theories of human behavior all stand side by side, all taught and researched, yet few create procedures able to change human behavior in beneficial ways. Population psychologists seem to be moving toward the reinvention of the square wheel. It is a luxury we cannot afford.

There are, in addition, certain "socialization" variables that psychologists doing this "scientific

and theoretically sophisticated" research are subject to and tend perhaps to maintain irrelevant behavior. Caplan (1973) noted that rarely, if ever, are advancement and recognition judged on the effects of one's scientific behavior on the people or problematic phenomena studies. What is good for the social sciences and the individual social scientist may not entail that it be good for those on whom research is being done.

Recently, in an American Psychological Association survey of 368 faculty members at 95 universities, 83% of the faculty members thought academic psychology should be concerned with solving social problems and 79% said it was making no significant contribution. Among 2,140 graduate students, 90% said psychology was not doing much to solve social problems. Both faculty members and graduate students cited unequal opportunities for education, poor race relations, deterioration of the environment, militarism, war, and poverty as issues psychology should be dealing with (Lipsey, 1973).

The point of this cursory review is to identify an epistemological bias in current psychological research in family planning and population change that contributes to its lack of relevance in providing solutions to problems.

There are empirically and technologically based alternatives available that are not as epistemologically eloquent, but perhaps are more to the point: changing people's behavior in ways that may provide solutions to family planning and population change problems.

In short, population psychologists simply must get on with the business of effecting changes in problem behaviors. Building theories or identifying variables that have little implication for altering problem situations is a luxury we cannot afford. Variables that explain or are considered causal may be the least alterable. For example, we do know that under certain conditions urbanization causes population explosions in minority groups. Yet the population psychologist or the applied demographer can do little to reduce the birthrate by manipulating "degrees of urbanization." Research into the relationship between urbanization and population growth has little immediate payoff in solving the problem. But psychologists can find ways to facilitate and promote behaviors related to contraceptive use, decision making, and modes of sexual activity. We are faced with societal problems that require solutions in fact, not in principle. The primary questions are, What works? On whom? Under what conditions? *Why* things work is a long-term ambitious goal and one that may not enhance our chances for survival.

Family Planning Programs: The Current Status of Intervention Strategies

A number of ad hoc empirically oriented family planning programs have, in fact, been instituted. These programs emphasize (*a*) provision of supplies and services, (*b*) the dissemination of information, and (*c*) the education or training of health care agents (Bogue, 1966; Lozare, 1972; Ludhiana, 1963; Puerto Rico, 1963; Taichung, 1963; Worrall, 1971). Currently, major emphasis in family planning programs is placed on dissemination of information, supplies, or services. These variables are considered the major factors to be manipulated. Some programs have experienced moderate success with those families already expressing an interest in family planning (Ross, Germain, Forrest, & Van Ginneken, 1972). These programs do not extend to the vast majority of families who, for many reasons, do not receive family planning services.

Empirical evidence suggests that motivational factors might play a significant role in increasing the effectiveness of family planning programs (Ridker & Muscat, 1973). A number of alternatives have been suggested, for example, educational subsidies for children; old age security; employment opportunities; financial, material incentives; persuasion by significant people; and sundry educational programs. These alternatives possess a number of common attributes:

1. It is assumed that substantial impact can be made in family planning by providing more positive consequences (incentives or motivation) for a target population, for example, nonfertility, menstruation, or menstrual aspiration.

2. It is assumed that economic factors have great impact on motivators in a given culture.

3. Systematic efforts should be concentrated in a search for incentives. If appropriate incentives are identified and programmed, then changes will occur in family planning behavior.

What seems to be lacking from many family planning programs is (*a*) a clear statement of behavioral goals for a given family or program, (*b*) a methodology for focusing on those factors and behaviors that will most likely have an effect on the

problem behavior of a given family, (*c*) a procedure for selecting an appropriate intervention (change) strategy for a given behavior, and (*d*) a strategy that can represent *immediate* behavioral outcomes and lead to subsequent modification of a particular program.

IS FAMILY PLANNING A RATIONAL PROCESS?

One criticism of family planning endeavors is that family planning is not a rational process. This is, in part, due to the complexity of behavior; for example, there are a host of variables that might account for a family's decision to have or not to have children. "Rational" behavior usually suggests isolation of critical problem parameters and an ability to predict family planning behavior via the manipulation of those parameters. Thus, rationality of a behavior, simple or complex, is largely determined by our ability to perform a functional analysis of those events antecedent and consequent to the behavior under investigation.

Part of the problem may be that there are a number of conceptual orientations to problems available to behavior scientists. Each alternative, for epistemological reasons, arbitrarily includes and excludes various data, levels of data (behavioral, mental, historical, subconscious), and methodologies (nomological, inductive, comparative group, $N = 1$).

Problems that seem insoluble or extremely complex in one discipline or epistemological context may, in fact, be defined in different ways in another context and be subject to an alternative empirical approach. Let us take, for example, the problem of another complex set of behaviors: autism. We will trace its definition, its treatment, and finally its pessimistic prognosis within a medical model context and contrast this to recent developments in an alternative model for treating autism, a behavioral model. Recent developments with this latter alternative suggest that autistic behavior may be subject to a functional analysis.

Until quite recently, the predominant approach to the understanding and treatment of autism and other severe childhood disorders has been the *medical model,* which has included both psychodynamic (psychogenic) and biogenic schools (Rimland, 1969; Rutter & Bartak, 1971; Rutter, Bartak, & Newman, 1973). The medical model is characterized by two assumptions or working hypotheses concerning the etiology and treatment of autism. The locus or cause of the disorder is internal. The child's behavior is merely symptomatic of a variety of inner events (cognitive, structural, inherited, or metabolic). Another hallmark assumption of the medical model has been that therapy is derived from and consistent with notions of causation. This orientation has, in fact, been used in treating illness for several millennia. It is one of many alternatives, and in the case of autism the most optimistic interpretation of the results has been that they are "ambiguous." Many writers have noted that existing theories of autism are as yet unverified if not actually disconfirmed. Both the psychogenic and biogenic approaches to autism have been criticized for not providing confirming evidence of the essential internal emotional disturbance (Rutter, 1969) or of a physiological one (Rutter & Bartak, 1971).

In sharp contrast to the medical model is the *applied behavior analysis model.* The child's disordered behavior is the problem. No inference to personality, history, or unobservable or internal emotional conflicts is made. Instead the events that either generate or maintain the behavior are to be found in the child's immediate environment, particularly those events that impinge on the child in systematic ways, that is, certain classes of reinforcers that maintain inappropriate behavior. In other words, the child's problem is regarded as a direct consequence of here-and-now external stimulus events.

The contrast in these two models is indeed dramatic and exemplifies the point that statements concerning complexity and rationality are a function of epistemological bias. Until recently the most predominant model for autism was medical, along with its attendant epistemological consequences. Searches for underlying causal variables were the mainstream of research activity. Progress was slow. Autism was an untreatable mystery. Theories and rich in-depth euphemisms were abundant. However, when an alternative model was employed, namely, that characteristic of applied behavior analysis, the problem definition, locus, and methodology changed dramatically, and "complexity" became a useless descriptive variable. A search for a cause was laid aside, preceded by emphasis on solving the problem rather than explaining the problem. A technology was developed that provided a number of rapid and dramatic changes in the autistic child's behavior (Ayllon & Michael, 1959; Ferster & DeMyer, 1961; Lindsley, 1956). True, a causal framework has not been developed,

but a technology to treat the problem and change behavior has. An alternative framework has led to a radically different approach to problem solving, one that appears more parsimonious and practical. Its heuristic value and ultimate contribution to a causal explanation of autism might be limited, but, at the risk of being trite, it works!

The potential gains possibly obtained from an applied behavior analysis strategy to family planning problems, aside from its theoretical irrelevance, cannot be overlooked. An applied behavior analysis strategy identifies an array of problem parameters that are quite different from those derived from more medically and nomologically based inquiry models. For example, possible parameters might relate to the types of consequences that maintain very specific problem-related behaviors (social approval, status, extrinsic nonrelated material rewards, peer recognition, etc.). The schedule for their delivery (daily, immediately, hourly), their amount, or intensity might be another set of parameters, and other parameters might relate to controlling situations or discriminative stimuli which set the occasion for appropriate and inappropriate behavior (time, pre-intercourse procedures, meals, pill ingestion, and setting). Thus, what appears to be valued as irrational behavior might merely be a function of its epistemological context, as with autism. When viewed through alternative frameworks and accompanied by a different array of data and methodologies, family planning behavior might be quite rational.

Applied Behavior Analysis: An Existing Technology

A powerful technology for changing human behavior has emerged during the past two decades. Known variously as behavior modification or applied behavior analysis, this technology has been applied successfully to problems in education, psychotherapy, and institutional care (Thoresen, 1973). One reason for this success has been the techniques or behavior change principles that are part of applied behavior analysis. The systematic use of reinforcement, punishment, and modeling can account for substantial changes in behavior. But beyond technique, the success of applied behavior analysis is due in large part to the sequence of activities in which applied behavior analysts engage in order to solve problems. These activities, termed here as the *behavior analysis process*, may be applied to any problem, be it organizational, clinical, or personal.

Although there is considerable room for argument as to the exact range of these activities, most behavior analysts would agree that the following components are common to most, if not all, successful applications of applied behavior analysis.

BEHAVIORAL OBJECTIVES

An initial step in applied behavior analysis is the specification of objectives. Objectives in family planning have often been too vague to be meaningful. For example, an objective "to promote positive attitudes toward small families among Mexican-Americans in the Los Angeles area" does not specify a behavior, the extent or amount of the behavior required, or the conditions under which it should occur. In contrast, the objectives "to take one oral contraceptive pill every morning before breakfast" or "to visit the family planning clinic on the first Tuesday of the month at 10:00 a.m." specify the behavior, the amount, and the conditions. Proper specification of objectives in family planning can be extremely beneficial because the problem, in order to be solved, must be conceptualized as behaviors to be changed.

FUNCTIONAL ANALYSIS

An integral part of applied behavior analysis is a procedure called *functional analysis*, in which behavior is analyzed in the context of its antecedents and consequences. This procedure is often used as a diagnostic tool in applied behavior analysis programs because contingency relationships are often revealed through the observation of antecedent and consequent events. For example, antecedent events for taking a pill might be (*a*) a reminder, (*b*) a chart, or (*c*) the bottle placed on the breakfast table. Consequences might be social reinforcement in the form of a spouse or sibling compliment or a prearranged incentive for the behavior. Functional analysis, so named because behavior is a function of its antecedents and consequences, provides information about the events that control behavior and, therefore, about the events that must be changed to effect a change in behavior. Antecedents and consequences to "taking one oral contraceptive every morning at breakfast" control the behavior. The critical antecedent in a change strategy might be the system of cues a woman uses to remind herself to take the pill. Engineering such an environment

would set the occasion for the behavior. A meaningful consequence, in addition to the obvious one of remaining nonpregnant, might be a kind word from her husband. This would help to maintain the behavior.

BEHAVIORAL DATA

It has been said that the most important contribution of behavior modification has been in the use of behavioral data collected in naturalistic settings (Bijou, Peterson, & Ault, 1968; Johnson & Bolstad, 1973). The process of applied behavior analysis relies on information about the actions of human beings in the situations in which those actions are most relevant. An emphasis on what people *do* might be the most important step for family planners to take at present. For example, if a set of parents produces seven children, it might be inferred that their attitude toward birth control is at best laissez-faire. *All too often the attitude has been the data.* It would be more meaningful to collect information about the nature of the action that resulted in an unplanned conception by isolating a behavior and studying its antecedent/consequent events.

Although some behaviors in family planning must be started "from scratch," most of these behaviors are already extant in the repertoires of individuals. For example, everyone has pill-taking behavior in his repertoire, but this behavior must be brought under the proper stimulus conditions. Each family follows certain rituals antecedent to intercourse; these activities, with the addition of some birth control steps, can also be brought under the appropriate stimulus conditions.

PRINCIPLES OF BEHAVIOR

It is impossible to comprehend human behavior without considering the observable environmental, physical, social, and cultural events in which behavior occurs. These events, which precede or succeed behaviors, must be considered carefully in attempts to either change or initiate new behavior. Applied behavior analysis tries to identify these events antecedent and consequent to behavior and to "rearrange" the environment to obtain desired outcomes. This "rearrangement" entails deliberate and careful selection of potentially effective antecedents and consequences to influence the occurrence of more adaptive behavior. Principles of behavior such as reinforcement, extinction, cuing, and punishment are, in fact, different combinations of pleasant and aversive antecedents and consequences. Their application in the form of contingency arrangements obtains desired behavioral effects, for example, acquiring, eliminating, increasing, decreasing, or maintaining behavior. Techniques such as behavior shaping, behavior contracting, modeling, and behavioral self-management help the scientist to apply principles of behavior (Thoresen, 1973).

Thus the specification of objectives and data in behavioral terms provides observable and measurable units of behavior, and a functional analysis points toward the factors that govern behavior. Finally, the actual application of one of the principles of behavior change, such as reinforcement, shaping, cuing, or extinction, is indicated by the feedback of information gathered through the following sequence of activities in the behavior analysis and change process:

1. State the problem in behavioral terms.
2. Perform functional analysis of problem-related behaviors (incorporate interdisciplinary contributions).
3. Designate target behavior.
4. Formulate behavioral objectives.
5. Develop an intervention strategy (apply principles of behavior).
6. Implement the intervention strategy.
7. Evaluate (compare data with behavioral objectives).

The Interdisciplinary Contribution to Societal Problem Solutions

The effectiveness of applied behavior analysis depends largely on identifying relevant behaviors and events for observation. Applied behavior analysis can provide the operational and procedural aspects of behavior change. Yet a universe of observational data exists for each human being and the problem-related behavior. Systematic and knowledgeable guidance is required to isolate critical behaviors and events in the culturally unique environment of each individual. Different cultures, for instance, provide differential male sex roles and incentives (e.g., money versus status), religious practices, and social behaviors which may lead the scientist to observe and select different contingency arrangements in developing an intervention strategy for population change. We may, for example, learn that there are different economic and social antecedents and consequences for large families in rural,

as opposed to urban, settings. Descriptive and empirical data from the disciplines of anthropology, psychology, and sociology can help to focus observations and analysis on relevant behaviors and events (Bijou et al., 1968).

Knowledge largely derived from marketing and advertising disciplines may provide techniques that enhance the delivery and acquisition of behavior in an intervention strategy. Thus, techniques relating to (*a*) personal influence channels, (*b*) media selection, (*c*) message structure, and (*d*) promotion may be useful adjuncts to tools derived in the social sciences (Lieberman, Gillespie, & Loghman, 1973; Worrall, 1971).

Behaviors related to societal problems and their solutions are embedded in a larger social systems context; that is, behavior change of patients and clients depends on the behavior of change agents such as public health officials, counselors, and nurses. Social systems theory (Berrien, 1968) and systems design and analysis (Zifferblatt, 1972) lend both a conceptual and operational framework to the maintenance of the change agent's behavior and thus the client's behavior. Recent investigations in the field of education indicate that generating behavior change is largely a function of developing effective change programs for the agents of change, as well as for the target population (Bushnell & Rappaport, 1971). The generation and maintenance of these supportive behaviors are equally subject to the principles and techniques of applied behavior analysis. Thus, applied behavior analysis can provide operational procedural contexts for the contributions of other disciplines (Zifferblatt, 1973).

Applied behavior analysis, in conjunction with technology, can provide the powerful tools needed to deal with critical societal problems. The challenge to psychology is clear. An effective behavior change methodology must be employed to address the most serious crisis yet to face mankind.

The Intensive Design: A Methodology for Analyzing Intervention Programs

Most analysis and evaluation models employed in intervention programs have been what Kaufman (1972) termed "summative," designed to determine if there are significant terminal differences between treatment and control groups. Various factorial designs are used to compare completed treatment programs with numerous subjects assigned to a variety of treatments. The supposed advantages of these designs are well known. Statistical techniques allow the experimenter to calculate the probability that results were due to chance rather than to treatment effects; multiplicity of cells, each with a large N, allows for greater generalizability; controls are placed on unwanted sources of variability. Great emphasis is placed on generalizability of results and ascertaining the degree of causality of particular problem-related variables. There are, however, some severe limitations to this model when emphasis is not placed on causal and generalizability criteria, but on achievement of results (behavior change) with a given individual. As discussed previously, problems of family planning and population change are a function of many idiosyncratic behaviors and events in a culture. Changing the behavior of individuals concerned with the problem requires careful selection of variables from the many possible sources of the problem. The goal is purely a technological or practical one: solving the problem, rather than developing a generalizable unilateral theory or explanation of the problem. Traditional analysis and evaluation models are typically concerned with group (generalizable) trends. It is quite possible that in a large group study, a statistically significant effect may be demonstrated when in fact the variable or treatment suggested is clinically senseless. While the effects are statistically generalizable, they do not enable prediction of how any particular subject would respond.

In a results-oriented context we are concerned with individuals achieving criteria and how this might be accomplished. To illustrate, in a results-oriented program we are concerned with whether or not an individual is engaging in birth control behaviors. If the desired result fails to occur, a different technique may be employed and so on until the desired effect does occur. Clearly, an individually and practically oriented analysis model is needed to progress systematically toward practical problem solutions. Applied behavior analysis, as the term connotes, characterizes such a methodology.

Thus, applied behavior analysis offers the following advantages for the family planning psychologist interested in actually changing the behavior of individuals (Thoresen, 1972):

1. The specific actions of the individual subject are the unit of focus, rather than comparisons between groups of individuals.

2. The frequency and magnitude of an individual's behavior can be examined continuously during all phases of the investigation.

3. The individual subject serves as his own control in that behaviors resulting from intervention are compared with his own baseline before intervention.

4. Control of variables related to the problem is facilitated by immediate and ongoing feedback of the individual's performance.

5. The clinician can determine immediately the extent of relevant changes continuously during treatment and other treatment if necessary, based on the data provided. The clinician is able to continuously alter treatment conditions as he strives to obtain criteria.

6. Typically, applied behavior analysis observation and analysis are employed in the settings in which the behaviors naturally occur. All data gathered are thus based on naturalistic conditions.

Applied behavior analysis methodology is particularly suited to results-oriented intervention. A basic observation and analysis procedure is employed to identify (*a*) problem behaviors, (*b*) appropriate behaviors, and (*c*) significant events that precede and follow these behaviors and tend to maintain them. After a behavior is targeted for change, the environment is rearranged to bring about this change. Data are kept continuously on the occurrence of the behavior. Any change that occurs is immediately observable. If no change occurs, then a new arrangement is designed. This process is repeated until the outcome criterion is attained.

Thus, applied behavior analysis strategies are designed to facilitate outcomes rather than to provide generalizable knowledge. Generalizability is usually obtained through controlled series of replications (Sidman, 1960).

Family Planning = Behavior Change

Family planning behavior change, in any culture, is individual behavior change. Procedures such as health education, accessibility, and exposure to information are simply not powerful enough to change behavior. If these procedures were effective, there would not be so much concern over the relative inefficacy of current preventive health programs. While "behavior change" and "education" seem to be redundant terms, the latter is notoriously lacking in operational attributes.

Each family brings to the family planning specialist a singular repertoire of rituals, interactional idiosyncrasies, individual family arrangements, and specific behavioral assets and liabilities that provide little justification for unilateral action, for example, different social skills, different scheduling habits, different responses to spouse criticism, different home spatial arrangements, and different significant people. While it is important to recognize the possible impact that incentives or motivators might have on family planning, in general it is more important now to identify what events from an array of incentives or motivators and what specific behavior in a given family should be focused on. Both the family planning counselor and the family should participate in this decision, thus increasing chances for success. A systematic methodology is available (*a*) for identifying behaviors and variables that might be brought to bear on them, (*b*) for altering the specified behavior in an appropriate direction, and (*c*) for evaluating program outcomes.

Applied Behavior Analysis and Family Planning Programs: A Suggested Direction

In this section of the article, two suggestions will emerge for the application of applied behavior analysis strategies to family planning and population change: (*a*) the development of a training program and materials for family planning or health workers and (*b*) the development of a taxonomy of relevant behaviors and contingency arrangements for a given culture and a taxonomy of behavior requisites for all existing birth control devices.

Family planning programs in newly developing countries are usually implemented by medically trained personnel at various levels. Training is typically in medical information about the various birth control devices, communication modalities, and health education. Indonesia's program, for example, employs mass motivation programs, family counseling, information dissemination, a speakers' bureau, entertainment, and family planning education as training components (Winslow, 1972).

This multifaceted approach to the problem of population change may be considered characteristic of other programs in India, Taiwan, Costa Rica, Pakistan, and the Philippines (Lozare, 1972; Ross et al., 1972; Stienborg, 1970; Wake Forest, 1971).

The effects of a behavior change technology have been demonstrated on a variety of behaviors related to health, social, and educational situations in several thousand empirically based studies (cf. Bandura, 1969; Franks, 1969; Katz & Klutnick, in press). Negative thoughts, hitting, tantrums, managing blood glucose levels, cigarette smoking, cardiac anxiety, obesity, and pill taking have been operationalized as behaviors that are subject to analysis and change. In addition, applied behavior analysis strategies have repeatedly demonstrated effectiveness in identifying, analyzing, and solving problems in families (Brown, 1972) by deliberate installation of new adaptive responses or by changing motivational factors (contingency arrangements).

Most relevant is the point that the technology demands no special theory or extensive training skills. Appropriate levels of skills can be acquired relatively quickly. A review of training applications of applied behavior analysis skills reveals successful training and application by parents, children, neighbors, nurses, physicians, and grandparents (Guerney, 1969).

Applied Behavior Analysis and Cultural Contexts

Furthermore, applied behavior analysis procedures are designed to analyze behaviors in their natural context, so these processes are generally culture free. Applied behavior analysis is a set of procedures, not a set of theories. Rather than imposing a set of theoretical biases on a given culture (e.g., interpreting Malaysian fertility procedures via Freudian theory), applied behavior analysis provides means for the analysis of the unique variables that control behavior across cultures. For example, a set of rural Malaysian parents may not practice any means of birth control, and this behavior may be maintained by certain covert reinforcing consequences. A large family may be functional in that culture by providing social and economic comfort for the parents in their declining years. A set of rural Canadian parents may not practice birth control, and this behavior may be maintained by an entirely different set of reinforcing consequences. A large family may be functional in the latter culture by providing immediate labor for the family's farm.

The antecedents or substantive events that set the occasion for intercourse may vary in different cultures and also lead us to an understanding of the problem behavior and how it might be altered. For example, in Malaysia where families may live together in one room, spontaneous intercourse (capitalizing on privacy) might be a powerful event antecedent to intercourse. In rural Canada the intemperate climate may generate certain affiliative social behaviors leading directly to intercourse during highly predictable time segments (the cliché about "long winter nights" might be appropriate here). Both overt and covert events and behaviors are part of the functional analysis of problem behaviors. The exact idiosyncratic event or behavior may vary from culture to culture, but the process of ascertaining them is quite the same. In each case such a problem analysis would suggest which events—antecedents or consequences—might be altered to produce changes in behavior.

Suggested Training Program Objectives

1. To develop an empirically based, outcome-oriented prototype model for delivering and maintaining family planning programs.
2. To train existing personnel in family planning and health in skills that will enable them to develop their own family planning or training programs.
3. To provide families with the opportunity to (*a*) generate empirical data on family planning, (*b*) participate in the decision-making process with the family planning worker, and (*c*) experience a greater degree of satisfaction and long-term gains from the family planning experience.
4. To provide a model for defining a societal problem that enables many personnel to participate in its solution.
5. To develop a set of training procedures, manuals, and materials relevant to the varied needs of a newly developing country, in the areas of basic behavioral analysis and change skills.

Training objectives can be designed for (*a*) trainers of family planning workers, (*b*) family workers, and (*c*) the consumer.

Suggested Training Objectives for Family Planning and Health Workers

1. Observation of behavior related to family planning problem:

1.1 Application of behavior observation skills;
1.2 Use of behavior observation forms;
1.3 Training families to observe their own behavior;
1.4 Identification of problem-related behavior and behavior required to solve problem.

2. Environmental analysis of problem-related behaviors:
   2.1 Identification of antecedents and consequences available for (*a*) the behavior to be changed and (*b*) the new behavior targeted;
   2.2 Selection of consequences and occasions (motivating environment) for the targeted behavior;
   2.3 Formulation of behavioral objectives for the family.
3. Application of behavior change strategies:
   3.1 Selection of appropriate change strategy: reinforcement, shaping, contracting, extinction, cuing, modeling, self-change.
4. Collect data.
5. Evaluate outcomes and modify program until behavioral objectives are achieved.

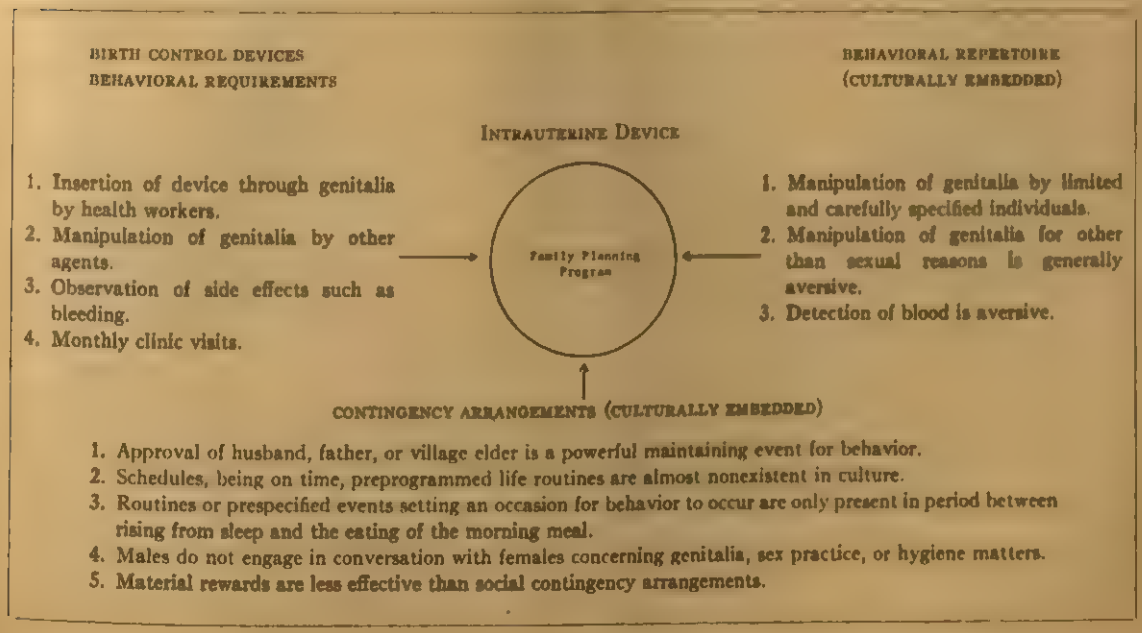
Prerequisites to the Application of Applied Behavior: A Behavioral Taxonomy of the Culture and of Applicable Birth Control Devices

Ralph Linton (1959) defined culture as "the way in which man behaves." Culture generates and supports behaviors, some of which are relevant to family planning. A culture generates consistent and predictable contingency arrangements as well as behaviors. Applied behavior analysis-based family planning programs must be behaviorally sensitive to the cultural context of a program. A behavioral taxonomy would identify all possible behaviors and contingency arrangements in a culture that might be relevant to family planning concerns.

In addition, every birth control device makes certain "behavioral demands" on its user. The chain of behaviors required for effective use of a condom is quite different than those required for oral contraceptives or an IUD (see display). Behavioral differences might relate to accessibility, self-management, spouse support, repetitiveness, contexts, etc. The development of effective family planning programs requires careful analysis both of the behaviors required for effective birth control device usage and those that members of the family are capable of performing. The interface of these two taxonomies can provide the family planning worker with a powerful decision-making base for the selection of behaviors and birth control devices for individual families.

A taxonomic manual for family planning can be developed, interfaced with an applied behavior analysis strategy and packaged in a format for ready employment by field workers.

Applied behavior analysis strategies are process based rather than problem based; that is, emphasis



is typically placed on applying a common process or technology across any problem behavior. Each new behavior entering into a problem definition does not demand a new set of analysis and change skills. A behavioral analysis might reveal that existing behaviors (life-style) in a Thai family might be capitalized on with the use of an IUD, rather than an oral contraceptive, and maintained with a culturally relevant incentive. In another family a behavioral analysis might reveal that a family's behavioral repertoire does not support an IUD, but is best capitalized on through the use of an oral contraceptive. In yet another family an oral contraceptive might be chosen, but because of severe behavioral deficits, the behaviors required to maintain effective usage must be installed and maintained. Behavior change programs for each family are developed with the same set of applied behavior analysis skills. Applied behavior analysis skills train workers to (*a*) define problems and solutions in terms of specific behaviors, (*b*) identify contingency arrangements that are functionally related to the desired behaviors, and (*c*) apply any or a combination of change strategies until program objectives have been achieved.

Thus the existence of certain cultural practices (behavioral repertoires) and the behavioral requirements of a given birth control device must be incorporated into planning for each family's needs. The use of applied behavior analysis strategies makes it possible to avoid subjecting individual families to unnecessary anxieties and obtrusive procedures in implementing a family planning program. Every birth control device, operationally speaking, makes certain behavioral demands on a family. Family planning workers, utilizing applied behavior analysis skills, can select a birth control procedure compatible with a family's behavioral repertoire or build a behavioral repertoire compatible with the existing birth control procedure. In either case, the happiness, stability, and integrity of the family may be maintained effectively while introducing such a radical change as family planning.

What seems most important is the point that potentially useful family planning programs may fail because science has not paid systematic attention to (*a*) the behavioral requirements of a given program and its interface with existing family behavioral repertoires and (*b*) effective methods to change family behaviors so as to increase usage of family satisfaction with birth control procedures. Rudimentary shibboleths prevail in this area, for example, "just increase accessibility," "better information dissemination," "educate the people." The most sophisticated birth control device is simply not good enough if people do not use it, do not use it correctly, or do not continue to use it. Helping families to employ appropriate family planning procedures requires a technology that is as scientifically based as that employed in developing the procedures themselves. Birth control devices are not soft drinks, soap powder, or cigarettes. They are a very personal experience for family members and inextricably related to pervasive and long-lived value systems. The linkage between many individual psychosocial and religious parameters and the use of devices may well be the individual behavioral analysis of the behaviors related to the problem and the solution.

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Animal Clinical Psychology

A Modest Proposal

DAVID S. TUBER *Ohio State University*
DAVID HOTHERSALL *Ohio State University*
VICTORIA L. VOITH *Ohio State University* [1]

Animals have been intimately associated with man for thousands of years. Although this relationship has been dictated primarily by utilitarian considerations, its character has been greatly influenced by a practical understanding of animal behavior. Attendant upon the domestication of animals as a source of food were the skills that enabled early man to share both labor and leisure with many other species. The dog, for example, has had a particularly protracted and unique relationship with man for almost 10,000 years. Trained for tracking, retrieving, herding, protection, and sport, the dog has also served as a companion. More exotic creatures such as the falcon and hawk were trained for hunting and sport as early as 3,000 years ago. The training of animals solely for the purposes of entertainment has been practiced for at least 4,000 years and has encompassed a wide variety of species both domestic and wild. Thus, the development of the necessary training skills has proceeded in the absence of any formal discipline of psychology and, for the most part, continues to exist apart from it.

Although a seemingly natural ally, the field of experimental psychology shares no common heritage and seldom recognizes or examines this body of independently developed knowledge. In fact, it has been only within the last two decades that the experimental psychologist has emerged into the area of applied animal psychology. In 1951, Keller and Marion Breland used the *American Psychologist* as a forum to enthusiastically announce the founding of a new area of applied animal psychology: commercial animal training. Stating that behavioral technology had now reached a level of development such that the efficiency of the new behavioral techniques would "outstrip old-time professional animal trainers [p. 202]," the Brelands envisioned an area of unlimited opportunities and financial benefits for experimental psychologists. Whereas the Breland's commercial venture, Animal Behavior Enterprises, has indeed flourished, a survey of modern dog training manuals reflects little modernization; the employment statistics for experimental psychologists with animal behavior backgrounds suggest even less improvement and much less hope (cf. the May 1972 issue of the *American Psychologist*).

Subsequent reports by other behavioral scientists have described the introduction of applied animal psychology into industrial and military settings. Skinner's (1960) experiences in training pigeons as organic missile control systems for the military are well known. More recently, Cumming (1966) and Verhave (1966) recounted similar experiences concerning the adoption of trained pigeons in industry for use as quality control inspectors. In all of the above cases, the quality of the technology in producing the desired behavior was successfully demonstrated. However, the reception by the military and the industrial reviewers was less than enthusiastic and the projects were terminated. Unfortunately, the optimism of the Brelands has not been borne out.

Ironically, experimental psychology has received acceptance in the extrapolation of behavioral principles to the modification of human behavior. Witness the current use of conditioning therapies in the treatment of phobic reactions (Wolpe, 1958) and in the treatment of retardates and more incorrigible mental disorders (Ulrich, Stachnik, & Mabry, 1966).

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Requests for reprints should be sent to David Hothersall, Laboratory of Comparative and Physiological Psychology, Ohio State University, 1314 Kinnear Road, Columbus, Ohio 43212.

Considering the outcomes of earlier ventures into the field of applied animal psychology, it is with guarded optimism and a tempered enthusiasm that we come before this readership to describe yet another endeavor: the application of psychological principles to the treatment of behavioral disorders in animals.

For the past two years, we have been engaged in this application of psychology. In this article we report on some of our efforts thus far and on the mechanics of a service which is functionally a clinical psychology for animals. All of our clients (pet owners) have been referred to us by veterinarians and, as such, have had their pets examined to ensure that the behavioral problem was not attributable to any underlying organic dysfunction. In most cases the origin of the behavior and the circumstances under which it first appeared are unknown or obscure. Typically, the problem has been allowed to persist untreated or has resisted treatment by more conventional means. Too frequently, we represent the final resort. Procedurally, we employ a team approach. Our various backgrounds (Hothersall: operant conditioning; Tuber: classical conditioning; Voith: veterinary medicine and conventional training techniques) are combined in the analysis of the behavioral problem, and a training program is developed that often represents a synthesis of these skills. We have found that the most efficient approach is to utilize the owner as the agent of change and the home as the training environment. Consequently, a considerable effort is concentrated on translating the program into an understandable terminology, devising practical analogies to a laboratory situation, and counseling the owner in the implementation of the program. When the owner contacts us concerning an appointment, he is requested to maintain a daily log detailing relevant information regarding the behavioral problem, the circumstances surrounding its occurrence, and any additional information that might be pertinent. The initial interview, attended by the pet and as many family members as practical, is used for obtaining a thorough case history, understanding the domestic environment into which the training program must be interjected, and providing an opportunity to observe the family–pet interactions. At a subsequent session, the owner or the person responsible for the training is presented with a written training program which elaborates on the rationale and describes in detail all relevant manipulations. This is then discussed and may even be rehearsed with the owner. In addition, we provide daily training protocols and behavior records which are to be filled out by the owner as the training progresses. These are useful as training guides for the owner and are indispensable to any evaluation of the effectiveness of the intervention. They may merely confirm a success or identify the source of difficulty, hence affording the basis for a knowledgeable adjustment in either the program or the approach to the problem. All training programs are routinely evaluated with the clients after approximately three weeks of training.

The following case summaries describe the essential points of several pet problems that we have encountered; they are presented to characterize the flavor of our approach and the mechanics of our service.

Higgins

Higgins is an affable four-year-old Old English Sheep Dog of Goliath proportions whose tranquil demeanor was breached only by an intense fear of thunderstorms. At the first indication of an impending storm, Higgins would begin an accelerating pattern of aimless pacing, profuse salivation, and marked panting which was rapidly climaxed by the hurtling of his 110-pound body against any obstacle in a futile attempt to escape. A variety of confinement procedures and tranquilizers had been tried without success, and the behavior had continued unabated for two years. The crisis that threatened the relationship between Higgins and his petite owner occurred when they were forced to share the owner's foreign sportscar during a thunderstorm.

Since the fear was elicited by an identifiable and reproducible stimulus, a course of therapy involving counterconditioning in the fashion of Jones (1924) and Wolpe (1958) seemed most appropriate. Having satisfied ourselves that a stereophonic reproduction of a thunderstorm would elicit the fear—Higgins actually overwhelmed the three people in the room with him—the stimulus situation was presented in graded steps along the dimension of intensity. Beginning at an intensity that did not evoke the fear, Higgins was brought under command to lie down, and the subsequent occurrence of thunder was paired with a food reward of chocolate bars. As long as Higgins remained calm and maintained the antagonistic posture, the intensity of the recreated, artificial storm was gradually increased. Training was initiated in the laboratory

in daily sessions lasting one hour: The beginning intensity for each session was always slightly less than the terminal level achieved during the preceding session. We had been proceeding gradually with few difficulties; over the course of the first five conditioning sessions we had progressed from a thunder intensity level of a meager 35 decibels to that of a resounding 75 decibels when a typical summer thunderstorm intervened to test our efforts. Happily, the owner reported that Higgins initially exhibited only a mild version of the original fear response, that he was easily brought under control, and that she was able to successfully practice the training as we had done in the laboratory. Subsequent sessions in the home with artificial and real storms have succeeded in the further attenuation of the fear response. Realistically, we do not expect Higgins to ever seek pleasure in thunderstorms, but the change in his behavior has been striking and is at least compatible with his surroundings.

Hookie

A more difficult class of problems is concerned with the interruption of established misbehaviors that occur only when the pet is left alone. The behavior appears related inextricably to the absence of the owner, and consequently, any direct control over the behavior is precluded by the absence.

Hookie is an 18-month-old male Afghan whose owner necessarily left him alone for extended periods of time. Upon his return, the owner would find chewed clothing and furniture, a distribution of feces which was described as pernicious, and Hookie, wearing the appropriate expression of shame. The conventional remedy of confinement merely restricted the range of destruction. The punishment, which was necessarily delayed, proved ineffective. The initial interview, supplemented by a daily baseline log, suggested that appropriate manipulations of the prevailing stimuli would modify the behavior considerably. Preparatory to prolonged absences, during which destructive behavior invariably occurred, the owner engaged in an elaborate pattern of ritualistic acts that were intended to secure the house from without as well as from within. No such precautions were required either by absences of short duration or by Hookie's behavior during such absences. Therefore, a necessary part of the training program was to degrade the validity of both contextual and temporal stimuli associated with prolonged absences. By introducing a stable, novel stimulus into the training situation, we provided a manageable cue to which acceptable behavior could become associated and could easily be maintained by training. Further, this training would provide a background against which we could extinguish the undesirable behavior. The new stimulus for Hookie's training was provided by a radio tuned to a local rock station which already had a strong association with the presence of the owner. The new response was simply any behavior other than that of the destructive variety.

Conceptually, the strategy is the expression of the operant reinforcement schedule differential reinforcement of other behavior under the control of a discriminative stimulus. Viewed as a transfer paradigm, this association between a new stimulus and new response has been shown by Wickens (1973, pp. 229–231) with cats to be immune to any proactive effects of a prior association. But more remarkable, such a new response appears to have deleterious retroactive effects in that it displaces the response of such a prior association over a long retention interval.

The mechanics of the training were relatively simple. A schedule of training absences was devised such that as training progressed, the average duration of the absences was gradually and variably prolonged. The initial absences were by design very short in order to minimize the occurrence of any misbehavior and to maximize the probability of intercepting and punishing it. During all absences the new stimulus was always present. Later in training, the cues formerly associated with extended absences were systematically introduced into the training situation. Good behavior on Hookie's part following an absence was always reinforced by the owner with an effusive greeting and a brief play session. Misbehavior was to be punished by a stern reprimand followed by a retreat to remedial training at shorter absences with only the novel stimulus. The owner laboriously followed our suggested training schedules, which often required two training absences each day accompanied by tedious cue manipulations. He was duly rewarded for his efforts. In the intervening four weeks between the final interview and the training evaluation, Hookie had violated the house only once despite the occurrence of several unplanned, extended absences by his owner. Accordingly, the training could safely be reduced to a maintenance schedule of several sessions per week.

Sandy and Jenny

A situation in which the determinants of an undesirable behavior were inherent in the very nature of unavoidable, energetic contact was resolved by the unique modification of the interaction between a small terrier, Sandy, and a three-year-old child, Jenny. Sandy nipped Jenny regularly during play and was becoming increasingly sensitive to all play such that the nipping was beginning to generalize to other children. The interview, which was attended by the parents, the child, and the dog, confirmed that the nipping was elicited by the normal excesses and unsophisticated style of the play itself. A training program was developed which was modeled after the children's game Simon Says. It involved teaching Jenny how to pet an inanimate object—gentle openhanded petting being rewarded with candy. Independently, Sandy was taught to sit on command for a reward of a slice of hotdog. When both were trained satisfactorily, they were brought together and the games were combined. Sandy was required to sit; Jenny was requested to approach Sandy and gently pet him only once. Both were then rewarded immediately. As the training progressed, the game became slightly more entailed in that Jenny was required to pet an ever-increasing number of different areas prior to obtaining her reward, and Sandy, by default, had to tolerate the ever-increasing amount of petting prior to gaining his reward.

The parent was the moderator of the game, the judge of correct responses, and the dispenser of the rewards. Progression to longer and longer schedules of interaction was determined entirely by the comfort and skill of the participants, which were carefully monitored and adjusted by the parent.

The game proved completely effective in eliminating the nipping, and we have suggested that an abbreviated version of the game should be practiced to prevent the recovery of the old play patterns. In addition, we suggested that the game be used as a vehicle for introducing new children to Sandy and ensuring their skill in handling the dog.

Cautions, Comments, and Other Considerations

Transporting the principles and methodologies of a behavioral science beyond the walls of the laboratory is a formidable challenge: refreshing, often rewarding, frustrating, and occasionally intimidating. We have experienced a form of culture shock. Gone are the controlled environments of the experimental chambers and the efficiency afforded by the most fundamental of instrumentations. Absent too are the security offered by group means and the closure provided by the control group.

Whereas in the laboratory one's research methodology is dictated primarily by the problem under investigation, the methodology selected by the animal clinical psychologist is disproportionately influenced by the sensitivities of the owner and his demands, the tenor and routine of an ongoing domestic situation, the physical surroundings, and the pet's relationship to all of these variables. The applied animal psychologist must be aware of, and be particularly adept at incorporating, just those variables that a good research strategy seeks to render ineffectual. What may be the treatment of choice may not be the treatment of convenience—and it is the latter that often determines whether or not a training program, no matter how ingeniously conceived, will be palatable to and consequently implemented by the owner.

Discovering inexpensive analogies of laboratory apparatus demands a similar ingenuity which leads one to explore the shelves of hardware stores and novelty shops. One quickly learns to appreciate the merits of a child's cricket when a conditioned reinforcer is desired. Stereophonic sound effects recordings are another resource when artificially reproducible stimuli are required by a training program.

To all of the preceding must be added the inescapable reality of economics. Overhead expenses dictate fees; fees mean ledgers, billing, and collection. Too often, the fees will be weighed against the cost of euthanasia—a very real consideration for some owners and consequently for the practitioner who must necessarily consider the pet as his primary responsibility. Fees also expose the experimental psychologist to a variety of agonies totally foreign to his academic background which has in fact shielded him or otherwise rendered him contraprepared. There are those clients who expect no fewer guarantees from their pet's psychologist than from their television serviceman, and more than from their family physician. The practitioner must learn by experience to expand the limits of his sensibilities. Fortunately, most clients are concerned, understanding, and often knowledgeably sophisticated about their animals and generally delight in them. Their acceptance of your efforts and

their willingness to pursue a sometimes arduous training program are mutually rewarding. The foregoing has been offered, not as a deterrent, but as a caution which must be given thoughtful consideration by any prospective practitioner.

There are an estimated 40 million dogs kept as pets in this country. Although the relative frequency of serious behavior problems is not yet determined, the responses from veterinarians in this area have been encouraging. It does appear that a need for such a service exists and that it is a legitimate extension of experimental psychology. Whether or not such a need can be satisfied by an applied area of animal psychology would seem to be determined largely by the efficacy of our technology and willingness of interested students to pursue such a career. The efficacy of the technology resides partially in the perceptiveness of the applied psychologist, but to a greater extent to the depth of his ties not only to the basic scientist but to his counterpart in the human clinic as well. There is a pleasing irony surrounding the adoption of a therapeutic treatment for a dog which was realized for use with human patients, based on principles generated by experimentation on dogs. The evolution of ideas, reflected in our treatment of Higgins, has been circuitous but advantageous.

We have already alluded to other contributions by the basic scientist to the applied area, but it is important to point out that the advantages of such an alliance are mutual. An example is the article "Misbehavior of Organisms" (Breland & Breland, 1961) which has emerged as a crucial element in the current reexamination of learning theory (Bolles, 1972; Hinde & Stevenson-Hinde, 1973; Seligman, 1970).

Ancillary but very pragmatic considerations have been suggested by Little (1972) regarding remedial measures precipitated by the dismal employment prospects for psychologists. The vulnerability of comparative and experimental psychologists within the academic marketplace might well be attenuated by an area of animal clinical psychology. By expanding the employment opportunities at both the master's and doctoral levels, it would provide a means of maintaining a viable training program without resorting to a reduction in enrollments. In addition, it could also expand the traditional academic roles of psychologists to include schools of veterinary medicine.

We are aware of at least three other groups of psychologists and veterinarians who are currently engaged in behavioral applications similar to our own. Possibly there are more, and we hope that this article will initiate an exchange of information and communication between such groups. We are prepared to act as a clearinghouse for such needed interactions.

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Eclecticism at Work

Approaches to Job Design

B. E. SANDLER *Center for Creative Leadership, Greensboro, North Carolina* [1]

The nature of work in America is in the public eye. A great deal of attention has been focused on such things as job design, the quality of working life, job satisfaction, and productivity. An unprecedented opportunity exists for behavioral scientists to apply their skills to seeking solutions to the problems of industry. However, the opportunity is not without its perils. There are two primary dangers: first, that we will limit ourselves to using the approaches and techniques with which we are familiar; second, that we will use them in situations in which they are inappropriate. If we fall into either of these traps, we will fail to capitalize on the opportunities that exist to extend the limits of our knowledge and skills.

Job design has been a major focus of the more general concern about work. Numerous aspects of job design have been advocated, implemented, and reviewed (Davis & Taylor, 1972; Maher, 1971; Sandler, 1974). The coverage given many job design efforts by the popular press, media, and the professional literature is reminiscent of approximately 20 years ago. *Plus ca change, plus c'est la meme chose.*

Certainly some things have changed, and in these changes lies hope. The changes, I believe, lie in the business climate—not within psychology. Changes in the international balance of payments, the industrial output of other nations, and the stridency of today's workers have made businessmen more aware of the need to find solutions to the problems facing them and more receptive to innovation.

Job Enlargement

Job enlargement, a term I am using in a generic sense to include enlargement, enrichment, and participative decision making,[2] has been a major approach to job design. Proponents of job enlargement see it as a way of reducing the high degree of specialization brought about by scientific management (Taylor, 1947). It is felt that reducing the amount of specialization will make the work more meaningful and satisfying and may result in desirable changes in productivity, quality, and other variables of interest.

Because job enlargement is typically presented as an approach of broad applicability, the question that must be asked is, To what extent can it be used to come to grips with the difficulties faced by American industry?

Job enlargement has been useful in some settings. Ford (1969), for example, described the use of job enlargement with more than 1,000 Bell System employees in 10 companies. Experimental and control groups were used in all but one instance. Four indexes of the effect of the enlargement projects were used: (*a*) technical measures of performance; (*b*) employee attitudes as determined by turnover, grievances, and questionnaires; (*c*) customer attitudes; and (*d*) manager's attitudes as determined by desire to drop or expand the project, or move the project to a permanent status. The results were generally positive, although one trial produced no change. Increases in the quality and quantity of work and reductions in turnover were common.

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Requests for reprints should be sent to B. E. Sandler, Center for Creative Leadership, 5000 Laurinda Drive, P.O. Box P-1, Greensboro, North Carolina 27402.

[2] One way of enlarging jobs is to increase the size of the job in terms of the number of operations performed. Another method is to permit the employee to take a part in decisions that affect him or his job. While these are different methods, the practical effect of both is to enlarge the province of things with which the employee is concerned. In this sense, participative decision making is a form of job enlargement.

The reactions of employees and managers to the job enlargement projects were, in general, positive.

In a study designed to determine the generality of job enlargement, Paul, Robertson, and Herzberg (1969) studied laboratory technicians, sales representatives, design engineers, and factory supervisors in a British company. As determined by various measures, job enlargement was applied successfully to all of these groups. Paul et al. concluded that job enlargement is a generalizable concept and is probably feasible in all settings. Furthermore, they felt that the gains from job enlargement are significant and lie primarily in the area of job performance.

However, Hulin and Blood (1968) believe the case for job enlargement has been overstated. After a review of the literature, they concluded that

> the argument for larger jobs as a means of motivating workers, decreasing boredom and dissatisfaction, and increasing attendance and productivity is valid only when applied to certain segments of the work force . . . [p. 50].

While this position has been questioned (e.g., Shepard, 1970), the fact that industry continues to focus on job enlargement indicates that it is alive and well, in spite of inconclusive data.

Although more data are needed for a better understanding of the manner in which psychological and sociological factors affect workers' responses to job enlargement, one suspects that the limitations on applicability imposed by these factors are relatively minor compared to those imposed by technical and economic variables. Levitan and Johnson (1973) discussed the way these factors may limit the ability to alter jobs. They pointed out that technology is an important factor in determining job design and emphasized that changes which necessitate the replacement of expensive capital equipment are unlikely to be made.

Two of the major concerns regarding job enlargement are that it may reduce productivity and increase costs. While these concerns have not been problems in the reports that have appeared (e.g., Ford, 1969; Paul et al., 1969), one suspects that cases in which problems have arisen may not have been reported.

The attitudes of managers are another variable affecting the implementation of job enlargement programs. Bucklow (1966) indicated that still another impediment to the implementation of job enlargement is the "unquestioned acceptance of the methods and assumptions of scientific management and the traditional management theorists [p. 74]."

In view of the limitations that technical, economic, and attitudinal factors pose to broad-based programs of job enlargement, it is necessary to investigate other approaches. We must work to conceptualize the problem in ways that promise more broadly applicable solutions to the problems of industry.

Modified Workweeks

Modifications of the standard five-day workweek have been initiated by a number of diverse firms. The response to the modified workweek has been generally positive—on the part of both employees and management. The modified workweek appeals to management because it leads to reduction in the cost of doing business, either through increased production, decreased turnover, decreases in start-up and stop time, increased motivation, as well as other variables as yet to be identified.

The modified workweek appeals to employees because it redistributes their free time into more meaningful patterns. Although an employee may work the same number of hours under a modified schedule (say 40 hours) as he did previously, he is now doing it in fewer days (say 4 instead of 5). The result is an effective increase of time at essentially no cost to the worker. The redistribution of time into more meaningful groupings comprises the essence of the modified workweek.

Many of the reports that have appeared have been merely descriptions of the modified workweeks, summaries of the experiences of companies on it, and methods for converting to it (e.g., Hellriegel, 1972; Poor, 1970). In a longitudinal study of worker adjustment to the four-day workweek, Nord and Costigan (1973) found that overall attitudes to the four-day workweek were highly favorable. Absenteeism decreased following the introduction of the modified schedule, and then decreased more by the end of the study. Differential effects of the four-day workweek were found to be associated with age, plans for the use of nonwork time, and job pace.

Clearly, the modified workweek can result in significant gains for all concerned. The questions that must be considered are, How broadly applicable is the modified workweek? and What is the long-range outlook for it?

The modified workweek has broad applicability. Modifications can be made, at least in theory, in the workweek of virtually all employed persons. Of

course, there is a gulf between theory and practice. For example, labor unions have been staunchly opposed to modifications of the workweek that result in an increase in the length of the workday. Thus, the 4-day 40-hour schedule has been opposed by most unions. Whether union resistance represents a long-term problem or merely a temporary restraint to alterations in the workweek remains to be seen.

Another impediment to the modification of the workweek is the existence of laws, both state and federal, which regulate hours. These laws, and the bureaucratic procedures necessary to demonstrate compliance, are enough to cool the ardor of all but the bravest (or most troubled) innovator. These laws may restrict the speed with which modified workweeks are adopted. However, as the modified workweek gains in popularity—and every indication is that it will continue to do so—increased pressure for modification of restrictive legislation can be expected.

If the modified workweek is inevitable, the most important question is, What are the long-range prospects of it in terms of employee satisfaction, motivation, production, and other variables currently being affected by changes to modified work schedules? Clearly, it is only possible to speculate.

When the modified workweek becomes the norm, it will, by definition, no longer be novel. When almost everyone is working a schedule that results in more meaningful groupings of free time, the perceived value of the schedule will likely decline for all. It is hypothesized that one of the values of the modified schedule is that it affords the individual who is on it prestige, status, and recognition (relatedness needs in Alderfer's, 1972, ERG—existence, relatedness, growth—theory). These values, in addition to the more effective time distribution, contribute to the effectiveness of the modified schedule. With increased adoption of modified schedules, these relatedness needs will no longer be satisfied by the schedule per se. Hence, one could predict an increase in turnover, absenteeism, decreased production, etc. As modified workweeks of all sorts become more available, one of the important barriers to employees leaving a firm operating on a modified schedule is also removed, since a position with another firm on a modified schedule is relatively easy to find. That is, it is hypothesized that as modified workweeks become more widely available, they will become increasingly less effective, but more necessary.

Modified work schedules—modified in the direction of greater flexibility and a more useful distribution of leisure time—are probably destined to replace the now standard five-day schedule. If our hypothesis is correct, they will lose some of their effectiveness as motivational tools in the process. They may, however, serve to make work, especially unpleasant work, more palatable by reducing the amount of "psychological time" that must be devoted to it. This possibility needs to be explored and tested.

The Problem Solver and the Problem

The question of how to achieve a broadly applicable solution to the problems facing industry still remains. The methods discussed above are useful, but have limited applicability either spatially or temporally. Where do the answers lie?

Discussing potential solutions is premature without first discussing the problem solver (the psychologist or the executive) and the problem. Campbell (1969) discussed the former. He distinguished two types of individuals. The first, referred to by Campbell as the *trapped administrator*, has committed himself in advance to the efficacy of the solution and, consequently, is committed to it. The *experimental administrator* (or psychologist), however, has justified the solution on the basis of the importance of the problem. As a result, he is committed to seek other solutions if the first one fails. Thus, our first recommendation is that in seeking solutions to the problems of industry, one would do well to attempt to emulate Campbell's (1969) experimental administrator.

Perhaps the most difficult part of implementing an effective solution is finding a fruitful problem. The importance of problem finding and problem formulation is gaining increasing attention (Csikszentmihalyi & Getzels, 1971; Getzels & Csikszentmihalyi, 1974; McGuire, 1973; Taylor, 1972). There are problems and there are problems, and the goodness of the solution is related to the goodness of the problem that has been selected.

One method of developing problems is through the process of transformation (Taylor, 1972):

> It can be asserted that only simple problems lend themselves to direct solution processes, and that complex problems, if they could be solved directly, would already have been solved. Complex problems require complex solution process, or processes which transform these problems into forms that allow for solution. Transformation processes as an approach to rendering problems soluble include reversals,

lateral thinking, contrasting, analogy, metaphor, transpositions, and remote associations [p. 11].

Our lack of success in dealing with the troubles of industry may stem from our failure to transform the problems as they are seen and presented by management into forms that allow for solution. For instance, the problem of monotonous and repetitive jobs has long troubled industry and industrial psychologists. Solutions have been attempted by dealing with the problem directly and trying to modify the nature of the job. Perhaps more fruitful solutions would emerge if the problem were stated differently, thus allowing for unrecognized solutions as a result of the transformation.

The problem of repetitive and monotonous jobs can be transformed to one of the perceived speed with which time seems to pass. That is, the job is seen as boring because too long a period of time is spent doing it. Since time passes slowly, the undesirable features of the job are amplified psychologically, and their negative valence affects the entire job. If the speed with which time *seemed* to pass could be increased, these difficulties might be overcome. Monotony and repetitiveness may be important variables affecting the perceived passage of time. However, research might discover additional variables that could be manipulated effectively to affect the perceived passage of time. The focus of the problem is no longer on changing the nature of the job but on the variables that affect the passage of time. This transformation may result in a form of the problem that allows for a more fruitful solution. If not, other transformations and other solutions must be tried (à la Campbell, 1969).

Toward a Psychology of Responsiveness

Increasingly our society is becoming more permissive with regard to the freedom of the individual to meet his needs in the manner most appropriate and satisfying to him. This permissiveness, embodied in the colloquialisms "do your own thing" and "whatever turns you on," has found expression in many aspects of life. Work has been one of the notable exceptions. Work has remained a last outpost of rigid conformity to an arbitrary, and perhaps sacrosanct, set of standards. The assumptions underlying the standards are never questioned. It matters little whether the assumptions are those of Theory X or Theory Y (McGregor, 1960). Proceeding on the basis of either theory is bound to ignore the vast body of data regarding individual differences. No unitary approach can successfully address itself to the full range of variation represented by the employees of a given business—even a small one. But, while an organization cannot afford to function in a manner designed to be all things to all men, it is possible for organizations to be more flexible and responsive to the needs of their members.

The need for a variety of executive compensation plans, and flexibility in modifying and implementing those plans, has long been recognized. The assumption has been that the needs of the executive should be a factor in determining the nature of the compensation package, and that the investment made by the organization in the individual's compensation package would be realized through effective performance. These are reasonable assumptions—for the executive as well as for the man on the line. The possibilities available as a means of increasing the responsiveness of the organization to the needs of its members are limited only by the scope of those needs and the willingness of the organization to innovate.

The approach that promises the greatest amount of responsiveness to employee needs (as determined *by the employee himself*) is what may be described as a cafeteria approach, in which the employee selects from a number of alternatives those most appropriate for him. Certainly, the nature and number of alternatives offered would have to be based on an analysis of probable costs and benefits. However, in the absence of contradictory evidence, there is no reason why the cafeteria approach to employee need satisfaction could not prove as practical as the cafeteria approach to food service.

Adams and Solaski (1964) investigated an aspect of the cafeteria approach to employee need satisfaction in a study of the psychological value of compensation increase options. The options used in the study are as follows:

OPTIONS WITH A COST OF ABOUT $600 TO COMPANY [3]

1. $600 salary increase; no vacation increase; no increase in company contribution to employee savings plan.
2. No salary increase; 12 additional days of vacation; no increase in company contribution to employee savings plan.

[3] These items are adapted from *The Psychological Value of Compensation Increases Options among Exempt Salaried Employees* by J. S. Adams & R. R. Solaski, General Electric Behavioral Research Service, New York, 1964.

3. No salary increase; no vacation increase; increase in company contribution to employee savings plan from 3% to 9%.

4. $300 salary increase; 6 days vacation increase; no increase in company contribution to employee savings plan.

5. $300 salary increase; no vacation increase; increase in company contribution to employee savings plan from 3% to 6%.

6. No salary increase; 6 additional days of vacation; increase in company contribution to employee savings plan from 3% to 6%.

Each participant in the study was told to assume he would get a raise in the form of one of these six options and was asked to choose among them. Adams and Solaski then determined the psychological value of each alternative and compared it with that alternative's real value and cost to the company. Only one option—$300 plus 3% savings—had a psychological value greater than its cost to the company. But the ratio of psychological value to estimated cost to the company indicated two additional alternatives that were more attractive to the employee and the company than a straight salary increase. These were $300 plus 6 days of vacation, and 6 days of vacation plus 3% savings.

Comparison of the psychological value of single components, the sum of these components, and the psychological value of the combinations, indicated that the value of the combinations was greater than the sum of their parts. That is, the psychological value of $300 plus 6 days of vacation was $97 greater than the sum of its components. The psychological value of $300 plus 3% savings, and of 6 days of vacation plus 3% savings, exceeded the sum of their components by $96 and $81, respectively:

> The increased value of combinations over what naturally might be expected is substantial. As a percentage of the expected value, the psychological value of the optional combinations is *17 to 20 percent greater*. The exact reason for this significant interaction of option components is not known, but a likely explanation is that an option containing more than one form of compensation is more apt to satisfy individual needs since individuals have several needs [Adams & Solaski, 1964, p. 11].

Adams and Solaski's (1964) work has been considered in some detail because it is one of the few studies available addressing itself to the cafeteria approach to employee need satisfaction. (Also see Adams, 1962; Lawler, 1971; Mahoney, 1964; Nealy, 1964; Nealy & Goodale, 1967; Shuster, 1969.) Adams and Solaski's data are suggestive in that they appear to indicate that organizations may be able to make more effective use of their employee compensation resources by increasing the flexibility with which those resources are utilized.

The possibilities in extending the cafeteria approach to other aspects of the organization are exciting and may hold the promise of increased satisfaction for both the organization and its employees.

One possible aspect of the cafeteria approach, which utilizes aspects of the modified workweek as well, is the introduction of flexible working hours (Bolton, 1971). To date, this approach has been used more widely in Europe than in the United States. A recent article in the *Wall Street Journal* (Wong, 1973) reported the experiences of a number of American firms with flexible working hours. The essence of the approach is that it permits the employee to select the hours he will work, as long as he abides by certain rules regarding earliest and latest starting and quitting times and total hours worked. Certain plans permit employees to work a greater or lesser number of hours during a given period, with a resulting credit due them (in terms of time off) or debt owed by them (in terms of additional work time). The experiences of the firms interviewed by the *Wall Street Journal* indicate the use of flexible working hours appears to be a great success in terms of employee satisfaction and reductions in tardiness and absenteeism. Moreover, it represents some data concerning the feasibility of using a form of cafeteria approach in a variety of different settings.

There is reason to believe that the cafeteria approach could be broadened to include the work patterns of the employee. To do this it would be necessary to develop a number of alternative work patterns (data collected from the employees in a manner similar to that used by Adams and Solaski, 1964, would be useful in this regard) and then let the employees select the alternatives most appropriate to them. In a paper entitled "The Work Module: A Proposal for the Humanization of Work," Kahn[4] developed a type of cafeteria approach to organizational design. Lawler (1973) also discussed the issues inherent in recognizing and accepting the existence of individual differences in the work place and their implications for a cafeteria approach to organizational design.

The nature of this approach can be illustrated

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[4] An unpublished paper by Robert Kahn, entitled "The Work Module: A Proposal for the Humanization of Work," may also be viewed as representing another version of the cafeteria approach. It is available from the Survey Research Center, University of Michigan.

using the differences between job enlargement and the unenlarged job. When a job enlargement program is put into operation, it is not unusual to find a subset of the work force who would prefer to perform the repetitious, unenlarged, familiar job (Argyris, 1973). Their reasons for this might range from resistance to change to enjoyment of the repetitiveness of the function they are performing. While the reasons for the differing preferences may be interesting, and perhaps worthy of study in their own right, they need not concern us. Rather they can be taken as an indication that different individuals see different types of activities as most satisfying to them. If the legitimacy of this is accepted—and we do not feel a missionary zeal to proselytize for what we "know" to be "right"—the implication is that we should establish two alternatives or options. One option would utilize a job enlargement approach; the other would retain the previous job design. If the system is also designed to permit and encourage flexibility (perhaps evolution would be a better term) we might find people switching from one option to the other as their needs, or their perception of the most effective way to satisfy them, change. As this occurs, the employees could play a role in the development of new alternatives, thereby helping the organization evolve. This would represent the stage that Land (1973) referred to as *mutual growth*.

In evolving toward greater mutual growth, the organization must continue to recognize the legitimacy of patterns of behavior and need satisfaction with which it might disagree. It should not sit as an arbiter of behavior through, for example, implicitly supporting the Protestant ethic. Rather, it must permit and encourage its employees to grow in the manner most appropriate and satisfying to them, with the only *quid pro quo* being certain specified criteria of performance.

The process through which this goal is achieved will necessarily be one of successive approximations. Job enlargement, participative management, modified workweeks, and flexible hours can be viewed as intermediate stages. We can only speculate as to what forms the alternatives that could be established might take. However, the speculation is interesting, and fruitful hypotheses can be derived from it.

For example, an organization might establish three alternative work patterns. Option 1 might be for those individuals who are interested only in putting in a given number of hours in return for their pay, with no desire for advancement. Salary increases might be given at periodic intervals, contingent upon certain specified criteria, until a maximum is reached. Nothing is expected of these individuals, either explicitly or implicitly, except a specified level of performance. Option 2 may differ from Option 1 only in that it utilizes a modified workweek of some sort. Option 3 could be established specifically for those individuals who desire to advance to supervisory positions. While the entry work (and wages) for this option might be identical with Options 1 and 2, it would be known to all employees that supervisory personnel would be selected from among the employees choosing Option 3.

Other options could also be developed. These might make use of job enlargement, participative management, performance contracting, or a host of other alternatives. It is not my intention to attempt to develop a list of feasible options, but rather to consider the merits of the approach.

In a given organization, a substantial number of employees might choose Options 1 and 3. In doing so they would be responding to the alternatives in a manner calculated to either maximize or optimize their personal need satisfaction. Under these circumstances the organization is set up to encourage such behavior. Usually, however, the organization discourages this type of behavior because there is only one work pattern and everyone is required to fit the pattern at some minimally acceptable level or leave the organization. Certainly no one is naive enough to assume that employees will not engage in attempts to maximize personal need satisfaction just because the organization may discourage them from doing so. It is likely that the result will be either covert attempts at constructive satisfaction, or covert attempts at destructive satisfaction through increased waste, downtime, sabotage, etc.

The employee who wishes to "look good" and wants to advance is also in a difficult situation. Improved performance on his part is likely to violate unspoken understandings regarding productivity, and peer group pressures will be mobilized to return him to the fold. If he succeeds in resisting these pressures, he is likely to be ostracized from the group. If he subsequently is given supervisory responsibility, this is likely to make his position considerably more difficult.

It can be hypothesized that the present work pattern which throws together people with vastly differing needs and aspirations may exacerbate con-

flict between them and serve to limit the effectiveness of their performance. On the other hand, a cafeteria approach which results in a relatively homogeneous grouping of people according to their needs and aspirations may reduce conflict and remove some of the factors limiting effective performance.

Implementing a multiple track system might prove difficult, at least initially. Designing in the type of flexibility that would permit an employee to switch tracks as his circumstances, needs, or perceptions changed would also be difficult. It may be easier for organizations with a number of separate locations to implement this system than would be the case for a small company with only one plant. The approach is probably applicable to both. Establishing this type of diversity across organizations might be reasonably effective in better meeting the needs of employees, but not in meeting those of the organization. To meet both individual and organizational needs, the diversity must exist within an organization. However, as organizations develop and implement more effective ways of meeting employee needs, they are likely to be rewarded with the type of improved performance for which they have been searching.

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Statistics

Tool or Master of the Psychologist?

ANTHONY SIGNORELLI *Union College* [1]

Psychologists, when discussing the historical development of their endeavor, frequently note its roots in philosophy and as frequently proclaim that it is gradually attempting to emulate the physical sciences in divorcing itself from this parent by becoming more quantitatively and qualitatively precise. That a science of psychology is progressively evolving is evidenced, it is often claimed, by the fact that it has increasingly embraced a mathematical tool—statistics—for use in evaluating its postulates. What is overlooked is the fact that the use of mathematical tools in the development of the physical sciences bears little resemblance to the use of statistics in psychology. Statistical procedure has influenced theory, the direction of research, and instrumentation in psychology to an extent and in a manner foreign to the influence of mathematics on the physical sciences.

While it is true that many of the postulates of prominent theories in psychology were not based on statistical evidence, it is becoming increasingly popular to construct theories based on, and embodying, statistical formulation (e.g., Estes' learning theory, Feigenbaum and Simon's explanation of the serial position curve, and John's hypothesis of memory storage). In addition to theorizing statistically, attempts to prove theories or to rebuke their alternatives are relying on statistical design with growing frequency. Perhaps this preoccupation with statistical procedure has invaded experimental psychology more pervasively than any other division of psychology, although no division is uncontaminated.

A common procedure of the experimentalist is to apply a treatment—which a theory suggests or which he believes, at the suggestion of the theory, will influence behavior in some manner—to a group of subjects and compare the results of their performance against the performance of subjects not receiving the treatment. The comparison is a statistical one that measures the sufficiency of the difference between the numerical averages obtained from the two groups to determine if it is due to chance or treatment.

This "test of significance" procedure, which has become the keystone of experimental research design, has been criticized by several authors (Bakan, 1966; Bolles, 1962; Edwards, 1965; Meehl, 1967; Rozeboom, 1960). The procedure is founded on the belief that since the groups to be compared are randomly selected, any chance, unknown, or unmeasurable factors that may influence differences in performance will be equally distributed between the two groups and will thus be nullified when the comparison is computed. Any difference that results can then be attributed to the treatment under study. The main thrust of the criticism of the above authors was against the acceptance of this "null hypothesis" syndrome. Bakan (1966) noted that the probability of the null hypothesis occurring is extremely small under any circumstances. If, in fact, all extraneous factors were controlled by random sampling, then one should expect no subject in the control group to demonstrate behavior of the kind supposedly produced by the treatment under study, and each subject in the experimental group to demonstrate it. Since this ideal does not occur, probability limits are arbitrarily established so that one may demarcate between chance and treatment effect. This procedure is not entirely satisfactory. As Bakan pointed out, the deviation from the null hypothesis has a tendency to increase with an increase in the number of subjects utilized in the experiment. Since the significance of the results is dependent on the size of this deviation, the probability of obtaining significant results increases with sample size, regardless of the validity of the hypothesis under study. Yet, increasing sample size so as to obtain a "truly representative"

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[1] Requests for reprints should be sent to Anthony Signorelli, Union College, Springfield Avenue, Cranford, New Jersey 07016.

sample is a procedure recommended by the majority of statistical textbook authors.

Bolles (1962) pointed out that a significant finding cannot be interpreted as necessarily supporting a hypothesis:

> statistical rejection of the null hypothesis tells the scientist only what he was already quite sure of—the [subjects] are not behaving randomly. The fact that the null hypothesis can be rejected . . . does not give E [the experimenter] an assurance of . . . his particular hypothesis, but only that some alternative to the null hypothesis is true [p. 642].

Clearly, if factors, which one is either unaware of or unable to control, influence the outcome of the experiment, then one cannot with any certainty know the extent or the nature of their influence, nor contribute the results of an experiment to the specific treatment studied rather than to such factors. In spite of this ambiguity concerning the meaning of the results obtained from such experimental design, theoretical propositions are often considered confirmed or negated by them. Meehl (1967) wryly noted:

> review articles which purport to survey the current status of a particular theory in the light of all available evidence, treat the confirming instances and the disconfirming instances with equal methodological respect, as if one could, so to speak, "count noses," so that if a theory has somewhat more confirming than disconfirming instances, it is in pretty good shape evidentially [p. 112].

This toting up of statistically significant results, pro and con, to a particular hypothesis often fosters unnecessary and unproductive research. In addition, this reliance on significance frequently blinds psychologists to implicit implications of experimentation and leads to controversies that are not resolvable at the level on which they are engaged. One such example is the controversy of the definition of reinforcement as consummatory response or as drive reduction.

An experiment performed by Sheffield and Roby (1950) demonstrated that a nonnutritive, sweet-tasting substance (saccharine) was a sufficient reward stimulus in a conditioning situation. Since saccharine obviously did not relieve hunger, the experimenters concluded that reinforcement could not be defined as a stimulus or an event that led to drive reduction. They proposed instead that the eliciting of the appropriate response was the primary reinforcing factor.

Miller and Kessen (1952) responded to this finding with an experiment that they suggested failed to support the Sheffield and Roby hypothesis (1950). They injected either milk or a saline solution directly into the stomachs of rats via a stomach fistula. The rats were placed in a T-maze and rewarded with milk if they selected the correct side, and with a saline solution if they ran to the incorrect side of the maze. The animals demonstrated a significant preference for the milk side of the maze. The authors concluded that the drive-reducing properties of the milk were reinforcing, a conclusion that, of course, supports the drive reduction hypothesis.

Subsequent to the above reports, a series of experiments attempting to demonstrate the superiority of one or the other of the two hypotheses have been published, all with the appropriate statistically significant results. If, however, the obvious is accepted—that both findings are valid rather than mutually exclusive—it would be clear that what is necessary is not further experimentation with compilations of statistics, but a definition of reinforcement that is adequate to account for both findings.

The statistical significance of an event is largely dependent on the frequency of its occurrence. Thus, event frequency has become an important aspect of psychological study. Many research hours are spent producing frequent and repetitious events or seeking averages of observed behavior under the assumption that in this manner the lawfulness of the behavior or the principles thought to be basic to the causation of the behavior will be discovered. This tenet is evident, for example, in Skinner's methodological approach to learning. Skinner (1950) has suggested that the rate of response reflects the rate of learning. He, therefore, advocates the study of input variables and their influence on the rate of responding. By so doing he believes lawful relationships between the input and the response will be discovered, allowing psychologists the ability to predict and possibly to control behavior. Clearly evident in this approach is the idea that frequent correlation (between input and output) defines lawfulness. Further, the belief that repetition (of responses) reveals a basic process (learning) is obvious.

That the frequency of an event defines its nature is not a belief unique to Skinner. It is a prevalent belief in psychology. Levels of intelligence are defined by the frequency of occurrence, that is, that which is normal occurs most frequently. The frequency of the manifestation of particular behaviors often defines the category of mental illness and, in general, the average behavior observed—be it in the study of children, adults, animals, or classes of behavior such as personality or social intercourse—

is assumed to represent the essential or normal behavior applicable to the population observed.

Another prevalent influence of statistics on the practice of psychological research has been the willingness of psychologists to accept with no apparent discomfort the concept of chance. Because frequency defines the normal or essential, the infrequent event is considered abnormal or accidental. In an experimental situation, for example, the effect of a treatment under study is seldom demonstrated without exception. Provided the exceptions are at a sufficiently low level of significance, they are usually credited to chance. What is disturbing about this approach is the fact that psychologists seldom demonstrate a desire to seek explanations for chance results. Sheffield and Roby (1950), for example, found that satiated animals as well as hungry animals demonstrated a preference for saccharine over plain water. However, the hungry animals ran significantly faster and drank significantly greater amounts of saccharine than did satiated animals. If one accepts the performance of the satiated animals as due to chance, no further explanation is required. But if one accepts the performance as fact, then it is necessary to explain why animals, who were motivated by no apparent "hunger drive," nevertheless demonstrated "learning." Similarly, Miller and Kessen (1952) labeled as *error* a rat's preference for the saline side of the T-maze on any particular trial. It is error apparently due to the fact that it occurred significantly less often than the event of the rat approaching the milk side of the maze. There is no attempt to explain or to investigate the cause of such error—undoubtedly it is a chance event! The paramount importance of the statistically significant result caused investigators to virtually ignore seeking explanations for any nonsignificant findings. *Chance* is a euphemistic term for *ignorance*. The continued failure to investigate chance results, or factors that lead to such results, will in no way alleviate this ignorance.

Over 40 years ago Lewin (1931) published an article in which he suggested that the concepts and methodology of psychology resembled Aristotelian physics rather than Galilean or post-Galilean physics. Aristotelian physics, he noted, was deeply concerned with classification of objects and events. An object or event was designated as a member of a class by virtue of the number of characteristics or properties it displayed in common with other events or objects. This classification process was essential since laws governing the behavior of objects and events were considered limited to specific classes—lawfulness was not universal but class dependent. So, for example, laws governing rocks differed from those governing feathers. One of the criteria applied in Aristotelian physics for establishing the laws underlying behavior was frequency or repetition. Events that occurred regularly and repeatedly were considered lawful; the unique event, it was believed, resulted from chance.

Similarly, psychologists are concerned with classification of human events. Behavior is classified into categories such as normal or abnormal, learned or innate, motor or verbal, memory, perception, etc. Average characteristics or properties for each of the classes are enumerated, and distinct principles or laws are sought. As with Aristotelian physics, frequency or regularity of occurrence is a vital criterion for the establishment of lawfulness; the unique or infrequent event is credited to chance.

The Galilean approach to physics rejected the concept that lawfulness is limited to specific classes. Rather, it was based on the philosophy that all events and objects are governed by universal laws. Classification of events or objects is not of primary importance since their behavior is not governed by virtue of their membership in a class but by laws that apply equally to all objects or events. Given the fact that laws apply universally, it follows that the unique as well as the frequent are amenable to lawful explanation. In addition, lawfulness is not primarily predicated on the frequency or repetitiveness of observed events. Rather, laws are abstract principles that can be applied to actual events. As Lewin noted, Galileo did not formulate the law of falling bodies by calculating an average from numerous, repetitious events. Instead, he deduced an underlying factor that was common to all falling objects. One cannot observe the law he derived from frequent observation of numerous falling bodies. In fact, one can observe the descent of falling bodies of different "classes" at equal velocity only in a vacuum, that is, under controlled, experimental conditions. The physicist thus uses experimentation to demonstrate the validity of a law, to show its application under various conditions, or to demonstrate an exception to the law. The psychologist, on the other hand, performs much of his experimentation or records numerous observations of events in the hope of discovering or detecting in the data lawful relationships. In general, such laws, which are thus apparently discovered, apply

only to a limited class of events rather than to the totality of behavior.

Since laws can be applied to unique events in physics, any demonstration of an exception requires that the law be revised or abandoned; the exception cannot be credited to chance. Physical laws are, thus, much more vulnerable to attack than are psychological laws. Because most of the evidence, pro or con, concerning psychological hypotheses is collected via statistical procedures and demonstrates the validity or incorrectness of the hypothesis on a probabilistic basis, it is most difficult to demonstrate a flaw in a concept with a single exception. Any such negative result can be considered a chance finding. In addition, since laws are class dependent, negative results can be rejected because they fail to measure the class of events they purport to measure.

Lewin (1931) suggested that the distinction between Aristotelian and Galilean physics is not the mathematical formulations of the latter. While Aristotelian physics did not utilize mathematical tools, Lewin believed statistical concepts were imbedded within its procedures. The measure of central tendency is readily applicable for abstracting the common characteristics of a class of events; tests of significance are excellent for demonstrating differences between classes; correlative statistics can be applied to events within a class to demonstrate their repetitive, covariant nature. In Galilean physics, mathematical procedures do not produce concepts but are utilized to add preciseness to the concepts.

Statistical procedure leads to the measure of generality and probability. Formulations extracted from their results are stated in terms of probability and are thus imprecise. The calculation of averages tends to result in the establishment of classes that are depicted as independent and mutually exclusive, and the belief that frequent observation is required for the demonstration of lawfulness is fostered.

The pervasive influence of statistics in psychology can even be observed in the development of its instrumentation. In physics the instrumentation is designed in accordance with physical concepts. While mathematical or numerical values are an important part of the instrumentation, they are not absolutely essential. For example, the ohmmeter, which measures the resistance to electrical flow offered by a resistor, is designed from concepts developed by Georg Ohm concerning electrical energy. While it indeed has a numerical scale to measure the amount of resistance, this scale is not an indispensable part of the instrument; one might also utilize a scale of hieroglyphics. The numerals add precision to the measure, they do not influence what is measured. In psychology, instrumentation is influenced by statistical concepts. The Skinner box is designed not to measure the presence of such theoretical factors as intensity of drive or reinforcement; rather, it is designed primarily to measure the frequency of the response. Intelligence tests and personality and diagnostic scales require the compilation of averages and correlative statistics to produce classifications and to demonstrate reliability and validity of the scale; the factor under measure is inferred from the power of the statistical results.

While there are psychologists who firmly believe that theirs is a science soon to be comparable to physics and who suggest that the use of mathematical tools demonstrates this approaching equivalence, it is clear, as Lewin (1931) emphasized, that psychology as it is presently structured bears closer resemblance to Aristotelian physics than to contemporary physics. Considering the futility of experimentation that evolved from the Aristotelian philosophy, in addition to the dearth of knowledge it bequeathed to future generations, it would behoove psychologists to reexamine the influence of their procedures on the underlying philosophy of their science.

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Comment

Multiple Testing and Type I Errors:

A Reply in Defense of Multifactor Designs

Perhaps the relationship between multiple testing and the Type I level of significance bears repeating, yet the article by Udolf and Raymond (December 1973) is a cursory and misleading presentation of the problem.

The question of multiple testing and Type I error rates was brought to the attention of psychologists in the late 1950s by Ryan (1959, 1962). Since the first Ryan paper, the question has received a considerable amount of discussion (Games, 1971; Kirk, 1968; Levin & Marascuilo, 1972; Miller, 1966; Myers, 1972; Perlmutter & Myers, 1973). Therefore, the effects of multiple testing and the compensatory procedures, whether computing multiple t or F tests, is well documented.

Udolf and Raymond (1973), on the other hand, suggested that to "eliminate problems encountered with shrinking significance levels in the face of multiple F tests . . . it is simpler and more precise to conduct an independent experiment for each independent variable of interest." Since the time of Fisher (1935) it has been known that factorial experiments are more precise and simpler to manipulate than one-at-a-time experiments. Even if one does not suspect an interaction effect and does not even choose to examine for such effects, it is incomprehensible how two consecutive experiments in which two separate sets of subjects must be randomly selected and assigned can be considered simpler than a single randomization schema for a factorial design. In addition, factorial experiments are more efficient than one-at-a-time miniexperiments in that the treatment factors are evaluated with the same precision by means of only a fraction of the number of observations than would otherwise be necessary (Fisher, 1935).

Besides misrepresenting the relationship between factorial and one-variable-at-a-time experiments, Udolf and Raymond (1973) appear to be confused about error rates since they recommend substituting one multiple testing sequence for another. That is, if an experimenter's strategy is to vary two independent variables consecutively, in an attempt to answer a research question, then the experimental investigation is more appropriately conceptualized as a related twofold integrated operation, not two unrelated events, as implied by Udolf and Raymond (1973). The error rate for the complete experimental investigation would be a function of the two related one-at-a-time subexperiments and would not differ from the error rate for two concurrent multiple F tests. Udolf and Raymond's (1973) recommendation of substituting multiple miniexperiments for an omnibus factorial design is indicative of their misrepresentation of the causative component of the problem; that is, alpha protection levels are effected by *any* multiple testing schema, not by complex factorial designs per se.

Instead of suggesting a solution that is untenable for the problem, Udolf and Raymond (1973) could have referred to the Bonferroni procedure, which is a viable solution for controlling the probability of a Type I error when computing many tests of significance (Games, 1971; Perlmutter & Myers, 1973).

The rationale of the Bonferroni procedure is to set a Type I probability risk for each test of significance so that the sum of the risks will not exceed the alpha protection level that the researcher wishes to maintain. For example, consider a two-variable design and its accompanying analysis of variance statistic in which there is an F test associated with each treatment variable and the interaction of the treatment variables. If the experimenter wishes to maintain a .10 alpha protection level over all three tests, that is, over the experiment, then one means of partitioning alpha is to subject each F test to a .0333 (.10/3) level of significance and thereby control the overall alpha level at approximately .10. If the experimentwise error rate is to be evenly divided among the tests of significance, then Dunn (1961) has provided a table of critical values. The Bonferroni procedure, though, allows the experimenter to partition the Type I error to conform to his/her concerns regarding each test, just so the sum of the individual risks does not exceed the desired experimentwise error rate; e.g., .04, .04, and .02 could be used for testing the main and interaction effects, respectively, thereby maintaining a .10 overall protection level. As Perlmutter and Myers (1973) pointed out, the Bonferroni procedure is appropriate for any multiple testing situation in which the experimenter wants to control the probability of a Type I error.

Also, there are additional techniques that an experimenter may consider adopting instead of, or in conjunction with, the Bonferroni procedure, to protect the probability of committing a Type I error. Anderson (1968) noted that the experimentwise error rate can be controlled in multifactor paradigms by pooling sources of variation believed to be negligible. This pooling will reduce the number of hypotheses to be tested and hence aid in controlling the Type I error rate. Experimental design techniques can also be adopted to reduce the number of testable hypotheses. Incomplete designs, such as fractional factorials, enable the researcher to test a limited number of important hypotheses, with a frac-

tion of the number of subjects than would otherwise be required for a completely crossed design, by confounding some of the effects with higher order interactions (Kirk, 1968). Therefore, the number of hypotheses tested would be smaller than what would ordinarily be tested in a complete design, and the likelihood of a Type I error due to excessive multiple testing is diminished.

Either, or any combination, of the above three techniques would be appropriate strategies for controlling the Type I error rate for factorial experiments, while also permitting the researcher to benefit from the efficiency of factorial experimentation.

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H. J. Keselman
University of Manitoba

Further Thoughts on the National Board of Examiners in Psychology

Berg (November 1973) commented on my proposal (Coates, March 1973) for a National Board of Examiners in Psychology. It would be the task of this board to license *all* PhDs in psychology and, if there were an oversupply of candidates for certain jobs (e.g., teaching general experimental psychology), to limit the number of PhDs that were licensed. In other words, the passing score on the examinations administered by the National Board of Examiners in Psychology would be arbitrarily raised (or perhaps lowered) as a function of the number of candidates for different types of positions such as academic teaching, clinical, and industrial. The purpose of this comment is to respond to Berg's remarks.

First, Berg suggested that there is no need for a National Board of Examiners in Psychology because state licensing boards and mechanisms (unspecified) in use in universities and industry insure that adequate services are provided by psychologists. I reject this argument because it only concerns standards used *after* employment has begun. My proposal for a National Board of Examiners in Psychology concerns standards used *before* employment has begun.

Second, I agree with Berg that my proposal to arbitrarily limit the supply of PhD psychologists in the marketplace by raising the passing score on the examinations of the National Board of Examiners in Psychology is a little too Machiavellian in character. In other words, I no longer think that we should arbitrarily restrict the number of PhDs who can compete for jobs.

However, I still think that the National Board of Examiners in Psychology is a good idea. It seems to me that it could serve the useful function of providing national standards and examinations for licensing the PhD in psychology. I am not at all satisfied that a PhD has acceptable credentials when he/she has taken courses in one department with perhaps 25–30 psychologists on the faculty and has completed a PhD thesis with 4–5 psychologists on his/her committee. With such a few psychologists to evaluate a candidate's performance, it is all too possible that factors such as an ingratiating manner, a long time in the program, and a desire to not hurt the feelings of the candidate or the candidate's adviser may serve as mitigating circumstances that allow a slipshod candidate to obtain a PhD in psychology. Thus, I continue to propose that a National Board of Examiners in Psychology be established.

Fourth, Berg stated that "it would be interesting to see who would sit on this (NBEP) board, and what criteria the board would use in arriving at its eligibility list." I reject this argument. It should be no more difficult to establish a set of examinations and standards than it currently is for lawyers for the bar examination; for physicians for the licensing examinations; for psychologists for awarding the diploma in clinical, school, and industrial psychology through the American Board of Professional Psychologists; or for psychologists and others who prepare the Graduate Record Examinations. Are psychologists such a diverse and divided group that they could not establish a set of examinations? I think not.

To decrease the rancor that may occur among psychologists in setting up examinations to be administered by the National Board of Examiners in Psychology, I suggest that an examination committee be established for each division of the American Psychological Association by a vote of the members of the divisions. Or, perhaps elections could be held in such broad areas as academic psychology, clinical psychology, and industrial psychology.

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BRIAN COATES
Washington State University

On "Salvation and Its Vicissitudes"

Beit-Hallahmi's (February 1974) article appropriately challenged the assumptions of clinical psychology. I tend, however, to react to his thesis as I react to other similar positions (e.g., Ryan, 1969, 1971), that is, by questioning whether if we focused primarily on social systems we would continue to be psychologists as usually defined. Wouldn't we instead be ill-trained sociologists, economists, political scientists, and/or social activists? If, as Hebb (February 1974) asserted, "what psychology is about" is the mind, we would surely be off base if we departed radically from the "emphasis on individual responsibility, internal causation, and individual solution to problems [p. 126]" which Beit-Hallahmi disparaged.

I will not argue with the point that our social systems are imperfect and in need of major modification if not complete overhaul. I do doubt that psychologists, clinical or otherwise, can make their best contribution as social mechanics. The assertion that our social systems profoundly affect the minds and behavior—perceptions, attitudes, cognitions, defenses, coping capacities, adjustments, mental health, mental illness, or whatever our interest—of our clients or patients is incontestable (Sarason, 1974, chap. 5). How shall psychologists function to be true to these facts and to the individual focus which has been the definitive and unique contribution of our discipline?

It certainly would not be original to suggest that we are or should be the experts at the interactions: interactions between biological predispositions and environmental forces, between instincts and learning (Hebb, 1974), between individual minds and dispositions on the one hand and social institutions and influences on the other. Such a focus, or focuses, would employ our traditional methodology, data, and expertise, but would also provide sound bases for our recognizing environmental/social/institutional deficiencies and for working in our individual and corporate ways toward appropriate social remediations. Perhaps this is what Beit-Hallahmi had in mind, but I think it would be a mistake if, just as *mind* is being restored to the mainstream of our discipline, psychology should give way to a new radical environmentalism and become merely a social science.

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RICHARD D. KAHOE
Georgetown College, Kentucky

Up with Our Foremother

Bernstein and Russo (February 1974) are to be commended for pointing out the contributions of women to psychology. It was a disappointment, however, that the earliest fighter for the place of women in psychology was omitted from the discussion. I refer, of course, to Mary Whiton Calkins (1863–1930), who began at the turn of the century to make her way in a male-dominated American psychology.

Ms. Calkins was a graduate student of William James, Hugo Munsterberg, and Josiah Royce at Harvard. Though she completed all doctoral requirements with distinction, Harvard's policy did not permit her, a female, to receive a degree (nor would she accept a PhD offered by Radcliffe in recompense). Nevertheless, Mary went on to establish a psychological laboratory at Wellesley College—the first such laboratory at a women's college—and to publish widely in the fields of psychology and philosophy.

Her major contributions to psychology were the *method of paired associates* and her *self-psychology*.

In 1905 Mary Calkins became the fourteenth president of the American Psychological Association, the first woman to hold this position. In later years she was elected an honorary member of the British Psychological Association.

Although well honored in her own lifetime, Mary Calkins has not been given her rightful place in the history of psychology. The times are ready to look again at Mary Calkins not only in recognition of her work in the field of psychology but also in recognition of her early demonstration and conviction that women have a rightful place in scientific and academic endeavors.

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STEPHANIE PRATOLA
Smoke Psychological Laboratory
Gettysburg College

A Note in Support of Tyler on Elitism

In her Presidential Address at the APA convention, Tyler (December 1973) made some interesting comments on elitism that were encourag-

ing to this author. The following is an offering of some thoughts on the same issue. My hope is that these ideas will help in the effort to establish this topic as an important one that deserves continuing exploration.

The potential for elitist exploitation is inherent in the very nature of psychology. We, as psychologists, have taken on the task of trying to understand human nature in a detached sort of way. Along with other areas of study and artistic expression, our activity constitutes a kind of cultural introspection. The culture grants us the privilege of invading its privacy in the hope that the results of our probing will be beneficial to everyone. This is a mutual trust, and a delicate one, for simple reasons.

From the point of view of psychology and psychologists, the temptation to use this privilege mainly for personal and institutional self-enhancement is a great one. From the point of view of the culture, the temptation to use psychology to develop short-term expedient solutions to painful problems is equally great. The maintenance of this delicate trust requires thoughtfulness and sensitivity; we have been lacking in both.

In an effort to legitimize our work, we have tried to model psychology after the physical sciences, in which the scientist must prove his knowledge by demonstrating his control over the objects and events that he studies. The culture, in its naive worship of science, has amply rewarded psychologists for their scientific legitimacy. We have aspired to and we have been placed in a fundamentally elitist position.

Not only has psychology as an institution been rewarded with scientific legitimacy but in recent years the field has become potentially lucrative for *individual* psychologists. As more and more aspects of human living become institutionalized, individuals become less resourceful and more dependent on "experts." The public has thus become increasingly willing to reward psychologists with money and social status in return for descriptions of, and advice about, human behavior.

From an ethical point of view, such caretaker relationships are a tightrope that can be broken unwittingly at either end. How is it possible to know when each side of such a relationship is meeting its responsibilities, giving as much as it takes? How is it possible to know when the adviser's efforts are self-serving, keeping the advisee in an increasingly dependent position, so that monetary and social rewards will be increased in the future? On the other hand, is it not possible for the culture to abuse psychology? For instance, are we being prudent to acquiesce so readily in the face of current pressures for applied research? Is it not possible that profound social problems that have been in the making for centuries will refuse to yield to a decade or two of behavioral inquiry and intervention? What will be the fate of psychology should such a failure occur? As we strive to understand the ethical problems of our discipline, it should become clear that elitism is at the center of these problems.

And elitism could destroy us. As we seek and accept more remuneration for our psychological wares, we will create a greater need for them. Increasingly we will be called on to meet demands that are far beyond us. When this happens it will become clear that the mutual confidence of the culture and the profession has been violated, and both sides will suffer greatly.

To help maintain this mutual confidence, Tyler recommended that we be more thoughtful about planning our research directions and making our knowledge available to the general public; that we keep in mind the needs of psychology as an organized body of knowledge, the needs of our subjects and collaborators, and the needs of society. I would add to this program: We need to establish the exploration of ethical issues as a fully acceptable and fully supportable (i.e., financially) aspect of our professional activity and as a required part of both undergraduate and graduate training in psychology.

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PATRICK CAVANAGH
University of Kentucky

Edgington's Tabulation of Statistical Procedures

In interpreting his tabulation of statistical procedures used in APA journals during 1964–1972, Edgington (January 1974) argued that statistics courses and texts should not fail to cover the procedures and methods used most often in psychological research. For example, he suggested that the analysis of variance for repeated measures and factorial designs are prime candidates for inclusion because they were used in 88% of the articles employing the analysis of variance, which in turn was used in 71% of the articles tabulated for 1972.

Certainly most psychologists would not disagree with the view that statistical training should be relevant to the needs of the users. However, I am concerned that Edgington's recommendations might be taken too literally or, worse yet, overgeneralized. Perhaps unintentionally, Edgington has conveyed the impression that we should let current practices play a major role in deciding which topics to include in statistics courses and texts. In principle, this criterion could be applied to both undergraduate and graduate situations, allowing for more extensive coverage at the graduate level. At the extreme, strict adherence to the criterion could produce a "vicious circle"; the statistical skills of each new generation of psychologists would not differ significantly (no pun intended) from the previous generation.

If we follow Edgington's tabulation, most of the training would be

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Norman Milgram
Department of Psychology
Tel Aviv University
P.O. Box 16271
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Childrens Hospital of Los Angeles
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P.O. Box 16271
Tel Aviv, Israel

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Calvin D. Catterall
92 South Dawson Street
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Second International Congress of Collegium Internationale Activitatis Nervosae Superioris: June 30–July 3, 1975; Prague, Czechoslovakia

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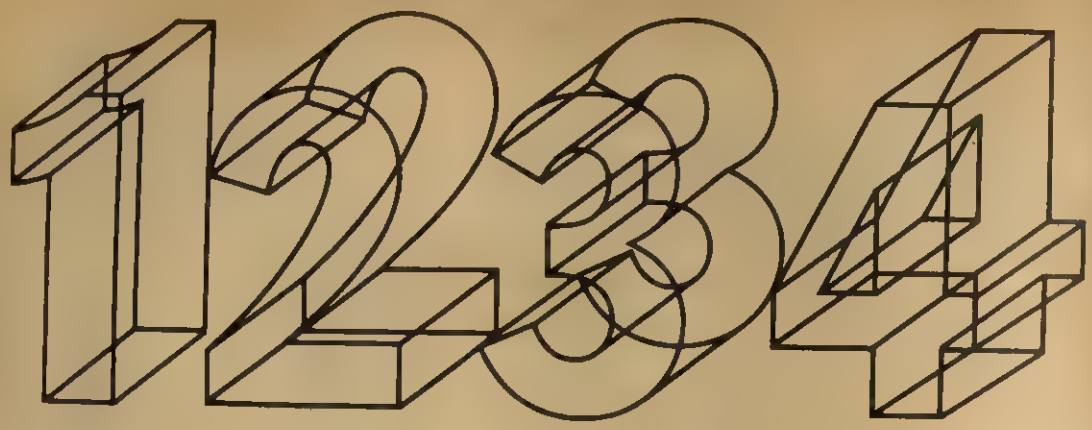

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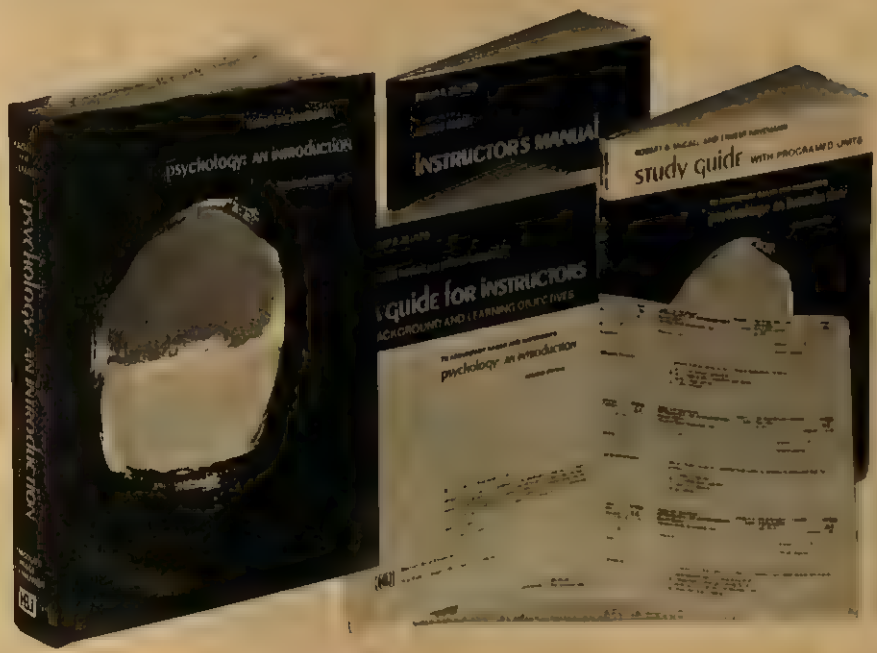

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Remember That Old Theory of Memory?

Well, Forget It!

JAMES J. JENKINS *University of Minnesota*[1]

From the early 1950s until the early 1960s, Wallace Russell and I directed a project aimed at examining the role of language in behavior. While that project involved most of the staff of the psychology department at Minnesota at one time or another and concerned itself with everything from clinical protocols to nonsense syllables, the long-term, persisting core of the project turned out to be the studies that Russell and I performed with word associations. Here we examined the effects of established associations on learning, recall, recognition, perception, and problem solving. Everywhere we looked we seemed to find powerful relationships. Then we proceeded to establish associations *de novo* and repeated these studies with materials constructed entirely in the laboratory so we could control the histories of the associations that we were dealing with. (An account of these studies is given in Jenkins, 1963, 1970.)

Those were very exciting days for us. When the research program began, there was virtually no experimental evidence for mediation theory, although such a theory was widely applied in the analysis of higher mental processes (e.g., Osgood, 1953). By the time the program was over, mediational phenomena were well established, due in part to our efforts. When the program began, experimentalists (with a few exceptions like Cofer & Foley, 1942) regarded association norms as something belonging in the clinical domain. By the time the program was over, experimentalists were using norms for all kinds of materials as important ingredients in their investigations. But, on the other hand, when the research program began, we were hopeful that we could proceed directly from our experimental work to a general account of language itself. By the time the program was over, we were convinced that *that* particular journey was not possible.

So, in spite of the fact that the theory we espoused was gaining in popularity and in spite of the fact that we had produced a very large number of successful experiments (Russell and I had done about 120 studies and David S. Palermo and I had done about 40 or 50), we abandoned that line of research. The trouble was that we became convinced that the "higher mental processes" were not the kinds of things that we had originally believed them to be. In spite of the fact that we were moving rapidly, we were not going where we wanted to go.

As we struggled with this problem, I became convinced that I was caught in a metatheoretical trap, a trap that I had built for myself without ever realizing it. My friends and I were carrying about with us common sets of unexamined beliefs and attitudes that both directed and limited our research and theory in many ways. I will leave it to others to examine their own presuppositions; the following are the ones that I now see as dominant in my thinking in the 1950s:

1. *Units.* I believed that words were the fundamental units of language. To me this was natural and obvious.

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[1] This article was the presidential address to Division 3, presented at the meeting of the American Psychological Association, Montreal, August 1973. Preparation of this article was supported in part by National Science Foundation Grant GB-35703X and National Institute of Child Health and Human Development Grant HD-01136 to the University of Minnesota Center for Research in Human Learning and by the Graduate School of the University of Minnesota.

In addition to the students and colleagues mentioned in the text, I would like to thank Robert Shaw, Walter Weimer, Robert Verbrugge, Nancy McCarrell, Jerry Wald, and Christine Bremer for their assistance. The central theme of the article, however, was derived from suggestions made by the late Richard M. Jenkins and it is to him that this article is dedicated.

Requests for reprints should be sent to James J. Jenkins, Department of Psychology, University of Minnesota, 205 Elliott Hall, Minneapolis, Minnesota 55455.

2. *Relations.* I believed that there was one kind of relation between words, associative linkage. Words became linked to each other through use together or use in the same "frames."

3. *Structures.* I believed that mental structures (if there really were any) were assemblies of links, essentially *chains* of the fundamental units in their fundamental relation. Hierarchies, like Hull's "habit family hierarchies," were simply lists of chains varying in strength, so that one was employed first, then another.

4. *Complex behaviors.* I believed that complex behaviors were built of simple subassemblies and that things got more complicated but *not* different "in kind." (This belief justified concern with simple units and relations and sanctioned experiments on such units in the faith that they would eventually add up to the complex behaviors of language.)

5. *Mechanistic explanation.* I believed that explanation ultimately rested on a description of the machinery that produced the behavior. I believed that a description of the machinery plus the history of the organism and its present circumstances inevitably predicted its behavior. There are two corollaries of this belief. First, I thought that the action of the machinery must necessarily be automatic. And, second, I could see that most of the interesting behaviors had to be explained by extensive reliance on learning and memory.

While these particular expressions of belief are probably unique to me, it is easy to see that they embrace the usual metatheoretical presuppositions that we identify with associationism. The associationist believes in some kind or kinds of basic units. (The number is not important.) He believes in some kind or kinds of relations between units. He holds that the more complex behaviors are the same "in kind" as simple behaviors. He believes that explanation consists of an explication of mechanism. He believes that behavior *is* automatic and for any kind of complex behavior, he relies on memory.

This view is so pervasive in American psychology that it is almost coextensive with being an experimentalist. Indeed, I think many of us confuse the dicta of associationism with the grounds of empirical science itself. But associationism is only one view; it is not a necessary view.

In this article, I would like to sketch an alternative position and present some experiments that convince me that there is exciting and productive work to do when we approach our field from another perspective. I will argue that this position alters the way we interpret phenomena, emphasizes the importance of natural problems and realistic methodology, and changes what counts as an explanation.

The domain I have chosen is memory. Associationism is most dominant and clearly revealed in this area, and, as I have pointed out, a general associationist account of almost anything leans heavily on memory.

In place of the traditional analysis, I suggest a *contextualist* approach. This means not only that the analysis of memory must deal with contextual variables but also, and this is my point today, that *what memory is depends on context.* This view clashes with the associative view, but I hope that I can convince you that the contextual approach is closer to the truth in the simple sense that it is in better correspondence with experimental data than the usual associative theory.

A Contextualist Formulation

The term *contextualism* is not highly familiar to American psychologists, but it is an American philosophical position that has been intimately intertwined with American psychology for three quarters of a century.[2] Another name for it is *pragmatism,* and it has its roots in William James, C. S. Peirce, and John Dewey. Unfortunately, to most of us the term pragmatism refers to only one aspect of the total philosophy, namely, the use of practical criteria for judging "truth." That aspect is not the one I want to emphasize here, nor is it the aspect that bears most directly on conceptions of memory. Hence, I have chosen the less familiar, but more descriptive, name *contextualism.*

Contextualism holds that experience consists of *events.* Events have a *quality* as a whole. By quality is meant the total meaning of the event. The quality of the event is the resultant of the interaction of the experiencer and the world, that is, the interaction of the organism and the physical relations that provide support for the experience. The relations can be thought of and analyzed into *textures.* A texture in turn consists of *strands* lying in a *context.*

[2] For an introduction to contextualism and its contrast with mechanism, the reader is referred to Pepper (1970).

Consider as an example of an event a listener hearing a sentence. The total meaning of the sentence in that context is the quality of the event. We might note that the event persists in time over some period but that all of it at once forms a single, unified psychological moment. This is the "specious present" of William James—the span of dimensionalized time is psychologically unified in experiencing the quality of the event. The textures that support the quality of the event are those of the hearer and those of the world. If our concern is with the qualities of the utterance, we can turn our attention to its texture, which consists of the words and grammatical relations between them in that context. Within that texture we can easily make out strands that make up the phrases. We can also see that some of the strands extend across the context of the phrases, that a word in one phrase depends on or determines a word elsewhere. We can become aware of the choices and presuppositions that the analyzed strands refer to. Some words and phrases can be shown to acquire their exact meaning *in that event* from their relations to more far-flung aspects of the context: a gesture, a reference, a topic previously spoken of, an event known to both the speaker and the hearer to be in the future, presumed common knowledge, and so on.

In this fashion, the analysis of the utterance will proceed far beyond the immediate physical context of the sentence and far beyond the physical stimuli immediately present. For the contextualist, no analysis is "the complete analysis." *All* analyses eventually "sheer away" from the event into more extensive contexts. This argues that there is no one analysis, no final set of units, no one set of relations, no claim to reducibility, in short, no single and unified account of anything. What makes an analysis good or bad for us is its appropriateness for our research and science and its utility in our pursuit of understanding and application.

There is a crucial difference between associationism and contextualism. Associationism asserts that there is one correct and final analysis of any psychological event in terms of a set of basic units and their basic relations. When you have reduced an event to these terms, you are through; the job is done once and for all. The contextualist takes the much less comfortable position that a "complete" or "final" analysis is a myth, that analyses mean something only in terms of their utilities for some purposes. This means that being a psychologist is going to be much more difficult than we used to think it to be. And it means that the domain of experimentation and experimental methodology will have to be criticized more intently than we thought necessary before.

Before I can persuade you to join in these discomforts, I am sure that I must offer you something better than exhortations. Let us see if this view contributes to the generation and understanding of experiments.

Experimental Examples

I want to put before you three types of experiments that we have been performing in our laboratories at Minnesota during the last few years. I think each type illustrates some important aspect of the contextualist's position. The free-recall studies, which we will consider first, showed us the importance of "comprehension" in determining the quality of an event. The experiments on the recognition of events, which we will consider next, showed us the *un*importance of contiguity in time and taught us some novel things about the activity involved in the recognition of related and unrelated items. Finally, the studies of the integration of information, which we will consider last, brought both of these together and showed us that the subject's orientation toward comprehension changed the nature of what was remembered in such a way as to produce radical changes in recall and recognition.

FREE RECALL

The free-recall experiments started innocently enough in pursuit of an answer to a simple question: If we could get subjects to attend to the meaning of words, would they show more associative clustering in recall than subjects who attended to other aspects of the words?

Associative clustering is an old phenomenon (Jenkins & Russell, 1952). If one presents stimuli and responses from a word association test in random order and asks subjects to recall the words, the subjects cluster the associates together in recall. For example, if the list of words contains *table*, *king*, and *salt* early in the list, and words like *chair*, *queen*, and *pepper* later in the list, the subjects will recall the words out of order; that is, they will tend to recall *table* and *chair* together, *king* and *queen*

together, and *salt* and *pepper* together. In the late 1950s we assumed that this effect was an automatic consequence of associative strength, but in the 1960s we thought we had better take another look at it. Tom Hyde and I (Hyde & Jenkins, 1969) decided to see whether *what the subject was doing* when he heard the words had an effect on the recall phenomena.

For a task that would require *comprehension* of the stimulus words, we asked the subjects to rate whether the words were pleasant or unpleasant. For noncomprehension tasks we decided to focus the subjects on the *form* of the words; we asked them to indicate whether the words they heard would be spelled with an "e," or we asked them to estimate the number of letters in each word. We called these *formal* tasks. All subjects heard the same word list in the same order via a tape recorder.

The results were very interesting. When subjects performed the task that required that they *comprehend* the words, their recall was excellent and they showed a high degree of associative clustering. They were as proficient in recall and clustering as a control group of intentional learners who performed no orienting task at all! On the other hand, the subjects who performed the *formal* tasks recalled only half as many words and showed little associative clustering. (As the literature suggests, whether the learning was incidental or intentional had no appreciable effect on recall for any of the groups doing orienting tasks.) It was impressively clear that the most important determiner of recall was the nature of the *event* the subjects experienced when their task brought them into contact with the stimulus words. This finding suggests that subjects recall the quality of the events they have experienced, not stimuli to which they have been exposed. And in spite of the fact that we often think of comprehension as automatic for skilled readers and listeners (such as our subjects), a radical difference appears in recall between subjects who were required to comprehend the words and those who were not.

As contextualists we must say that the occurrence of the word on the audiotape is no guarantee of any particular experience for any auditor. The quality of his experience is determined by the interaction that he has with the physical texture presented. That texture can provide support for many different kinds of experience. In this simple case, the quality of the experience is governed largely by the task instructions that we have given the subject insofar as they affect his comprehension of the words.

But the associationist is not easily upset by this experiment. He draws our attention to explanations that do not have recourse to terms like *comprehension* and *quality of events*. And he objects that the phenomenon is limited. Let us look at some of these arguments and objections briefly:

"Maybe," say the critics, "the results are due to the number of units each group has to consider. The comprehension groups deal with words; the formal groups deal with letters. The formal groups are penalized because they have to think about a lot of units and the recall is in terms of the kinds of units the comprehension group is using." Fair enough. Let's change tasks and see if this possible explanation holds up.

Carroll Johnston and I (Johnston & Jenkins, 1971) selected *rhyming* as our formal orienting task, and we selected *appropriate modifiers* (giving adjectives for nouns and nouns for adjectives) for our comprehension orienting task. Thus, all subjects listened to words and wrote other words in response. The results duplicated the first experiment. The subjects doing the formal task (making rhymes) showed poor recall and little clustering. The subjects performing the task that required comprehension showed recall and clustering essentially like that of the intentional learning group, which had no orienting task to perform. Exit the first counterhypothesis.

It was suggested that the effect relied entirely on the organization made possible by the associates in the list and that the tasks did not really affect recall of individual words. This notion was quickly refuted by running comparable experiments with both related and unrelated words (Hyde & Jenkins, 1973). Tasks requiring comprehension resulted in twice as much recall as tasks that did not require comprehension, even including tasks like assigning parts of speech to the words.

The third and fourth alternative explanations usually arose together, although they are contradictory. We call them the *time hypothesis* and the *effort hypothesis*. The arguments are as follows:

The time hypothesis. The tasks requiring comprehension must be easy. The subjects do them rapidly and have the remaining time for rehearsal. The formal tasks take more time and, thus, these subjects have little opportunity to rehearse. The

differences you find are due to differential opportunities for rehearsal.

The effort hypothesis. The comprehension tasks are difficult. These subjects must pay more attention and devote more effort to the task in processing words. Thus, they remember more readily. The formal tasks do not require much attention or effort and thus are not remembered.

David Walsh and I (Walsh & Jenkins, 1973) tackled these "mirror twin" hypotheses in an unorthodox kind of study that Walsh invented. His method is as follows: Take a list of words. Determine their recall level with a good comprehension task. Determine the recall level with a formal task. Now, *using the same time intervals for presentation,* ask new subjects to do *two tasks* in the interval for each word. If the time hypothesis is correct, recall on any combination of tasks should be poorer than recall on either task alone. If the effort hypothesis is correct, recall on any combination of tasks should be better than recall on either task alone. However, if the quality of the experience is the important factor (as *we* supposed), then comprehension tasks should produce good recall whether they are performed alone or in combination with some other task. However, formal tasks should result in poor recall alone and in combination with each other. But when they are combined with a comprehension task, they should not detract from it.

This reasoning presents a nice experiment. If the time hypothesis is correct, all combinations of tasks will yield poor recall. If the effort hypothesis is correct, all combinations will result in improved recall. If the contextualist position is correct, those combinations containing a comprehension task will result in good recall and those combinations that involve only formal tasks will result in poor recall.

In an extensive series of experiments with many different kinds of formal tasks, the outcome was clearly in favor of the qualitative contextualist position. Both the time and the effort hypotheses were defeated. Comprehension tasks, alone or in combination with anything else, resulted in recall as good as that of an intentional learning group that performed no orienting task. Combinations of two formal tasks resulted in poor recall. (See Hyde, 1973, for an independent replication.)

The last candidate for an alternative explanation was related to the feeling that something about the experiment was artifactual. Perhaps, our colleagues suggested, the general "set" of the subjects was affected in some way by the comprehension instructions—that it was not the particular task or the fact that it required comprehension but, rather, that the subjects developed a different set which somehow affected the recall. Now, surely, overall set has an important influence on the outcome of an experiment, but our particular analysis denied that the effects we were getting depended on such an overall orientation.

To investigate this problem, Robert Till and I (Till & Jenkins, 1973), following a suggestion from Endel Tulving, performed a series of experiments with a within-subject, within-list design. In this experiment each subject performed each kind of task. The subject heard each word followed by a letter, for example, *table—A*. The word was the object of his attention as usual. The letter was a cue that told him which task to perform on that particular word. Some tasks required comprehension of the word and some did not. The technique permits the comparison of the recall of words that have been involved in comprehension tasks *versus* formal tasks for each subject during a single list presentation.

The results are reassuring with respect to the earlier analysis. Subjects recalled twice as many words from the subsets on which they performed comprehension tasks as they did from the subsets on which they performed formal tasks.

These experiments, and many others that we have performed, convince me that the best way to think of this experiment is in the contextualist's framework, taking the *event* to be the interaction of the subject with the individual word. The quality of this event is radically different in tasks requiring comprehension from the quality of the event when comprehension is not required. These differences in quality are reflected in the radical differences observed in recall.

How could you further enhance recall of the word list? For the contextualist, a reasonable move would be to develop a situation in which context was even more novel and the comprehension had to be developed in some special manner related to that context. Buford Wilson and John Bransford (personal communication, August 15, 1973), reasoning in just this fashion, created such an experiment. They asked one group of subjects to rate each word as "pleasant or unpleasant to you *if you were on a desert island.*" As they pre-

dicted, the recall of words by this group was even better than the recall by the usual "pleasant–unpleasant" rating group.

I must also point out before going on that the analysis given here applies equally well to other tasks that we know will facilitate the free recall of single words: tasks such as asking subjects to form images, to rate words for vividness of imagery, to sort words into semantic categories. If the task requires comprehension, the quality of the event is such that it is readily recalled.

EVENT RECOGNITION

Let us proceed to another type of experiment that I will call *event recognition* for want of a better name. What I want to illustrate here is that the phenomena we find in recognition depend on the quality of event that the subject constructs from the experimental material during the acquisition phase of the experiment. (In the free-recall studies we kept the materials constant and showed the effects of varied tasks; here we will keep the tasks constant and show the effect of varying the materials.)

For our experimental example let us take the exciting and original work of John Bransford and Jeffery Franks on the abstraction of linguistic ideas (Bransford & Franks, 1971). For our purposes, the seminal study they invented as graduate students can serve as the key illustration. The material in the study was a set of sentences. The subjects were asked to listen to the sentences, hold them in memory for a few seconds, and answer an elliptical question about each one. Although there were no instructions about the nature of the sentences that were used in the experiment, it is apparent to even a casual listener (or reader) that the sentences fall into four interrelated groups. Each group of sentences consists of the "elements" of some overall event, taken one, two, or three at a time in various sentence compositions.

A typical set of sentences that a subject might hear in such a study is given in Table 1. The reader who has never participated in such an experiment is invited to read each acquisition sentence, one at a time, count to five, answer the question, then go on to the next sentence, repeat the routine, etc. The reader should then take the recognition test. The best way to understand the experiment is to experience it.

After the acquisition set was read to the subjects, the recognition set of sentences was presented and the subjects were asked to judge whether they had heard each sentence before and to give a confidence rating to the judgment.

The results of the study are just the opposite of what one would expect from an "associative-link" model of memory. In general, the subjects are *certain* (with ratings between +4 and +5 on a scale of 5) that they have heard the full, complex sentences that describe the four events. In fact, of course, they have not heard those sentences, nor have they even heard any sentences that long. As the sentences get shorter and shorter (down to "The cat jumped on the table" and "The girl lives next door"), the subjects are less certain about whether they heard them or not, although they generally believe that they have.

Whether the sentence actually occurred or not in the acquisition set has very little influence on its rating as long as it is one of the possible sentences that describe the event. (In the recognition set given in Table 1, for example, no "old" sentence is presented, yet subjects will estimate that from 5% to 80% of the sentences were presented earlier.) If, however, the sentences are mixed (e.g., "The scared cat running from the barking dog broke the window"), *or* if a new arbitrary fact is introduced ("The tall girl who lives next door broke the window"), subjects reject the sentence with certainty.

What has happened here is clear to the contextualist. The subjects have used the various strands repeatedly available in the texture of the experiment to construct four events that are completely described by the four long, complex sentences. The quality of each of the events is indeed the total meaning of the complex sentence. Once the fusion of the strands into events has occurred (particularly since the strands are heard over and over again in various combinations), the subject cannot perform an analysis to recover the exact pattern of input that furnished support for the construction that he made.

The analysis here is very much like the contextualist's account of the development of science—pulling together separate strands and seeing the unity of events even though they are not presented contiguously in time or space. The notion of *fusion* is an old one in the contextualist tradition and is often appealed to in explanation of the paucity of analysis available in aesthetic experience or mystical experience. (And I might suggest in passing that in the case in which highly fused

TABLE 1

Sample Experiment

Sentence	Question
Acquisition sentences: Read each sentence, count to five, answer the question, go on to the next sentence.	
The girl broke the window on the porch.	Broke what?
The tree in the front yard shaded the man who was smoking his pipe.	Where?
The hill was steep.	What was?
The cat, running from the barking dog, jumped on the table.	From what?
The tree was tall.	Was what?
The old car climbed the hill.	What did?
The cat running from the dog jumped on the table.	Where?
The girl who lives next door broke the window on the porch.	Lives where?
The car pulled the trailer.	Did what?
The scared cat was running from the barking dog.	What was?
The girl lives next door.	Who does?
The tree shaded the man who was smoking his pipe.	What did?
The scared cat jumped on the table.	What did?
The girl who lives next door broke the large window.	Broke what?
The man was smoking his pipe.	Who was?
The old car climbed the steep hill.	The what?
The large window was on the porch.	Where?
The tall tree was in the front yard.	What was?
The car pulling the trailer climbed the steep hill.	Did what?
The cat jumped on the table.	Where?
The tall tree in the front yard shaded the man.	Did what?
The car pulling the trailer climbed the hill.	Which car?
The dog was barking.	Was what?
The window was large.	What was?
STOP—Cover the preceding sentences. Now read each sentence below and decide if it is a sentence from the list given above.	
Test set. . . . How many are new?	(old __, new __)
The car climbed the hill.	(old __, new __)
The girl who lives next door broke the window.	(old __, new __)
The old man who was smoking his pipe climbed the steep hill.	(old __, new __)
The tree was in the front yard.	(old __, new __)
The scared cat, running from the barking dog, jumped on the table.	(old __, new __)
The window was on the porch.	(old __, new __)
The barking dog jumped on the old car in the front yard.	(old __, new __)
The tree in the front yard shaded the man.	(old __, new __)
The cat was running from the dog.	(old __, new __)
The old car pulled the trailer.	(old __, new __)
The tall tree in the front yard shaded the old car.	(old __, new __)
The tall tree shaded the man who was smoking his pipe.	(old __, new __)
The scared cat was running from the dog.	(old __, new __)
The old car, pulling the trailer, climbed the hill.	(old __, new __)
The girl who lives next door broke the large window on the porch.	(old __, new __)
The tall tree shaded the man.	(old __, new __)
The cat was running from the barking dog.	(old __, new __)
The car was old.	(old __, new __)
The girl broke the large window.	(old __, new __)
The scared cat ran from the barking dog that jumped on the table.	(old __, new __)
The scared cat, running from the dog, jumped on the table.	(old __, new __)
The old car pulling the trailer climbed the steep hill.	(old __, new __)
The girl broke the large window on the porch.	(old __, new __)
The scared cat which broke the window on the porch climbed the tree.	(old __, new __)
The tree shaded the man.	(old __, new __)
The car climbed the steep hill.	(old __, new __)
The girl broke the window.	(old __, new __)
The man who lives next door broke the large window on the porch.	(old __, new __)
The tall tree in the front yard shaded the man who was smoking his pipe.	(old __, new __)
The cat was scared.	(old __, new __)
STOP. Count the number of sentences judged "old." See text for answer.	

events arise from strands that are not systematically presented, historical reconstruction as to the sources of the event will, as a general rule, be unsuccessful.)

Regardless of our particular analysis, it is clear that the phenomena disclosed by these experiments pose formidable problems for storage theories of memory. Selection and integration of relevant strands and the accompanying phenomena of fusion are stubborn facts that cannot be set lightly aside. But such facts are not easily discussed in the associative framework.

If one wants to "push" the contextual analysis a little, an enlightening experiment is one that keeps the superficial form of the basic experiment above, but changes the quality of the *events* involved. If one takes away the "contextual glue" of the four themes, the results of the experiment change in dramatic ways. Peterson and McIntyre (1973) presented individual, *unrelated* sentences of the same length and grammatical structure as in the original experiment. When they examined recognition data for sentences that were actually presented, they found that the recognition confidence curve ran in the opposite direction. With *related* sentences the subjects were sure that they heard the long, complex sentences, but were uncertain about the short, simple ones. With *unrelated* sentences, subjects were sure about the short, simple sentences, but were uncertain about the long ones. Of course, in a study with unrelated sentences, the only *events* available to the subject are the individual sentences themselves. The shorter and simpler the event, the more likely the subject can satisfy himself that it is really the same; the longer it is, the less likely he is to have integrated the event and the more possibilities there are that something is different. Thus, the sentence-length-recognition function can be reversed by changing the quality of the event. As the quality of the event changes, the results change predictably.

We must also notice that the phenomena that are apparent here are equally evident in a host of psycholinguistic experiments. The studies by Sachs (1967) on memory for form and content of sentences in stories illustrate both the salience of quality and the effects of fusion. When her subjects were queried about a sentence that was embedded in the context of a story, the *form* of the sentence was incompletely remembered. The *meaning* of the sentence, however, was readily retained because it was an integrated strand in the texture of the larger event. Violations of meaning or sense were readily detected, but form vanished with the specious present.

If we contrast Sachs's study with the psycholinguistic experiments that have used *unrelated* sentences (e.g., Clifton & Odom, 1966), we have another illustration of recognition phenomena changing when the quality of the event changes. In the psycholinguistic experiments, for example, the syntactic form of the sentence is systematically related to false recognition and false recall, but the truth or falsity of the content described by the sentence (the difference between affirmation and negation) is no more important than whether the sentence is active or passive in form. The point is, of course, that there is no context in which the content of the sentence makes any sense or any difference. Hence, affirmation and negation have only syntactic meaning. That is, if each sentence is an unrelated event, then only variables at the level of construction of the sentence are of any importance.

Experiments on miniature linguistic systems reveal similar phenomena. If subjects are trained to label items in a color-form matrix such as that used by Esper (1925), they learn the *system* but ordinarily do not remember which specific instances they were given in the training trials, even though they may have seen the same instances many times. In general, memory is for systems, not for instances. With a small set of examples, subjects may remember particulars. With a liberal set of examples, however, they cannot reliably discriminate the instances on which they were trained from other acceptable instances in the system (Foss, 1968; Keeney, 1969; Segal, 1962).

INTEGRATING INFORMATION

I want to present a more complex task that requires a good deal of work on the part of the subject. In this experiment the subject must "problem solve" to achieve the complete event that we want him to apprehend. This experiment was developed by J. Richard Barclay (1971) in extending the work of Bransford and Franks. Barclay's experiments show both of the phenomena that we have noted earlier. First, changing the subject's task changes the kind of event that the subject experiences. Second, taking sentences as events results in very different recall and recognition be-

haviors than those observed when the entire experience is integrated into a single event.

In several different experiments, Barclay presented subjects with sets of sentences. The sentences gave a piecemeal account of the pairwise relations of a series of five objects that were arrayed on some dimension such as left–right or taller–shorter or faster–slower; for example, there might be five animals standing in a left–right array, five men varying in height, or five cars varying in speed.

The "comprehending" subjects were told that they would hear sentences describing the relations between the five animals (or men, or cars) and that they were to listen carefully to the sentences so that at the end they could properly line up the objects in order. The control subjects were told that they would hear a set of sentences that were about animals (or men, or cars) and that they were to try to remember the sentences that they heard. After hearing the sentences several times, the subjects were given a comprehension test and a test of sentence recognition or recall.

The first result of the experiments was the obvious finding that 80%–100% of the comprehending subjects constructed the correct array of objects, but only 0%–10% of the subjects who were trying to remember the sentences could do so. The second result was that the comprehending subjects were excellent in discriminating true sentences from false sentences but could not remember which of the true sentences they had actually heard. All of the true sentences seemed familiar to them, and about equally so, whether they were original sentences, correct paraphrases, or correct inferences from the original sentences.

The group that was specifically told to remember the sentences presented quite a different picture. These subjects were poor at recognizing which of the sentences they had heard and almost equally poor at separating true sentences from false ones. In other words, when subjects considered the sentences as the events of the experiment, they learned very little, their confusions were determined by variables at the sentence construction level, and they showed evidence of massive interference. With the array itself as the event to be constructed, specific sentence memory was essentially nil, but the general constraint of what was an acceptable or "true" sentence was virtually perfect. The same results were obtained in recall experiments. The comprehending subjects recalled *no* false sentences and "recalled" true sentences independently of whether they had heard them or not.

This experiment is a dramatic example of the principles we have encountered in both of the foregoing types of experiments. First, what the subject was instructed to do with the input was the most important determiner of the nature of what was remembered. Second, when the event itself was well fused, the specific instances from which the event was constructed were not readily discriminated. Finally, the availability of the global event in this case makes it possible for the subject to reject one class of sentences (the false ones) and to accept another class of sentences (the true ones) as likely to have occurred even though he could not discriminate what actually occurred. That is, the event (the representation of the array which is remembered) is related to sentences in such a way that permissible utterances can be generated or recognized with great accuracy even though actual, historical sentences cannot. *What* is remembered is definitely not verbal and probably not even linguistic, but it plays a key role in determining which linguistic constructions are recognized and recalled.

Discussion

What kinds of lessons can we draw from these experiments? First and foremost, I think it is clear that we should shun any notion that memory consists of a specific system that operates with one set of rules on one kind of unit. What is remembered in a given situation depends on the physical and psychological context in which the event was experienced, the knowledge and skills that the subject brings to the context, the situation in which we ask for evidence for remembering, and the relation of what the subject remembers to what the experimenter demands.

Contextualism calls us back to considering what the subject believes and knows when we talk of memory. It simultaneously suggests that we look to the sources of this belief and to the subject's ways of constructing and reconstructing his experience. William James pointed out that the only thing that distinguishes memory from the other higher mental processes is just that belief. He argued that there is nothing unique in the object of memory and no special faculty evident in its manifestations. Apart from the belief that the con-

struction of the mind is attributed to the past, he saw nothing to set memory apart from perception, imagination, comparison, and reasoning.

Such a claim is unsettling because it says: *Memory is not a box in a flow diagram.* It is also threatening because it seems to demand an understanding of all "the higher mental processes" at once. Yet, that is what the data of our experiments suggest. Surely these studies imply that we cannot deal with memory without dealing with instructions, perception, comprehension, inference, problem solving, and all the other processes that contribute to the construction of events. The theoretical contextualist agrees. He argues that there is no other approach. To study memory without studying perception is to invite disaster in one of two ways: on the one hand, failing to understand the inputs to the subject or, on the other hand, pushing all the difficult problems out of memory into the unknown perceptual domain for someone else to study. To study memory without studying inference is to be baffled by the transformations that the subject puts on his experience. To study memory without studying language is to be confounded by paraphrase and the issues of meaning. And so on.

The contextualist avoids despair at this point, not by predicting success for a global assault on all higher mental processes simultaneously but by asking again, "What kind of an analysis of memory will be useful to you in the kinds of problems you are facing? What kinds of events concern you?" If you limit the events for some purpose, he will lead you into an analysis of context, textures that support experiences, strands that interrelate aspects of experiences, etc.

You should notice that methodological issues and the choices of experimental paradigms become crucial when you take the contextualist view. It is *not* true in this view that one can study any part of the "cosmic machine" to just as good an effect as any other part. The important thing is to pick the right kinds of events for your purposes. And it *is* true in this view that a whole theory of an experiment can be elaborated without contributing in an important way to the science because the situation is artificial and nonrepresentative in just the senses that determine its peculiar phenomena. In short, contextualism stresses relating one's laboratory problems to the ecologically valid problems of everyday life.

Conclusion

It would be easy to believe from this brief presentation that contextualism is merely eclecticism given a fancy name. It would also be easy to suppose that my plea is for miniature theories that will eventually integrate into a global theory. And it is easy to imagine that all of us could be contextualists and at the same time continue working in just the same fashion that we always have on associative theories. But none of these beliefs is correct.

Contextualism is historically tolerant of other approaches and is always willing to examine a claim that the cosmic machine is all of one piece. But at rock bottom there is a profound difference in belief between *associationism,* which presupposes fundamental units and relations out of which all else is constructed, and *contextualism,* which presupposes that events are primary and that the quality of events determines what the possibilities are for a host of analyses.

The associationist finds that semantic features (or word associates or images or what-have-you) are important in predicting recall of individual words. "Fine," says the contextualist, "but do not bet any appreciable amounts of money that those will be important considerations when you combine words in sentences or embed them in different contexts."

The associationist finds that grammatical features are predictive of memory distortions in single sentences and believes that he has a theory of comprehension. "Look out," says the contextualist, "those are unlikely candidates for relevant variables for understanding sentences in context." And so it goes.

The contextualist does not believe in one order of phenomena that dominates the psychological world. He does not believe that there is a single paradigm of explanation such as mechanism. Explanation for the contextualist consists of explicating *for some purpose* the quality of an event that we find important or interesting. That entails in turn a description of its spread in time and its degree of fusion. It further calls for a discussion of the textures that support the quality of the event and the strands making up the texture and relating it to relevant aspects of its context. Unlike the associationist explanation, the contextualist explanation is put together from the top down, rather than from the bottom up; and it is oriented toward

the *event* rather than toward the supposed *machinery*.

I believe that the field of memory research is moving toward contextualism, and I, for one, welcome it. I think that an awareness and acceptance of this movement will free us to explore the ecologically valid problems of memory that are of real importance to us. I think it will help us to be alert to the identification of new, emergent phenomena as they are encountered. In short, I think that contextualism provides a real, viable, and optimistic alternative to the oldest theory of them all.

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Translations in Gerontology—From Lab to Life

Utilizing Information

DAVID SCHONFIELD *University of Calgary, Alberta, Canada* [1]

When elderly people are asked to participate in investigations on aging, they sometimes object strongly and question the fundamental assumption underlying such research. They suggest that one older person differs so much from another that the quest for generalization is pointless. Although it is easy to attribute such reactions to a negative attitude toward the idea that behavior is in any respect predetermined, the laboratory itself justifies certain reservations. The journey from lab to life must begin with the finding that there are usually greater differences among a group of older people than among the young. In other words, standard deviations tend to increase with age. Figure 1, taken from Heron and Chown (1967), is a typical illustration. Each dot represents one score from a male sample on the Progressive Matrices Intelligence Test, a test of the ability to utilize information. Some people in their 40s, 50s, and 60s do better than the average 20-year-old. More importantly, there are a few scores among these older groups as good as the best of the younger crowd. It is the lab that provides the caveat for planning and action solely based on average scores. Older people are slower on the average than the young, but it would be wrong to exclude all older workers from jobs that require fast responses—some of the old are just as fast as the young. Sexual drives tend to decrease with age, but a prospective marriage partner with 20 year's seniority may well be one of the many exceptions to that rule. There are, of course, exceptions to rules about the young, too, as can be seen in the 30s and 40s (see Figure 1). However, in general, we can say that there are more older people who behave like the young than young people who behave like the old.

A second pitfall to avoid in the journey from lab to life is due to relativistic and impermanent connotations of the terms *old* and *young*. In Canada a few years ago we considered that we had elected a young prime minister, aged 48, but as a bridegroom of 50, we thought of him as old. In experimental studies, we similarly find that those between 35 and 45 are occasionally labeled as young compared to a group with an average age of 70, but labeled as old compared to a 20-year-old group. The results of such studies are summarized in sentences that begin, "With age, such and such occurs." From the applied point of view, it is of fundamental importance to know whether the difference is between the ages of 20 and 40 or between 40 and 60. The extrapolation type of error is less frequent than the interpolation error, because most studies of human performance employ a group of subjects in their early 20s and another group over 65 years of age, with no one in between. Again the findings are summarized as, "With age, there is deterioration in something or other." The

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Editor's note. The public demand that psychological research become "relevant" can be met by a demonstration that laboratory findings are indeed relevant to life's problems. In order to apply such findings, a crucial step of translation is required to bridge the gap between the conclusions of experimental investigators and solutions to practical difficulties. The need for bridges is nowhere greater than in gerontology because problems of the aged are so severe. Therefore, the Division of Adult Development and Aging (Division 20) of the American Psychological Association requested three renowned contributors to the literature on performance and aging to make the difficult translation from research findings in the laboratory to the multitude of problems of the aged. This article by David Schonfield and the following two articles by K. Warner Schaie and James E. Birren, respectively, are their responses.

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Requests for reprints should be sent to David Schonfield, University of Calgary, 2920 24th Avenue, N.W., Calgary, Alberta, Canada T2N 1N4.

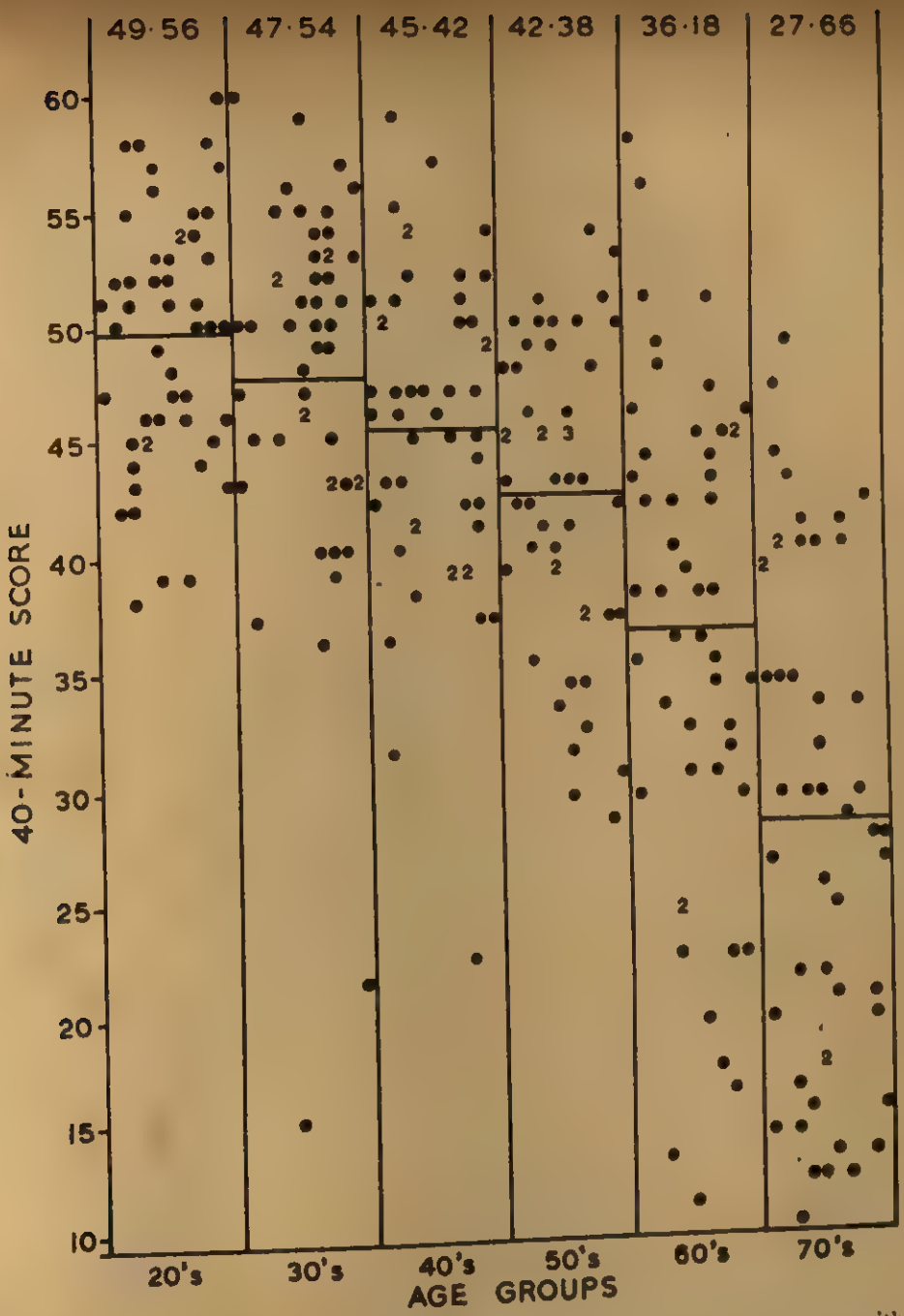

Figure 1. Progressive matrices (men): Distribution of 40-minute scores with means indicated by horizontal lines. (From *Age and Function* by A. Heron and S. Chown; London: J. & A. Churchill, 1967. Copyright 1967 by J. & A. Churchill. Reprinted by permission.)

suggestion of a gradual and equal decline in each decade between 20 and 70 is unlikely to be correct. When there are only two widely separated, empirically established points on the graph, it is wrong to join them by a straight line and suggest that this represents the age dimension. Figure 2 (Bromley, 1966) provides examples of this kind of interpolation. All of the decline may occur during one or two decades, with a plateau or even improvement during the remaining periods. Personnel officers and policy makers can justify on this basis their refusal to employ people over 45

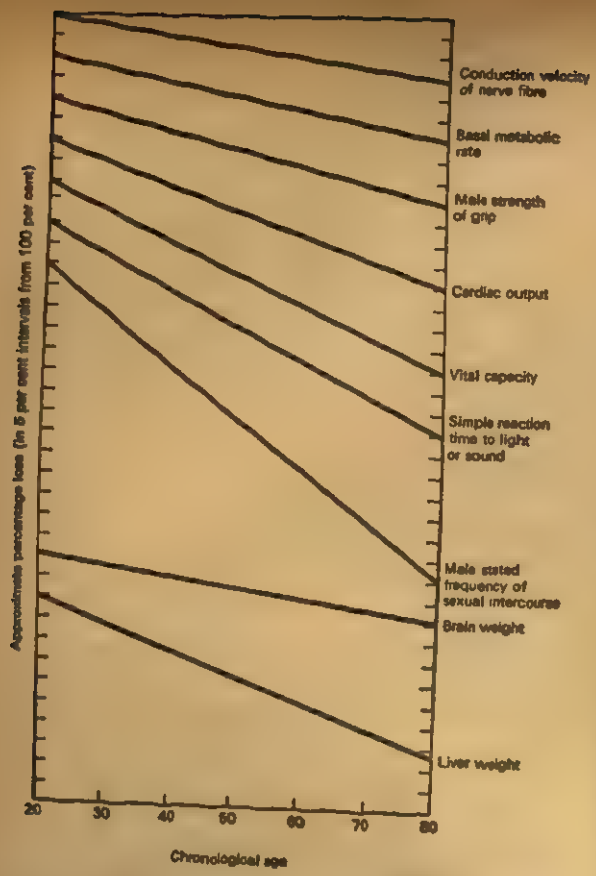

Figure 2. Relative age differences for several biological variables expressed as a percentage loss of function from 100% at the age of 20. (From *The Psychology of Human Ageing* by D. B. Bromley; Baltimore, Md.: Penguin Books, 1966. Copyright 1966 by Penguin Books. Reprinted by permission.)

or to press for the early retirement of those in their 50s. In general, we are abysmally ignorant concerning stability and change of behavior during the middle adult years. Be that as it may, the journey from lab to life must begin and end with the same flight number; we should not leave the lab with the 70s and arrive in life with the 40s.

A third methodological issue concerns the absence of age differences in the ability to utilize information. From the applied point of view, any human capacity that shows no difference or minimal change during the adult years should be pinpointed and highlighted. It is on the basis of such *conservation* of ability that older people can compensate for *losses* in ability. The theoretically oriented scientist often ignores findings that show no change with age. He is searching for signs of aging, and the absence of an age change is therefore only of tangential interest. Psychologists should not follow, what might be called, the *biological model* in this regard. We need to emphasize the absence of age differences in order to counteract negative attitudes toward aging. It is just not true that every day in every way we get worse and worse. Situations in which older people have special problems in acquiring information, recalling information, or utilizing information are also of fundamental interest. If such situations can be circumvented, competence can be increased and strain avoided. The most valuable findings of all are those in which the manipulation of one or more variables results in large reductions in age differences. Useful translation from gerontological research to life becomes reasonably straightforward when we can specify situations in which age differences are diminished or disappear.

Information Presented in Unusual Ways

Applied implications of many simple, theoretically based laboratory findings are often easy to envisage. It is not even essential to undertake the physical journey from lab to life—a little imagination suffices for the busy commuters between lab and conferences. Take as an example one of the earliest controlled studies on age effects. Ruch (1934) found that the difference between a middle-aged and an elderly group was much greater when spatial information was provided through a mirror as compared to direct vision. The looking glass difficulty for those over the age of about 55 has been confirmed in settings other than the pursuit rotor task which Ruch employed (Welford, 1958). This difficulty coincides with a general principle about the problem of overcoming long-standing habits. Therefore, we are warranted in alerting those who use mirrors in the "real world." Physiotherapists and speech therapists, for instance, often attempt to provide feedback by means of a mirror when rehabilitating patients following a stroke. During the initial learning period this kind of crutch is likely to be less helpful to the aged than to the young. Ruch's major concern was with the plasticity of nervous tissue, but that theoretical issue becomes superfluous baggage in our journey from the lab. There is at least one danger in this kind of translation of gerontological research. The message that the elderly have difficulty with mirrors and that extra encouragement coupled with patience is required for them can be-

come distorted into a recommendation that mirrors should be avoided in rehabilitation work with older patients. This possible harmful outcome is due to the prevalent carelessness in the use of language which has coincided with our obsessional carefulness about experimental design. An exception to the generalization about the age-associated looking glass difficulties is derived from another conclusion of gerontological research. Well-established skills that continue to be practiced show minor deficits compared to age difficulties in acquiring a new skill (Szafran, 1968). There does not seem to have been any specific investigation on this point with respect to mirrors. Nevertheless, applied psychologists should not be afraid to make the pronouncement that older dentists, for instance, will not experience special difficulties when using mirrors to examine teeth, providing they had their training when they were in their 20s or 30s. On the other hand, starting a second career as a dentist at the age of about 55 is likely to cause problems in utilizing and combining information derived from mirror vision and direct vision.

Ability to Ignore Irrelevant Information

We turn now from information presented in an unusual way to an age decrement in the ability to ignore irrelevant information. Only one report seems to have been published focusing specifically on the topic, and in that experiment of Rabbitt (1965) the older group's age was over 65. However, an increase in distractibility beginning probably at about 45 is suggested by a number of studies. The evidence on difficulty in identifying concealed figures (Basowitz & Korchin, 1957), in maintaining sets (Talland, 1959), and in maintaining performance on dichotic listening (Schonfield, Trueman, & Kline, 1972) all point toward a problem of excluding from attention material irrelevant to the task at hand. Distraction by irrelevant stimuli is an all-pervasive hazard in the real world, and innumerable examples could be cited in which special consideration should be given to this age-related difficulty. If one does not stick to the main point in conversations with older people information is less likely to be utilized and misunderstandings will ensue. On the other hand, the reputation that the aged themselves have for not sticking to a point can be considered a manifestation of distraction by their own irrelevant memories. When an older person is engaged in the more dangerous pastime of driving an automobile, distraction by the irrelevant reduces utilization of the vital relevant information. The translation from lab to life should result in the banishing of unnecessary billboards and road signs, especially on busy thoroughfares. Similarly, there should be a general discouragement of conversation when an elderly person is driving during rush hours or driving a new car or driving in a strange city. In other words, if an activity requires greater concentration and if the absence of concentration can result in injury or accident, special procedures are needed to avoid distraction on behalf of the elderly population.

Speed and Timing

The third example of simple translation from lab to life is derived from the area of speed and timing covered in Birren's article (see pp. 808–815, this issue). Field research in the early Cambridge studies showed that workers in their 50s tended to leave jobs in which performance was paced by a machine or another external source (Welford, 1958). Older workers seemed to have little difficulty, however, in continuing to hold up well in self-paced tasks. The reverse journey from life to lab produced experiments suggesting that acquisition of information was not readily time shared with decisions about responses to be made. Later studies, in particular those of Eisdorfer (1968), led to the conclusion that time limitations can be emotionally upsetting for older people. Ordinary conversation, especially with busy people, often resembles an externally paced task. The questioner impatiently waits for a response while the respondent is attempting both to understand the question and to prepare an adequate answer. By the time an elderly person is ready to talk, the questioner has given up and a new question is posed. Those who are in constant contact with the elderly—family, civil servants, and social workers—should convey an unhurried impression in order to avoid an inquisitional atmosphere with all of its emotional outcome.

Threshold Level

The raising of thresholds is the source of the fourth translation example. Food remaining near the mouth after eating and the occasional drip of mucus on the nose are sometimes not felt by a

younger person. The sensation is at about threshold value for the young and more likely to be below the touch threshold in older people. Alerting the elderly themselves to this kind of reduction of information input could have valuable benefits. Social relationships seem to become more precarious as we age, and they are likely to suffer when appearance is somewhat repulsive. Here, the recommendation is an extra check in the looking glass as opposed to the earlier example of a reduced usefulness of mirror vision.

Learning and Memory

The next translation example comes from learning and remembering. Theorists wish to pinpoint the locus of the age-associated deficit: Is it short-term memory? Is it acquisition? Is it the transfer from short-term to long-term memory? Is it retrieval? The practical implications, however, of increasing difficulties in reproducing newly acquired information are often straightforward. Surely we can tell physicians that they should provide written instructions for their elderly patients, so that memory can be checked. After verbal instructions, the patient is less likely to remember whether the doctor said, "Do it three times a day," or "four times a day." Did the doctor say, "Swallow the medicine," or "Place it in the anus"?

Caution

The last example in this series emanates from the tendency toward increased caution with age. Botwinick (1966) has been the major contributor to the literature on this point, but others such as Szafran (1951) in his studies on utilization of vision and Canestrari (1963) on omission errors provide confirming evidence. As we get older we adhere not only to the advice to look before we leap, but we also seem to prefer to look while we are leaping. Industrial gerontologists can use these findings in the counseling they provide about suitable jobs for middle-aged and older workers. Inspection types of occupations, in which carefulness is at a premium, are probably ideal occupations for these age groups.

Conclusion

These six illustrations—use of mirrors by dentists and others, irrelevant information and driving, self-pacing in conversation, threshold touch sensation and its social implications, aids to remembering physicians' instructions, inspection-type jobs—suffice to exemplify one approach to translations in gerontology. A specific finding in the lab is recognized as an example of a more general principle, and a jump is made from the finding to a closely analogous situation in the "real world." A prerequisite is considerable knowledge of the discipline so that the practitioner recognizes which specific findings are supported by other investigators. The jump is a nonscientific activity and could perhaps be labeled gerontological common sense. It has been suggested elsewhere (Schonfield, 1972) that all research workers should be encouraged to make this jump by including a section headed "Proposals for the Application of Results" at the end of published articles. The principles will vary according to the breadth of their potential applications. When the breadth is narrow, as in the case of the looking glass example, they might be called "principlettes," and progressively wider applicability would become queen-size principles—for example, the irrelevant information finding—and king-size principles. (The nomenclature is derived from bed size and perhaps because of that it smacks of male chauvinism.)

A king-size principle suggested by the six illustrations is that functioning requires increased attention from about the age of 50. Attention to more than one activity cannot be shared, and concurrent multiple functioning becomes increasingly difficult. Processing of input requires attention because the meaning of signals does not occur so automatically. Processing of output requires attention because "actions do not take care of themselves." (That phrase was used by Sir Frederic Bartlett some 25 years ago in an unpublished lecture.) The fact of time pressure is attended to instead of being ignored, perhaps because paying attention has become a habitual strategy both for relevant and extraneous facets of the task. Even psychological functions, such as remembering, do not "take care of themselves" and remembering to remember demands vigilance. The principle of extra attention has implications for almost all situations in the everyday life of older people.

The use of the term *principle*, rather than *theory*, in these descriptions of translation from lab to life has been deliberate. Following Broadbent (1971), I confess to "distaste for the amount of theory current in psychology" with its "stream of papers giving hypothesis, prediction and verification [p.

5]." Although there is no inherent contrast between theory and practice, the attitude prevails that a theory has greater theoretical value when it has minimal applied value. The pursuit of principles rather than theories and hypotheses may help gerontologists to keep applications at the forefront of our attention. Broadbent (1971) rightly said:

> The researcher remote from practical pressures may indeed be free to study major variables in which at this instant society does not seem to be interested, but he should not use this freedom in order to study minor variables, until there are no major ones within reach of our techniques [p. 4].

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Translations in Gerontology—From Lab to Life

Intellectual Functioning

K. WARNER SCHAIE *University of Southern California* [1]

Developmental psychologists often find themselves in a cross fire between their more traditionally oriented colleagues who distrust the need for the quasi-experiment, which must be one of the major thrusts for the developmentalist, and the clinician or other applied professionals who feel that the data provided by the scientist prove the obvious or are quite unintelligible. It is the purpose of this article to speak to this issue and to show that people like myself do perhaps have ways of combining the rigor of the scientific method with an address to problems that may indeed be of social consequence.

A substantial portion of this article will be concerned with the myth of intellectual decline. That is, I will try to spell out as simply as possible why the presumed universal decline in adult intelligence is at best a methodological artifact and at worst a popular misunderstanding of the relation between individual development and sociocultural change. Second, I will briefly examine some possible ways in which old adults may differ qualitatively in their approach to intellectual performance, paying attention mainly to motivational factors which may indeed interfere in the performance of old adults on intelligence tests as well as life tasks designed for young people. Third, I shall then examine certain social policy implications which follow from the very different reading of the data on intellectual functioning in adulthood and old age which I and my associates have found necessary (cf. also Baltes & Schaie, 1974; Schaie, 1973b).

The Myth of Intellectual Decline

It has long been the popular notion that old people may have much wisdom based on a long life, but that they should not really be allowed to apply that wisdom to any matter of consequence or be entrusted to perform important societal functions because it is assumed that they are no longer capable of sustained high-level intellectual performance. This view has been strongly supported by some of the early cross-sectional studies of intelligence (e.g., Jones & Conrad, 1933; Wechsler, 1939). These studies indeed showed that on comparable tasks, peaks of intellectual performance occurred in late adolescence, with consistent decrement thereafter.

This view was first challenged by some early longitudinal studies of adult intellectual performance (Bayley & Oden, 1955; Owens, 1953) that found maintenance of most intellectual functions at least into the early 50s. Some of this evidence was soon dismissed as being due to the use of highly selected samples or the hypothesis that age may be kinder to the more gifted who were examined in these studies. Nevertheless, it has now become incumbent to take a close look at the issue of the discrepancies between the results of cross-sectional and longitudinal findings, and much of my own work during the past decade has addressed itself to that particular issue (cf. Schaie, 1965, 1970, 1973c).

In order to understand the evidence on intellectual change in adulthood and to infer social implications we must address three issues: The first deals with the question of whether intellectual decrement is a general or specific phenomenon, the second is concerned with the differentiation of ontogenetic changes in function from differences in performance level between generations, and the

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Requests for reprints should be sent to K. Warner Schaie, Gerontology Center, University of Southern California, University Park, Los Angeles, California 90007.

third is concerned with the issue of individual differences in intellectual development.

INTELLECTUAL DEVELOPMENT: GENERAL OR SPECIFIC

The early history of intelligence testing was greatly concerned with attempts to develop homogeneous indices that would maximally predict school performance, and later attempts to develop instruments more suitable for adults maintained the notion of the desirability of a single composite index of intellectual ability. But quite early factor analytic studies showed that there were indeed many different building blocks of intelligence (Guilford, 1967; Thurstone, 1941), and it did not seem unreasonable to assume that such differential abilities might follow different life courses. Such evidence was presented in cross-sectional as well as longitudinal studies (Owens, 1953; Schaie, Rosenthal, & Perlman, 1953). Schaie et al. found that older people did much more poorly on abilities such as spatial visualization and reasoning than was the case for tests of verbal meaning and word fluency.

Cattell (1963) pushed the argument a step further by proposing that there were certain crystalized abilities that were culturally overdetermined, and consequently once an asymptote was attained these abilities should not be subject to decrement, while fluid abilities being basically innate and biologically determined as a species characteristic should consequently also be subject to the decrement phenomena associated with the life rhythm of the species. Although the isomorphism with respect to the physiological base of the fluid abilities as opposed to the acculturated base of the crystalized abilities is currently under challenge (Arnold, 1973), there is evidence that would at least be compatible with Cattell's point of view requiring different developmental histories for different abilities (see Schaie, 1970). This evidence then suggests strongly that old people as a population subgroup perform better on certain abilities than on others. And this seems to be true for individuals as well, so that a single measure of intellectual ability may be particularly misleading for older individuals in describing their actual level of performance.

But the evidence from the cross-sectional studies showing that peaks of performance may occur at different ages for different abilities and that old people do better on some tasks than on others does not directly address itself to the question whether or not there is indeed decrement in intellectual functioning. This question must now be considered.

COHORT DIFFERENCES VERSUS ONTOGENETIC DIFFERENCES IN INTELLECTUAL FUNCTIONING

More than 30 years ago Kuhlen (1940) alerted us to the problem that not only do people age but so do cultures, and that it is therefore possible that the perceived deficit of the old may be no more than obsolescence in the face of a rapidly changing sociocultural environment. Unfortunately, neither cross-sectional nor longitudinal studies can deal with this question directly. Cross-sectional studies compare members of different age groups at one point in time who are consequently also members of different birth cohorts who must have had differential experiences that may or may not have anything to do with age. It could therefore be argued that the differences found by such studies during periods of rapid technological change ought not be interpreted as age differences at all but rather as evidence for differences in performance level occurring as a function of membership in different generations. But longitudinal evidence does not by itself bring us any further. As I have previously documented (Schaie, 1972), in the behavioral sciences we are forced to accept the fact that environmental impact has profound importance on determining changes in behavior. Consequently any change in performance over time must confound whatever maturational changes are occurring with those events attributable to the unique environmental impact occurring over a given period of historical time which may equally affect individuals at all ages. Results of a single-cohort longitudinal study may therefore be parsimoniously interpreted as being relevant to the chronicling of change occurring at a particular period in time, but not necessarily as reflecting ontogenetic change.

The way out of this dilemma is, of course, the adoption of sequential methods of data collection and analysis (Schaie, 1965, 1973c). Two approaches are needed: The first requires the replication of cross-sectional studies over several points in time. The second involves the carrying of several cohorts over the age ranges of interest. In our own work we have now done both and the

results are indeed revealing. They show with great clarity that a much larger proportion of the variance associated with age can be attributed to generation differences than to ontogenetic change and that both peak levels and slopes of change in ability are changing in a positive direction. In other words, there is strong evidence that much of the difference in performance on intellectual abilities between young and old is *not* due to decline in ability on the part of the old, but due to higher performance levels in successive generations. Some decrement, particularly on tasks involving motor speed, remains to be accounted for, but its onset does not seem to occur until the 60s, and large individual differences remain (Baltes & Labouvie, 1973; Schaie & Strother, 1968b; Schaie, Labouvie, & Buech, 1973; Schaie & Labouvie-Vief, 1974).

We have concluded then that the overall group data require us to consider most of the intellectual decline in the healthy old to be a myth. Nevertheless, we must consider that for given individuals there may indeed be significant decrement with age, but alternatively that there might be growth until very old age.

Two lines of argument should be pursued here. The first is concerned with the evidence that there may indeed be significant decrement in intellectual function for every or most individuals as a precursor to their physical demise. That is, changes in psychological function may occur concomitantly or even ahead of the final period of life. Evidence along this line has been presented by Riegel and others (Reimanis & Green, 1971; Riegel & Riegel, 1972; Riegel, Riegel, & Meyer, 1967).

The second approach to the matter of individual differences comes from a careful analysis of individual life patterns. Such analyses show that at each age-cohort level there are some individuals who (at least over a 14-year period of time) gain and others who lose, while many remain stable (Schaie, 1973a). Moreover, analyses of current life-styles and retrospective history of the very old, as well as the analyses of homogeneous groups of the highly successful elderly, reveal vast differences in the degree to which differences in performance are maintained in the later years of life (cf. Reichard, Livson, & Peterson, 1962; Schaie & Strother, 1968a). In sum, the old are no more homogeneous a group than the young, and aging is no kinder to the able than to those at a low level of functioning (Baltes, Nesselroade, Schaie, & Labouvie, 1972). We may suspect that knowing more about the microenvironment in which individuals live will give us a better handle on these differences, but work in this area has only begun.

Motivational Factors in the Intellectual Functioning of Old Adults

No matter what interpretation is placed on the comparison of old and young on tests of intelligence it is readily accepted that most of the devices used were constructed for the assessment of young people and that the experience of the old in dealing with the task situation may be quite different, while no doubt the motivational factors operating have altered. These issues will now be attended to.

CAUTIOUSNESS AND RISK-TAKING BEHAVIOR

It has been observed that old people are consistently more cautious than the young. This has been effectively demonstrated by Wallach and Kogan (1961), who used 12 different life situations, and replicated by Botwinick (1966a), who adapted the task especially appropriate for aged subjects. Interestingly enough, older people tended to avoid taking any course of action if this was a permissible alternative. But when risk taking was required it was noted that old people were not more cautious than the young (Botwinick, 1966b). How does this cautiousness apply to behavior on tasks of intellectual ability? Birkhill and Schaie (in press) administered the Primary Mental Abilities Test to a group of elderly subjects under high- and low-risk conditions, each condition further under a choice or no-choice set of instructions. A highly significant Choice × Risk interaction was found. That is, there was no difference in performance under the no-choice instruction, but under the choice instruction subjects performed significantly better under low-risk conditions. We may suspect, then, that at least some older people do less well when they are afraid that involvement in a task involves unreasonable risk of loss or embarrassment, but that careful control of instructional set may well induce the older person to consider alternatives he might otherwise eschew.

COHORT-APPROPRIATE ASSESSMENT TECHNIQUES

Although the Wechsler-Bellevue was one test especially constructed to counter the objection to the Stanford-Binet that it was not age appropriate, little has been done to continue the search of ma-

terials that might be appropriate to the careful definition of intelligence for adults and particularly the aged. The problem here is that once again we are confusing the contribution of ontogenetic and sociocultural change to the study of age differences. In other words, a test is not inappropriate for old people because it was constructed for young adults, but rather because it was constructed for the members of a different cohort with different sociocultural exposure. Paradoxically, I suspect, the original Wechsler-Bellevue would likely now discriminate against young adults. And R. F. Monge and his associates (personal communication, 1971) have shown that it is quite possible to construct tests[2] at will which favor the members of earlier cohorts (also see Demming & Pressey, 1957). I am suggesting then that our findings on cohort differences call for the development of more cohort-appropriate assessment devices, carefully linked by comparative factor analysis (Nesselroade, 1970), before the conventional young–old comparisons so popular in the gerontological literature can become truly meaningful. Along these lines, we may further question whether or not the kind of techniques that appeal to young children simply on the basis of curiosity or competence motivation can have similar appeal to adults. Yet there must be many life situations that are saturated with the factors relevant to the construct of intelligence which offer rich promise to future investigators.

Implications for Social Policy

I have previously argued that there may well be a conspiracy on the part of the middle aged to remove the old from active participation in society. I suggested (Schaie, 1973d) that it is quite true that in a primitive society cumulative trauma will lead to loss of energy level and functions with increasing age, such that old age and increasing pathology, and therefore deficits, are virtually synonymous. But this relationship has in the main been broken, and we can assume that in our present highly automated society, age differences in intellectual performance, while statistically significant, may in absolute magnitude be so small as to have virtually no social consequence.

We are now ready to deduce public policy implications from what we now know about the intellectual functioning of the aged. In the light of what we have said thus far it is reasonable to conclude: (*a*) that much of the presumed intellectual decline of the old is a myth, (*b*) that there are indeed age differences in intellectual performance due to different educational exposure and unequal access to life support systems required to remain at an asymptotic level, (*c*) that the level of intellectual functioning of those of us who will become old will be higher than that of those who are old now, and (*d*) that manipulation of motivational factors related to intellectual performance in the aged is feasible.

The most directly discernible consequences of this proposition seem to me to be found in the areas of adult education, the issue of variable retirement, and the question of age segregation.

ADULT EDUCATION

The evidence on intellectual performance in old age clearly suggests that people can and do function at a high level throughout life, and thus can be expected to continue the educational process into very old age. However, contrary to past practice, it is not good enough to think of "retirement skill," hobbies, and the like in planning adult education for the middle aged and aged. A consequence of our finding of significant differences in levels of functioning between generations would be the development of specific educational programs designed to reverse the cultural and technological obsolescence of the aged. I would argue that it might be highly desirable to have "Head Start" types of cultural exposure programs for today's elderly to permit them to share more adequately in our present society. Upgrading job skills for the middle aged and second- and third-career education at many life stages should be in high demand if introduced in a meaningful manner.

But this may be the crux of the matter. What we have said about the problem of cohort-appropriateness of measures of intelligence applies equally to adult education in general. We must carefully define what sociocultural skills have been missed by today's aged, and we must structure materials and instructions in ways that are appropriate for the old. For example, since we know that the old will avoid risky situations given a choice, it might well be that we have to introduce some compulsory education requirements for adults also, or alternately to set up adequate reward con-

[2] Unpublished experimental tests from the Syracuse adult development study.

tingencies for voluntary participation. We must furthermore adapt current technology of information acquisition to the special problems of the old. That is, we must recognize that there is a high frequency of sensory problems in old people. Thus, special materials using large type faces, learning programs having more redundancy, and computer-aided instructional devices specially programmed for the slower response of the aged may all be needed.

VARIABLE RETIREMENT

The data about stability of intellectual performance until shortly before death certainly argue for a reexamination of mandatory retirement policies. I would like to repeat my argument here (Schaie, 1973d) that age is a totally inappropriate criterion for determining whether or not performance is adequate for a given vocational skill. But we do have enough information on age differences for various skills and their relation to occupational requirements to develop formal techniques for appraising whether or not retirement is desirable or mandatory. Such procedures have been used successfully for years in determining fitness for persons in critical occupations such as pilots. I believe that any support for a rationale that mandated retirement by chronological age because the latter was a useful predictor of the criterion of intellectual lack of competence must now be laid to rest. In fact, if we had to select a chronologically meaningful retirement age (say, using the same criteria that might have led to age 65 when the social security laws were first written), it would now have to be at least a decade later than would have made sense 20 years ago.

Even more important are the data on vast individual differences in individual patterns of development. Thus, individuals who are still engaged in intellectual growth, no matter at what age, are not ready to retire from active involvement with the work of the world, while those who are on the downgrade early in life should not be required to contribute what they no longer can provide.

AGE SEGREGATION

The evidence on the preponderance of generational variables in age differences might superficially suggest that segregation of different cohorts in their own communities might relieve intergenerational conflicts and might permit the provision of simplified environments for the aged individuals who early in life had been able to cope well when the environment they were required to deal with did not meet the complexity currently encountered. It is, of course, desirable to reduce the complexity of the environment for all of us and to build life support systems, but the aim here should be to build complex support systems to permit well-differentiated patterns of behavior, rather than reduce the stimulation provided by the environment. It is not unreasonable to predict that more detailed analyses of an individual's microenvironments will show that maintenance of cognitive function is significantly affected by the complexity of the world we live in.

Indeed it may be argued that the difficulties encountered by the aged due to progressive cultural obsolescence can best be countered not by greater age segregation but by intervention early in life to reduce age segregation. That is, we need to rebuild our communities in ways that permit greater contact between the generations. We need to provide other than child-centered recreational and entertainment facilities. We must break the generational gaps by permitting opportunities for children and old people to interact without demanding a return to the extended family structure. Old people (and people of any given life stage) stick together not necessarily because they have common needs and concerns, but because they have had common previous life experiences not shared by others. But this fact is indeed a consequence of age segregation, rather than the justification for it.

Conclusion

We have suggested that the major finding produced in the gerontology laboratory in the area of intellectual functioning is the demolishing of the myth of serious intellectual decrement in the aged. Related to this were issues regarding risk-taking behavior and intellectual performance and the appropriateness of assessment devices in cross-cohort comparisons. From these findings we suggested that there are policy implications for many areas but specifically for revisions of thinking in the programming of adult education, as evidence for the desirability of variable retirement policies

and as evidence against the desirability of age segregation.

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Translations in Gerontology—From Lab to Life

Psychophysiology and Speed of Response

JAMES E. BIRREN *University of Southern California*[1]

This article states some principles and gives a perspective on research findings on aging that lie behind some of the daily life circumstances of growing old. Usually a science and real-life dialogue such as this is caught in a division between a humanistic orientation and the empirical scientific traditions or what one might call the mechanistic traditions. My proposition is simply that laboratory research on the psychology of aging is a potential friend of the aging adult and that we ought to utilize the findings of the laboratory for the benefit of an aging population.

Much of my own research and that of several of my colleagues has been devoted to the study of the differences that occur with advancing age in speed of response (Birren, 1963, 1965). The issue is, How is such research relevant to the circumstances of older persons? I believe it has several points of relevance. For one thing, older people are strikingly affected by accidents. Accidents occur to older people whether they are pedestrians, drivers of cars, or in their own homes utilizing the normal appliances in everyday life. In particular, older people hurt themselves through falls, often fatally. Sometimes it would appear that the older person lacks the speed and agility to execute an evasive action in the face of an impending accident. Sometimes the accident-avoiding action has a limiting physical quality about it; that is, the younger person would or could move or jump out of the way more quickly and thus avoid an impact. Sometimes the action also has to do with time taken to scan one's environment and make a decision rapidly enough to take appropriate action. Observation of an older adult can suggest that attention may become sluggish and shifting from task to task occurs more slowly than for the teenager whose mind appears to dart rapidly.

I think you might agree that following an older adult driver suggests that aged behavior on the road has some different qualities than the behavior of a younger driver. In their favor it must be said that older drivers rarely drive too quickly. Offsetting this is a tendency to unpredictable behavior and making errors of judgment as to when a turn is appropriate. My fundamental proposition here is that some of these qualities of behavior shown with advancing age are intimately involved with the speed of processing information by the nervous system.

Time as a Variable in Behavior

Usually one builds up to a principle by a review of the literature; however, in view of the nature of this article I wish to state my basic proposition at the beginning, then trace its development and implications. My basic proposition is that with advancing age individuals on an average show a tendency to a slowness in response which reflects a basic change in the speed with which the central nervous system processes information. For some reason this principle seems difficult for psychologists to accept, and indeed I note my own students after spending several years in study with me sometimes miss the main point as well as its implications. The main point is that with age any event processed by the nervous system takes longer. Whereas in the young adult speed of response is usually related to such things as familiarity, uncertainty, and motivation, a limiting factor

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Requests for reprints should be sent to James E. Birren, Andrus Gerontology Center, University of Southern California, Los Angeles, California 90007.

emerges in the older adult, that of processing time. I would like to introduce an analogy to make my principle clear.

If in this period of energy shortages one has an electrical brown-out, one will discover that many of the electrical appliances used in the home will run more slowly. The voltage drop results in a slowness of motor-driven activities. If one considers a calculator that is running with a slower motor during a voltage drop, any process the calculator was used for, such as addition, multiplication, subtraction, or division, would be slower than usual. Large computers are often characterized in terms of their unit processing time; the faster the time the larger the capacity.

I want to go a step further to extend my analogy with respect to an aging human organism, with the expectations that I am probably right but I may be wrong. General observations suggest that the aging individual can do most if not all of the things he did when he was younger but not do them as quickly. Often there is a tendency to want to regard the quickness of behavior as not an intrinsic property. The technical sense that I am suggesting is simply that speed of behavior in the young adult is commonly regarded as a dependent variable; it depends on other circumstances surrounding the performance. In the case of the aging individual I believe that speed itself becomes the independent variable with which we must interpret and explain other kinds of behavior. This has to do with the proposition that if there is a central slowing of processing or mediation then all phenomena we regard as important in behavior will slow down. Perception and memory will become less efficient, as will retrieval from the long-term store of previously learned material. Furthermore, the likelihood of novel associations will decrease.

Young adults appear to be quick or slow in their behavior in accord with the needs of the situation in which they find themselves. On the other hand, older adults seem to have a characteristic or general slowness of behavior, an expression of a primary process of aging in the nervous system. While the precise location or the physiological basis of this slowness is not known, its consequence of behavior is becoming more apparent. Although I appear to be saying that there is a generalized brown-out in behavior with advancing age such that speed of intrinsic events of the nervous system is a limiting function, it should not be assumed from this that it had to be inevitable. As some of my colleagues are pointing out (Schonfield and Schaie, this issue), the range of individual differences is considerable, and one might make the obvious statement that the speed of an event is not necessarily the determiner of the pleasure we derive from it.

Historical Position of Time in Relation to Behavior

Early psychologists and physiologists differed in their approach to timing of behavior with advancing age. In the second edition of Cowdry's *Problems of Ageing*, Malcolm Critchley, a neurologist, reviewed the nature of the changes in the nervous system with advancing age. He noted that there was a change in reflexes, a kind of sluggishness in response. Also he noted that with regard to motor activity "a slight generalized poverty of movement, is characteristic and is usually associated with slowness of movement, an attitude affliction in a mild state of hypertonus or—better perhaps—difficulty in relaxation [Critchley, 1942, p. 523]."

In the same volume, Walter Miles pointed out that speed of movement did appear slower in his older subjects. He tended, however, to keep separate the data on speed of motor movements and perception from the information about intelligence and age. In discussing intelligence he said,

> Speed of response is . . . dependent to no small extent on the physiological equipment available for psychological perception and response and it is with interest but without surprise that we learn that when the speed factor is eliminated the coefficient of age–score correlation for intelligence during the life span of adult maturity drops from an average of .4 or .5 in various more or less homogeneous groups to .3 or less for a single somewhat heterogeneous population [Miles, 1942, p. 769].

Miles, I believe, was hoping that in partialing out speed it would leave intelligence uncorrelated with age. In fact, by so partialing time out one may be throwing out one of the most intrinsic factors of intellectual activity, speed of association. Miles, as well as other psychologists, had been embarrassed by the data on speed and aging which seemed to suggest that the older organism was not efficient when data on verbal skills showed no decline or perhaps an increase.

Somewhat earlier than Miles and Critchley, Pavlov had observed that it was more difficult to establish conditioned responses in older dogs; he generalized his observations to suggest that with advancing age the neural processes were less lively.

This somewhat metaphorical way of expressing the relationship does, however, imply that the nervous system was slowing down. An earlier psychologist, Thorndike (1928), had maintained that speed was a different property of mind along with power and attitude in the consideration of effective intelligence. Both he and his students, however, tended to neglect speed as though it were a peripheral matter. In Thorndike's day, age 45 was old, and one of his interests may have been to assure that people in their 30s and early 40s could still learn effectively. Presumably we would now move up this age considerably. Following Thorndike, his students argued that timed psychological tests were particularly embarrassing to older persons. Given a certain slowness in responding, older people will not answer as many questions on an intelligence test within a fixed time limit. By removing the time limits, performance becomes higher and presumably better reflects the true intellectual power of the individuals. Herein lies the fallacy, however. One did not know 30 years ago whether an intelligence test taken with or without time limits was a more valid indicator of that elusive quality we call intelligence. One had a tendency if one were a psychologist a generation ago to regard this speed as somehow peripheral or distant from the important qualities of the mind. In the parlance of the learning investigators, speed was regarded as a "performance factor" somehow not intrinsic to the process itself. What a strange kind of nervous system was implicitly being postulated as though the vital neural junctions leading up to the response or indeed the operation of the motor neurons in the spinal cord as well as the descending tracts from the cortex which carry the neural activity necessary in voluntary movement were uninvolved. It seems most unparsimonious to regard the operation of the peripheral neural elements as independent of the higher processes in the aging nervous system.

Magladery (1959) pointed out the effect that the higher centers of the nervous system have on simple reflexes:

> The results emphasize, rather, potential prolongation of relatively low level responses when potentiation from higher centers may be diminished In the case of monosynaptic reflex discharge, and the processes subserved by such, this increased delay can not be of great functional significance in terms of any one motorneuron pool. When cumulative, however, and particularly when processes such as these are operative over complex spinal reflex arcs which are more dependent for excitation than single synapses on influences from higher centers, these increased central delays may be very important indeed. They may well be sufficient to account for good proportions of the slowed motor response times in the elderly [Magladery, 1959, p. 181].

Thus, we have a rather unique situation in which the neurologist and the neurophysiologist suggest that slowness of responses becomes increasingly important as the responses involve higher centers and the psychologist wants that slowness to be restricted to the periphery and limited to such things as movement and not to involve processes of thinking. This indeed, I believe, was wishful thinking on the part of the early psychologists, when they hoped that older persons would simply be slow in their behavior but that the processes of mind remained unchanged, that is, such processes as thinking, reasoning, perception, and recall from memory. However embarrassing it may be to us as individuals, it seems that we have an obligation as objective observers to tell ourselves and our fellowmen how it really is as we grow old and indeed how the nervous system itself changes and influences behavior.

From a physiological standpoint it would appear that the more complex the behavior and the higher the level of the nervous system involved, the greater the slowness in the response with age. It is possible that a central process associated with the ascending reticular activating system might lead to the observed slowness, but it would seem to be an unrealistic position to hold that the reduction in activation and alerting would affect only the voluntary or pyramidal motor system. I would counter this line of reasoning with the observation that given a rapid decay system in memory, a sluggishness in processing information would preclude the opportunity of using decaying information in combination with other stimuli. In brief, I suggest that man is not simply a calculator that is slowing down because of an electrical brown-out, but man is a calculator that has some components that decay with time such that if the events are not completed within a time limit the possibility of the task being completed successfully is diminished. This leads us now to consider the role of diminished speed in the various adaptive capacities of the organism's perception, memory, learning, intelligence, and perhaps personality.

Perception

A variety of past as well as recent studies have shown differences with age in perceptual functions,

particularly vision. For example, it has been known for quite some time that the flicker fusion threshold increases with age. That is, a flickering light is regarded as a continuous light at a lower frequency by older subjects than by younger subjects. This suggests that the neural events may not be quick enough to allow discrimination of the series of events (Weiss, 1959). More recently this has been shown elegantly in a perceptual masking experiment by Donald Kline (1972). In his study of monoptic visual masking, a second stimulus defined as visual noise followed the first stimulus by a variable interval. The effect of the blocking or masking stimulus occurs at longer intervals for the older individual. It was also shown that in order to be perceived, the minimum exposure time of the initial or target stimulus was longer for the older subjects. One is left with dual phenomena in which the excitation corresponding to the initial stimulus takes a longer time to reach its maximum effectiveness as well as to decay, as shown by a longer masking interval. Both of these findings suggest some loss either in Pavlov's terms of neural liveliness or speed of neural events with age.

If one begins with the notion that neural events are slower with age and uses this as the explanatory independent variable, then the perceptual phenomena would be subsumed as part of a slowness in general processing time.

Memory

Not only are perceptual processes likely influenced by a slowness in central nervous systems processing, but memory also. In 1965 I reported some limited data on the relationships of tests of memory to measures of speed of writing digits. An important result in the present context was that the correlations between the Wechsler Memory Scale and speed in young subjects was not significantly different than zero. In fact, it was −.01. In elderly subjects, that is, those over 60 years, the correlation between the Wechsler Memory Scale and speed of copying digits was .52. While the content of the Memory Scale may be difficult to define in the context of laboratory learning studies, it does indicate that with older adults the intercorrelations between speed measures and such tests of memory increase.

More recent studies of memory in the context of verbal learning (Johnson, 1973; Robertson, 1972; Schonfield & Robertson, 1966) indicate that the largest difference with age in memory functions is not in the storage of information but in retrieving it from store. It is shown that recovery of information from long-term storage is best done by recognition rather than by total recall. Furthermore, Johnson's experiments showed that cued recall benefits disproportionately the older person. While it is premature to directly implicate an effect of slower central processing time, it is not premature to speculate about its role.

One possibility is that if neural events are indeed slow, the scanning of currently presented information and information from long-term store may be slow. Under these circumstances the individual may show a decreasing probability of eliciting the appropriate material from long-term store and the high probability of distraction. The set or search criterion may decay during a protracted process of retrieval. Robertson (1972) in her studies indicated that older subjects do retain material but frequently will allow this material to intrude as inappropriate associations; that is, memory itself is less affected than its retrieval at an appropriate time. An alternative interpretation is that older subjects do not employ mediating processes to the same extent as young persons. Under this circumstance the aged are disproportionately aided when cues are supplied to them because they do not themselves tend to use cues in both the original learning and perhaps in the recovery from the memory store. Further research in this area is needed to either refute or substantiate the role of speed of central nervous system processing in both primary and secondary memory processes and in scanning and retrieving. The proposition being favored here is that the primary change is in speed of neural events which influences memory. Put in other terms, we do not remember because our associations are slow, not that we are slow because we cannot remember.

Learning

Presumably, if perceptual and memory processes are influenced by changing central information processing, then learning itself must be influenced. Considerable research has been devoted to the issues of pacing in learning. Results indicate that older subjects should be given longer intervals between stimuli to elicit a response. If crowded by short intervals, they failed to respond, and one notes a rise with age in omission errors rather than

commission errors. This recently has been brought into question by Robertson's work which indicates that both errors of commission and omission occur. Apparently whether one commits more or less errors of commission is a function of the particular experimental context.

In learning experiments the issue may be discerned as to whether older subjects want more time or need more time. Given the interpretation that they are cautious and seek more certitude before they respond, they want more time Taking the interpretation that the central processing time is limited, then they need more time. In the case of perceptual and memory processes it would appear to the writer at least that the older individual needs more time rather than wants more. In the case of learning the situation is not so clear-cut. One of the uncertain areas is how to interpret the evidence that the older individual is relatively better in incidental learning than in directed learning (Johnson, 1973; Wimer, 1960). The fact that older subjects have a more difficult time in intentional or directed learning would suggest some difficulty in controlling attention or set. Thus, in the learning situation the older adult is taking in too much information and therefore cannot concentrate on crucial information most relevant to the task. Here one can point out that some very illuminating experiments could be carried out at the present time on relationships between attention processes and speed of information processing in relation to learning and age. Given a change with age in the central nervous system such that information processing is slower in all aspects, then attention itself should not be spared. Indeed, the establishment and maintenance of a set should be more difficult in the older individual. There is in fact some evidence for this in the reaction time studies of Botwinick (1957). He found that for expected stimuli, set is more difficult to establish and maintain in the older individual. Future research should be directed at the relations of slowness in CNS function with age and attention.

Motor Skills

Considerable research from the Cambridge Laboratory established the fact that the greatest difference between young and old adults in slowing of motor skills occurred at decision points. That is, movement itself was not so much changed with age as was the redirecting of movement in response to new information (Welford, 1959). This of course fits the earlier expressed idea that the higher one goes in the centers of the nervous system the more the slowing becomes evident. While repetitious movements such as tapping are slower with age, it is in the redirection of movements that the additional time is taken (Rabbitt & Birren, 1967). Thus sequential tasks that require intervening decisions are disproportionately lengthened with age in contrast to simple repetitious movements. This of course fits the thesis that the change in central processing time is the critical one with advancing age.

Intelligence

Much of the newer research findings on intelligence with age are showing significant cohort differences or differences between the generations in intelligence. Indeed the review by Schaie (pp. 802–807, this issue) amply documents this. Nevertheless, despite the large differences in cohorts there is still a difference between verbal tests and those involving perceptual speed, the latter still showing decrements in an older population. The study of healthy older men at the National Institute of Mental Health reported differences in both speed measurements and on nonverbal intelligence measures such as the Raven Progressive Matrices, in contrast with vocabulary and information tests (Foulds & Raven, 1948). Horn and Catell (1966) commented about the fact that crystalized intelligence representing the store of information acquired over time continues to rise with age whereas fluid intelligence, a more dynamic catalytic cognitive property, declines. Schumacher (1970) emphasized these points and drew inferences about altered physiology of the nervous system that may account for this. It would of course fit the hypothesis advanced here that fluid intelligence should decline because it would represent a decline in the rapidity with which one can scan stored information and recombine it with current input for a needed and perhaps novel response.

Earlier research by Chown (1961) in which she correlated tests of rigidity with age and with intelligence indicated that with advancing age there was a tendency to show greater rigidity. It was difficult to interpret the rigidity outside of the context of cognitive functioning itself; that is, the central change may lead to cognitive deficit and to the apparent changes in rigidity. In this sense,

then, rigidity may be less a personality predisposition or a reversible set but rather a required difference in behavior due to the changes occurring within the nervous system itself. That is, many older individuals do not want to be rigid but are rigid because of a necessary consequence of the way the older nervous system functions.

Timing and Its Relation to Neural Activity

One of the current dilemmas is how to interpret the tendency for older subjects to show a lowered alpha frequency with advancing age (Obrist, 1965; Thompson & Marsh, 1973). Surwillo (1963) interpreted this as reflecting a change in the basic timing in the nervous system and, in fact, regarded the alpha frequency as a pacemaker of behavioral processes. Subsequently, the biofeedback conditioning experiments of Woodruff (1972) indicated that to some extent one could reverse the drift toward lower frequencies of alpha in the older subject by appropriate biofeedback conditioning and this would be correlated with the faster timing of responses. While the correlation between response speed and alpha was significant, the data suggest that most of the variance attributable to the relation between age and reaction time is unexplained by the change in alpha per se. Nevertheless it does show a linking in speed of response with neural activity.

Eisdorfer (1968), in his experiments with learning and autonomic nervous system activity, has emphasized that with advancing age there tends to be an overarousal of the sympathetic nervous system or, more particularly, increased activity of the peripheral autonomic end organs. This would seem to lie in contrast with the hypothesis that the central nervous system was showing a quality of underarousal. The unpublished evidence of Jeffrey shows that older subjects will improve their speed of response given amphetamines or stimulating adrenergiclike drugs. There remains considerable uncertainty as to the relative contributions of central versus peripheral sympathetic effects on age changes in behavior. Earlier, Birren (1963) reported data which indicated that elevated blood pressure was associated with slowness in response, thus a balance of central nervous system activity and peripheral autonomic activity was implicated. Increased sympathetic activity may reflect a relative inhibition or underarousal of the central nervous system with regard to facilitation of perceptual and motor responses. Thus the issue that older subjects on the average may show increased sympathetic activity may not in itself deny the possibility that there is a concomitant slowness in central information processing.

Implications of Data on Timing, Behavior, and Aging

For a considerable period of time behavioral data on aging appeared to be very compartmentalized, and very little thought was given to the possibility that there were any major common underlying processes. More recently, however, there seems to be recognition of the fact that with age there may be some general changes in behavior due to common processes. An important study was carried out in the Netherlands using 150 psychological and physiological variables (Dirkin, 1972). The measurements were obtained on 316 male workers in Dutch industry. Of interest was the fact that the cluster analysis of the main variables included psychological as well as physical variables. Investigators concluded, "This can be considered as an indication that the somatic and mental aspects of aging are highly interdependent, it even confirms that aging is controlled by a general process, influencing all subsystems simultaneously [Dirkin, 1972, p. 209]." With this encouragement one may regard the changes with age in central processing time as the major independent variable in explaining much of the behavioral changes with age. However, the timing itself becomes dependent on other physiological phenomena, particularly its relationship to somatic disease. As mentioned earlier there is a correlation between slowness of response and blood pressure such that the higher the blood pressure the slower the response. Also subjects with cardiovascular disease show a disproportionate slowing in information processing relative to others their age. One of the more recent developments in this area is linking slowness of response to predisposition to cardiovascular disease.

Rosenman has established the association of a personality style with the likelihood of developing cardiovascular disease, specifically coronary artery disease (Rosenman, Friedman, Jenkins & Borner, 1968). Subjects who show a constellation of time pressure, restlessness, responsibility, and many diffuse goals have a much higher likelihood of developing coronary disease than individuals who are

not so time pressured. Taking this evidence as a point of departure, Abrahams (1972) studied a group of young adults and classified them according to their predisposition or nonpredisposition to cardiovascular disease. Reasoning that later in life slowness of behavioral functions is related to cardiovascular disease, he thought that individuals early in life that were so predisposed would show slowing. Indeed, his results do bear this out. The fact that the reaction time of subjects in their early years who are predisposed to coronary heart disease is significantly slower than their nonpredisposed peers would indicate that there is a common basis for the predisposition to cardiovascular disease and the timing in the central nervous system itself. Slowness of information processing with age is an issue directly involved in questions about the basis of somatic changes with advancing age.

The data on slowness of reaction time with age have direct implications for how older adults can manage themselves in their environment. While response times of young adults may be slow or quick depending on the demands of the environment, older adults are slow as a general feature of their behavior. Furthermore, since disease is intimately related to the slowness shown by older adults, the view is advanced that while age appears to be accompanied by normal psychophysiological slowing it is exacerbated by the presence of disease, particularly those diseases of a stress character and involving the sympathetic nervous system.

Summary

One application of available information is that we may begin to attempt to identify individuals early in life who are predisposed to coronary heart disease by behavioral assessment. Subsequently, by suitable conditioning it may be possible to minimize the coronary risk that these individuals are to incur. Psychology would appear to have an important contribution to make to the early detection and management of somatic disease.

Research results to date have clear implications for the design of the environment to maximize the potentials of the older adult. One of the largest areas of potential contribution of psychologists is likely to be in the management of accidents, which are so disastrously high in frequency for older adults. Evidence suggests that it is not so much the motor response or muscular strength itself which leads to accident-prone behavior in the aged but rather slowness of decision times and the inability of the older person to rapidly discriminate relevant information from irrelevant information. Automobile accidents and pedestrian accidents often imply failure of judgment. One may speculate that the time taken to scan traffic flow, traffic lights, and other information and to arrive at relevant decisions at an appropriate time is often not possible given the timing characteristics of the older nervous system. A pacing of environmental events which is appropriate for the 25-year-old is not appropriate for the average 75-year-old to permit effective behavior. Time and pacing of the environment, particularly an urban environment, are commonly hostile to optimum adaptive behavior of the older person. While the environmental principles that are sound for older adults may also be sound for young people, such as minimizing irrelevant information and maximizing relevant information by important judgments, the basic issue of time itself is not readily resolved. That is, safe street crossing by an older pedestrian may require twice the length of time that it will for a young adult. Given the impatience of motorists, the young drivers' timing and the timing of young pedestrians dominate the present scene. The urban microenvironmental situation is harried for the older adult, and time itself is a key element.

As a point of departure for further research on the older nervous system I can think of no greater vantage point than studying speed and timing. Such information has prospects for considerable scientific gain in understanding the organization of behavior and for the design of an environment that will be supportive of older adults.

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Token Rewards May Lead to Token Learning

FREDRIC M. LEVINE *State University of New York at Stony Brook* [1]
GERALDINE FASNACHT *State University of New York at Stony Brook*

In the past 10 years or so clinical psychology has, with great pride, increasingly embraced the operant tradition of experimental psychologists. Behavior modifiers have learned and demonstrated that a wide variety of behaviors can be changed by the application of operant procedures in clinical settings. We have been involved in and read about the vast changes of schizophrenic behavior on a ward, children's temper tantrums, seizures, etc., by the judicious application of contingent reinforcement. It is clear that reinforcement works and that an operant analysis of behavior provides a powerful and useful tool for clinical intervention. By changing contingencies, the therapist can often produce rapid changes in disturbing behaviors.

Many programs have been using "token"-type programs to produce behavior change. The usual paradigm of these programs is to give the subject some material reinforcer for a predetermined response. Sometimes the reinforcer is a token or a point which can later be turned in for a prize of some sort. A child may, for example, earn 10 points in a week for completing his daily homework. At the end of the week he can trade these points for a desired toy. For the purposes of this article, we treat token programs as those that use reinforcers for predetermined responses.

Because of the dramatic success of token approaches and because of their relative simplicity, there is a growing tendency to utilize these approaches in more and more settings. Both education and industry seem determined to squeeze more "appropriate" behaviors out of their populations by reinforcing the "right" response. The gold star and the key to the executive toilet will be issued immediately only after the appropriate behavior is emitted.

There are numerous examples of this recent rush into the use of token reinforcers. Research Media, for example, is advertising a course in the use of tokens which can be used for, among other things, "improving both quantity and quality of academic performance. Coping with behavioral problems. Increasing productivity. Enhancing interpersonal relationships. Motivating personnel. . . ." Patterson and Gullion (1971), in a widely used manual for parents and teachers, advocated the use of token procedures to eliminate the negativism of "the 'I don't want to' child," to quiet "the overly active, noisy child," and to calm "the frightened child." A recent paper (Hauserman, Walen, & Behling, 1973) demonstrated that a token system (while in effect) increased racial integration of first-grade children. Although the immediate effect of these programs may be positive, we have trepidations about the long-range value.

Based both on theoretical considerations and on practical experience, we feel that token approaches will do more harm than good when applied in what is an increasingly promiscuous manner. In the words of the unacceptable medical model: The cure may be worse than the disease.

Within the experimental-behavioral approach, there is another growing tradition: the cognitive approach of attribution theory. Attributional theorists claim that how we attribute the causality of our behavior will in turn determine future behavior. Whether we perceive we are doing an activity for pay or for joy will influence how highly we will value the activity. An impressive amount of data from attribution type research has accumulated which indicates that giving a reward can have an effect counter to that desired. Giving points, tokens, M&Ms, etc., to a child for learning a lesson

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Requests for reprints should be sent to Fredric M. Levine, Department of Psychology, State University of New York at Stony Brook, Stony Brook, New York 11790.

will decrease the intrinsic satisfaction of the lesson. The first author, while in his uncritical operant days, had the rare experience of watching his five-year-old son keep his room tidy for several days. In the best of operant traditions, the author gave his son a toy as a reward for tidiness. Not only was the room never voluntarily cleaned again, but a reward was demanded for many other once-routine responses: "Will I get a reward if I give you a hug?" etc.

Most college students like Soma blocks, a puzzle of various shaped blocks which, if correctly placed, can be made into certain shapes. Deci (1971, 1972) found out how the fun can be taken from the blocks. He asked students to manipulate the Soma pieces to reproduce four configurations that had been drawn on paper for them. If the subject could not solve a puzzle in the 10 minutes allotted for each configuration, the experimenter stopped him and explained how to do it. After completing the puzzles, the subject was left alone in the room for 8 minutes; he was free to do anything he liked, for example, read magazines or work on the puzzles. Intrinsic motivation was measured by the amount of time the subject spent working on the puzzle. Half of the students were told at the beginning of the experiment that they would receive $1 for each correct solution and, therefore, a maximum of $4 if they solved all of the puzzles correctly. The other half of the students were not promised any rewards. Those students who were paid spent significantly less time working the puzzles during the 8-minute period when they were left alone than did those who had received no reward for doing the puzzles.

Lepper, Greene, and Nisbett (1973) used an educational setting to assess the effects of extrinsic reward on preschool children. The amount of time children spent at a drawing activity while other activities were going on was used as the measure of intrinsic value of the drawing. Lepper et al. used three groups of children. One group agreed to perform the drawing activity for a reward certificate. A second group took part in the same activity without prior expectation of reward although they were rewarded after the task. The no-reward control group neither expected nor received a reward. Experimental sessions were conducted outside of the classroom. The expected-reward group showed less interest in the activity when it was available in the classroom at a later time. The paradox again emerged in which rewarding a response led to a decrease of the response when the reward was withdrawn. In addition, interest increased slightly in the group that did not get any reward. Lepper et al.'s conclusion was that token economies, and extrinsic reinforcement in general, tend to undermine interest in those cases in which interest is already at a high level.

Kruglanski, Alon, and Lewis (1972) studied the relationship between external rewards and task enjoyment. The 10–11-year-old members of winning teams in a series of group competitions (Follow the Leader, Word Construction, Song-Matching, Discover the Rhyme, Speed Writing) were either rewarded with a prize or received no prize. The prize group had not received an initial promise of a reward. Later the children responded to questionnaires about their enjoyment of the games and their reasons for participating when other activities were available. The prize group rated their tasks as less enjoyable than the no-prize group. Those children who said they had performed the task for a prize enjoyed the tasks less.

In another study by Kruglanski, Friedman, and Zeevi (1971), one group of 15–16-year-olds was offered an extrinsic incentive for performing several recall and creativity tasks. The incentive consisted of a visit to the psychology department, a reward that had been previously determined to be attractive to the subjects. A second group was not offered an extrinsic incentive. Not only were recall and creativity higher in the no-incentive group, but ratings of task enjoyment were also higher for the no-incentive group as compared to the extrinsic-incentive group. A higher quality of both performance and motivation was found in the absence of external rewards.

Data on unexpected rewards have shown conflicting results. Kruglanski et al.'s (1972) unexpected-prize group rated their tasks as less enjoyable than the no-prize group. On the other hand, Lepper et al. (1973) found that children in their unexpected-reward group displayed an undiminished or increased interest in the drawing activity. As yet, no conclusions can be drawn from the data. If unpromised rewards should be found to have an incremental effect on interest, the effect cannot be repeated often: An unexpected reward is only a surprise the first time it is used.

As applied to token economies, the results of these studies indicate the trepidations the practitioner should have before instituting a token program. There should not be an attempt to use external rewards for behaviors that are of some intrinsic interest. If children are engaged in class-

room activities, adding rewards for learning should have the effect of increasing the learning while the rewards are used. However, the longer range consequences of using external rewards for responses that indicate interest in the activity are clear. The token rewards will lead to a decrease in interest.

An attributional explanation is often given for the finding that the intrinsic pleasure of an activity can be reduced by giving extrinsic reward for the activity. The attributional explanation (Bem, 1967) usually is in the form of the following: If one is doing activity X without a reward, then activity X must be worth doing. If one is getting a reward for activity X, it must not be worth doing without the reward.

However, in 1950, Gately, one of Harlow's master's students, reported results in monkeys which were startlingly similar to the "cognitive" findings. In the now famous hasp and latch puzzle (which led Harlow to question traditional drive reduction theory), there were two groups of monkeys. For one group the puzzles were baited with raisins; for the other group there were no food rewards. The rewarded group as a whole performed better than the manipulation-only group. More importantly, the unrewarded monkeys continued to display interest in the puzzles throughout the testing session while the rewarded monkeys lost interest in the puzzles as soon as they removed the food. Again, the group of monkeys who did *not* get food as a reward for solving the puzzle was the group who played with the puzzle with undiminished interest. Perhaps attentional variables are the key. Reinforcement may shift attention from the activity to the reinforcer.

The applied implications of the above research are clear. In our haste to demonstrate that learning can be increased, productivity raised, and manners improved, we operant-oriented therapists may, in the long run, be decreasing the frequency of the very behaviors we wish to increase.

Another question of considerable interest is what happens to those reinforced behaviors after tokens are withdrawn? What are the long-range consequences of the use of tokens? The hope was that tokens would "prime the pump" and allow natural reinforcers to take over after the desired behaviors were well established. It was expected that generalization would take place. Despite this hope, there is little acceptable evidence for tokens having this desirable effect. There are some recent claims that generalization exists (Cohen & Filipczak, 1971; Drabman, Spitalnik, & O'Leary, 1973; Patterson & Reid, 1973). Cohen and Filipczak (1971) found an initial drop in recidivism rates for their group of juvenile delinquents in the first year after a multipronged training program which included token reinforcement, programmed instruction, and individual academic help. Because of the multipronged approach, it is difficult to ascribe the reduction in recidivism to the tokens. In addition, the data indicated that the recidivism rates would be at the level of other juvenile delinquents within three years. Patterson and Reid (1973) claimed generalization over time in improvements in the behavior of aggressive children. Global evaluations obtained by telephone calls to the parents after termination of treatment indicated marked improvements in the children's behavior. In the absence of a control group, and without independent verification of the telephone procedure, the data cannot be considered as reliable evidence for generalization. There are several recent studies which indicate that responses generalize to nonreinforced situations. Since generalization is ascribed to a failure to discriminate differences between situations (Rachlin, 1970), it is important that demonstrations of generalization be of some duration. Craighead, O'Leary, and Allen (1973) claimed generalization which persisted several days. This is not sufficient evidence for such a clinically important conclusion. O'Leary, Drabman, and Kass (1973) concluded that after an effective token program was withdrawn in a special classroom, the appropriate behavior was maintained during a 20-day follow-up period. The authors reported a reduction from an average of 1.27 disruptive behaviors per 20-second interval to an average of .54 during the token program. An average of .56 disruptive behavior occurred during the follow-up. Although these figures seem to indicate generalization of appropriate behavior after the tokens were removed, a closer inspection of the data indicates that the greatest reduction of disruptive behavior occurred before reinforcers were used, during the initial baseline period. Possibly due to observer effects, the level of disruptions at the end of the baseline period did not appear to differ greatly from that during the token program and subsequent follow-up. In addition, there was no significant generalization of the appropriate behavior to the regular classroom.

Token programs that have been in effect in either the mornings or afternoons have not shown generalization to the part of the day in which tokens are not used (Becker, Madsen, Arnold, &

Thomas, 1967; Kuypers, Becker, & O'Leary, 1968; O'Leary, Becker, Evans, & Saudargas, 1969).

Krasner and Krasner (1973), for example, stated that changes in behavior produced by tokens have not demonstrated generalization to stimulus conditions in which there are no tokens.

Behavior changes have not been maintained over time after "tokens" were withdrawn. O'Leary and Becker (1967) contacted the new teachers of children who had been in a token program the year before. Although the children displayed less disruptive behavior than prior to implementation of the token program, the frequency of such behavior was clearly greater than it had been during the token program. O'Leary et al. (1969) found that the frequency of disruptive behavior during a four-week follow-up was not substantially lower than during their "praise and ignore" phase of the program when tokens were not used. Walker, Mattson, and Buckley (1969) reported that their three- and six-month follow-ups indicated that changes in task-oriented behavior were not maintained when the children were returned to their regular classrooms. In fact, the school district asked for help the following year with 5 out of the 11 children who had been in a token program the year before. Lovaas, Koegel, Simmons, and Long (1973) found that behavior of autistic children regressed after treatment.

However, there should be no surprise that disruptive behavior bounces back when the tokens are removed. Removal of tokens constitutes an extinction operation, not a generalization paradigm. Subjects are simply being taught how to earn tokens. In a typical operant laboratory experiment, rats stop pressing the lever in a Skinner box when the food pellets stop coming. Why? The rats have learned how to earn pellets. They have not learned to value pressing the lever. In the same way, children in a token economy classroom are simply being taught how to earn tokens. Therefore, when the tokens are removed it is hardly surprising that disruptions recur. Once the tokens are removed, the children are on an extinction schedule.

Experimental psychologists have shown us several procedures for increasing resistance to extinction, for example, intermittent reinforcement, delay of reinforcement, giving noncontingent reinforcement during extinction. It is possible to use these procedures with tokens to increase resistance to extinction. Drabman, Spitalnik, and O'Leary (1973), for example, were able to obtain generalization of appropriate classroom behavior in disruptive children to nontoken periods by using a procedure which involved administering tokens for 45 minutes each day and randomly selecting 15-minute control periods during which no tokens were dispensed. However, once we start instituting various ways of increasing resistance to extinction, we are making our programs more complicated and less practical. Nevertheless, an extinction paradigm by another name still smells like an extinction paradigm.

Therefore, once token-type programs are instituted, it may never be possible to fully withdraw tokens without the behavior going at least back to baseline. Furthermore, since tokens tend to decrease the intrinsic value of an activity, they may actually do more harm than good.

We do not mean to imply that an operant view of behavior is not a useful way of dealing with behavioral problems. Unquestionably, operant approaches have real value in helping to treat those populations, such as children and inpatients, in which the therapists can have reasonable control over the environment. In nine years of clinical work with such populations, it seems that the operant view is most effective in those instances when inappropriate behavior is inadvertently being reinforced. Sometimes we may be surprised about what the reinforcer is. We have seen several children whose behavior is reinforced by an angry, punitive response by the parents. So many parents yield to temper tantrums, chase children who run away, give into violence, whining, and insults that we behavior modifiers need not fear unemployment. An operant analysis often allows the clinician to change the consequences of the behavior so that disturbing behavior is not reinforced while incompatible appropriate behavior is reinforced. A child who gets attention by nagging is often one who is ignored while not annoying his parents. There is no doubt which of these behaviors will increase. We can use our knowledge of operant procedures to help rearrange the distribution of parents' attention and cookies so that we can be genuinely helpful. Tokens were not necessary for the acquisition of problem behaviors nor for their maintenance; we should, therefore, first search for the "natural" reinforcers of problem behaviors as a significant point of intervention. However, we must also use information gained in the cognitive tradition. The time has come for us to avoid a narrow operant perspective. Operant procedures have their place and their dangers.

Because of the danger that use of tokens will decrease the intrinsic satisfaction of activities, they should be avoided unless there is a real danger to the person or there is no alternative; for example, a child with renal infection and poor urinary training would justify a token-type approach to avoid a fatal renal infection. If a behavior is of low base rate and its occurrence is important, token approaches may have to be used. Hospitalized psychiatric patients who are not toilet trained, for example, place great tension on staff, who in turn get angry at the patients. A general increase in quality of recognition by others will occur when the patient does not stink of feces. The tokens may have to become as much a part of the daily schedule as are meals.

Before extrinsic reinforcers are used, however, the therapist should seek ways by which the intrinsic value of the activity can be increased. As pointed out by O'Leary and Drabman (1971), teachers should look at the interest level of the curriculum before looking to reinforce learning of material that kids just do not like. Industry should view job satisfaction in a variety of ways before using token approaches. For example, Herzberg (1968) stressed that workers need to be useful and to have some autonomy over their activities. Adams (1963, 1965) pointed out the debilitating effects of inequity. The focus with these "normal" populations in which there is not impending danger should be in making the work more intrinsically satisfying. By automatically giving tokens for tasks, although the task will be completed, there is the risk that the intrinsic satisfaction of the task will be decreased. Tokens do lead to powerful learning, but the learning may, in fact, be token.

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An Overview of Psychology's Human Resources

Characteristics and Salaries from the 1972 APA Survey

C. ALAN BONEAU *American Psychological Association*

JANET M. CUCA *American Psychological Association* [1]

Several major issues involving the existing supply and the production of psychologists are of current serious concern. For example, a real prospect exists that academic jobs in departments of psychology will constitute a smaller job market than has recently obtained for new PhDs. This fact of life complicates the current concern to increase the numbers of women and minority group members on faculties and in the profession generally. As a basis for planning, information is needed about the availability of minority group members and women and about the distribution of ages of current academicians. Also required is a hard look at available supply information as a prelude to a reconsideration of training goals and recruiting practices. Of interest is the fact that the employment problem may be ameliorated by the increasing tendency of psychologists to be employed in social problem areas as a result of public policy efforts at the federal, state, and local levels, for example, in health programs and in criminal justice reform. The imminent phaseout of federal funding for training, however, raises questions about the long-run availability of psychologists for public service positions.

These issues pose critical problems for individuals who make decisions affecting the field of psychology—deans and department chairs, agency heads, Congressmen, and Congressional staff members—and for students who are making career choices. Information needed for these many interest groups includes the numbers of available psychologists at various degree levels; the fields of their expertise; their geographic, age, sex, and race distribution; their range of salaries; the number of individuals currently receiving training, and much more.

This article, part of a series of analyses of different aspects of psychology's manpower, takes a broad look at the total manpower pool in an attempt to capture the global outline: its present characteristics, the information specified above, its trend over time—past and future. Further reports will deal with major psychological enterprises such as human service, education, and research. Data for these analyses will come from many sources, but principally from the APA Manpower Data System to be described below.

From 1954 to 1970, the APA cooperated with the National Science Foundation (NSF) in the production of a biennial census of psychologists, the psychology portion of the National Register of Scientific and Technical Personnel (NRSTP). After the 1970 canvasing, the NSF terminated funding for the National Register program and attempted to develop alternative sources of information on the overall scientific manpower pool in the United States. Involving much less detail than the Register and with insufficiently large samples for most special breakdowns, these plans were viewed by the scientific disciplines as largely unsuited for their specific purposes. Consequently, the American Psychological Association, acting independently and using its own resources, decided to expand its man-

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[1] The first author is Director, Programs and Planning, of the American Psychological Association; the second author, Staff Assistant and proprietor of the Association's Manpower Information Clearinghouse.

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Requests for reprints should be sent to the Manpower Information Clearinghouse, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

power survey activities. A few other disciplines such as physics and chemistry chose a similar course. The APA effort, in the interest of economy and efficiency, was planned as a systematic program to interlock its membership files, information for inclusion in its biographical publications, and needed statistical information on manpower. The system as developed has been described by Willette (1972).

In the spring of 1972, APA mailed an extensive questionnaire to its total membership (35,361)[2] and to another 26,556 nonmembers selected primarily from the membership rolls of other psychological societies and associations but also including graduate students. The final return included responses from 27,371 members (a 77% response rate) and 7,990 nonmember respondents who were eligible for membership in the APA (out of a total 10,785 nonmember responses). This report provides a summary of data compiled from those returns.[3] For comparison purposes, it is in part cast in a form that resembles previous reports on psychologists in this Journal that were based on past NSF National Register data (Boneau, 1968; Cates, 1970; Cates & Dawson, 1971; Compton, 1966; Lockman, 1962).[4]

Because of equality-of-opportunity considerations, sex and race information have become important characteristics of the manpower pool. A significant departure from previous reports will be the inclusion of racial and ethnic identification. Many analyses, previously mute about sex, will provide sex breakdowns, particularly salary tabulations. Also, because of collaboration with the Canadian associations in gathering data, we have tabulated citizenship as a characteristic.

The most meaningful comparison of present data with that of the past is based upon the total sample—member plus nonmember data—because the National Register was not limited to members of the APA. In view of the expense involved in acquiring nonmember data, however, it is unlikely that it will prove feasible in the future to acquire nonmember information from APA resources on a regular basis. Future comparisons probably will be among data gathered from the membership of the Association. Thus, for the purpose of presenting a baseline for future member data comparisons, in addition to the presentation of the total sample data we have chosen to present some data on APA members only.

Having reviewed the somewhat mundane technicalities necessary to introduce the data, we are now in a position to review it. The initial summary of data is in a form similar to that of previous reports on the National Register, a simulated National Register report, if you will. This summary presents the numbers of psychologists in the respective samples having specified characteristics. Then there is a brief review of trends since the first Register in 1954. Then we take a closer look at the current (1972) APA membership, considering the educational level and the sex of the respondents as primary variables for comparisons. Initially the analysis concerns itself with the numbers of psychologists with various characteristics, and salary data are later presented. Then appears a short presentation of data on the employment patterns of APA members. Finally we engage in a brief discussion of the numbers of psychologists and the legitimacy of the APA as a voice for the totality.

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[2] This total includes individuals who were members at the beginning of 1972 as well as those who were elected to membership during 1972. Data are primarily from early 1972, but the new members provided information in the fall. Thus, in summary, we were considering individuals who received their degree in 1972 or earlier.

[3] The representativeness of the responses has been discussed elsewhere (Boneau & Cuca, 1973). For members, the response data have a statistically significant but seemingly trivial underrepresentation of nondoctoral individuals. Outside data lead to an inference, currently unsubstantiated by statistics, that the sample overrepresents human service providers and underrepresents the academic-research sector to a small but unknown degree.

[4] Notable differences between National Register and APA data, however, are disguised by such an approach and should be highlighted. First, the questionnaires and individual questions themselves were different, the APA version being specifically tailored to the problems arising in the description of psychologists. Second, the populations and probably the sample characteristics were subjected to unknown warpages resulting from several factors, but notably two: (*a*) the APA data were collected in collaboration with the Canadian Psychological Association and the Corporation de Psychologie de Quebec in order to create a "Consolidated Roster" of North American Psychologists, and (*b*) the participation of APA members in the APA survey was greater than in NSF canvases due, it would seem, to the tie-in with entries in the APA *Biographical Directory*. Other differences will be noted in passing. Qualifications for inclusion in the NSF Register and in the present APA compilation, however, are the same, namely APA membership or putative eligibility for membership. These qualifications for membership are a doctorate in psychology or a master's in psychology plus one year working in the field.

TABLE 1

Characteristics of Psychologists Who Were APA Members or Eligible for APA Membership Reporting in 1972 APA Survey

Characteristic	Members		Eligible nonmembers		Members and nonmembers	
	N	%	*N*	%	*N*	%
Total *N* responding	27,371		7,990		35,361	
Level of highest degree						
Doctorate	21,353	78.1	2,938	36.9	24,291	68.8
Master's	5,639	20.6	5,028	63.1	10,667	30.2
Bachelor's and other	346	1.3	1	0.0	347	1.0
Total reporting	27,338	100.0	7,967	100.0	35,305	100.0
Sex						
Male	20,930	76.6	5,529	69.3	26,459	74.9
Female	6,409	23.4	2,446	30.7	8,855	25.1
Total reporting	27,339	100.0	7,975	100.0	35,314	100.0
Subfield						
General	385	1.6	193	2.9	578	1.9
Systems, methodology, and technique	334	1.4	92	1.4	426	1.4
Experimental	1,841	7.8	770	11.7	2,611	8.6
Physiological	479	2.0	327	5.0	806	2.7
Comparative/animal	122	.5	46	.7	168	.6
Developmental	997	4.2	315	4.8	1,312	4.3
Social	1,310	5.5	312	4.8	1,622	5.4
Personality	410	1.7	98	1.5	508	1.7
Psychometrics	290	1.2	97	1.5	387	1.3
Educational	1,651	7.0	505	7.7	2,156	7.1
Engineering	257	1.1	118	1.8	375	1.2
Industrial and organizational	1,566	6.6	198	3.0	1,764	5.8
Clinical	8,447	35.6	1,590	24.2	10,037	33.1
Community	432	1.8	69	1.0	501	1.7
Counseling	2,750	11.6	778	11.9	3,528	11.7
School	1,918	8.1	901	13.7	2,819	9.3
Other psychology	528	2.2	154	2.3	682	2.3
Total reporting	23,717	100.0	6,563	100.0	30,280	100.0
Employment setting						
Hospitals	2,128	8.5	425	6.9	2,553	8.2
Clinics	1,738	7.0	359	5.9	2,097	6.8
Independent/group practice	1,688	6.8	109	1.8	1,797	5.8
Government agency	1,267	5.1	314	5.1	1,581	5.1
Private or quasi-government agency	232	.9	71	1.2	303	1.0
Research establishment	666	2.7	127	2.1	793	2.6
Private consulting firm	614	2.5	65	1.1	679	2.2
Business or industry	789	3.2	194	3.2	983	3.2
Association or society	108	.4	20	.3	128	.4
Religious institution	93	.4	28	.5	121	.4
Military service	141	.6	67	1.1	208	.7
Law enforcement agency	18	.2	2	.0	20	.1
Judicial system	34	.1	11	.2	45	.1
Correctional system	201	.8	74	1.2	275	.9
Other noneducational	134	.5	6	.1	140	.5
Subtotal noneducational	9,851	39.6	1,872	30.6	11,723	37.8
Universities, medical schools excluded	8,602	34.6	1,608	26.2	10,210	32.9
Medical schools	910	3.7	152	2.5	1,062	3.4
Four-year colleges	2,382	9.6	696	11.4	3,078	9.9
Two-year colleges	660	2.7	423	6.9	1,083	3.5

TABLE 1—(*Continued*)

Characteristic	Members		Eligible nonmembers		Members and nonmembers	
	N	%	*N*	%	*N*	%
Regional school district	964	3.9	599	9.8	1,563	5.0
Secondary school	460	1.8	213	3.5	673	2.2
Elementary school	618	2.5	342	5.6	960	3.1
Other educational	442	1.8	222	3.6	664	2.1
Subtotal educational	15,038	60.4	4,255	69.4	19,293	62.2
Total reporting	24,889	100.0	6,127	100.0	31,016	100.0
Departmental setting in education						
Psychology	7,051	50.2	1,502	36.9	8,553	47.2
Education	2,186	15.6	526	12.9	2,712	15.0
Sociology	106	.8	32	.8	138	.8
Social work	57	.4	25	.6	82	.5
Behavioral sciences	439	3.1	137	3.4	576	3.2
Medicine	910	6.5	152	3.7	1,062	5.9
Business	275	2.0	36	.9	311	1.7
Administrative staff	939	6.7	458	11.3	1,397	7.7
Student counseling center	1,717	12.2	1,038	25.5	2,755	15.2
Other	363	2.6	163	4.0	526	2.9
Total reporting	14,043	100.0	4,069	100.0	18,112	100.0
Type of employer						
Federal government	1,985	8.0	394	6.5	2,379	7.7
State government	9,303	37.7	2,386	39.5	11,689	37.8
Local government	3,209	13.0	1,379	22.8	4,588	14.8
Private, for profit	2,579	10.4	410	6.8	2,989	9.7
Independent, nonprofit	5,124	20.8	1,012	16.7	6,136	19.9
Self-employed	1,583	6.4	133	2.2	1,716	5.6
Religious supported	1,048	4.2	326	5.4	1,374	4.4
Other	29	.1	4	.1	33	.1
Total reporting	24,680	100.0	6,044	100.0	30,904	100.0
Work activity[a]						
Research	3,823	17.6	865	16.5	4,688	17.4
Application/practice	8,539	39.3	2,003	38.2	10,542	39.1
Teaching	5,258	24.2	1,356	25.9	6,614	24.5
Management/administration	4,091	18.8	1,014	19.4	5,105	18.9
Total reporting	21,711	100.0	5,238	100.0	26,949	100.0
Years of experience[b]						
<1	963	3.6	930	11.6	1,893	5.4
1–2	3,798	14.0	2,402	30.1	6,200	17.7
3–4	3,706	13.7	1,210	15.2	4,916	14.0
5–6	2,889	10.7	842	10.5	3,731	10.6
7–8	2,282	8.4	514	6.4	2,796	8.0
9–10	1,942	7.2	359	4.5	2,301	6.6
11–12	1,619	6.0	310	3.9	1,929	5.5
13–14	1,543	5.7	212	2.7	1,755	5.0
15–16	1,445	5.3	216	2.7	1,661	4.7
17–18	1,399	5.2	187	2.3	1,586	4.5
19–20	1,435	5.3	170	2.1	1,595	4.6
21–25	2,102	7.8	292	3.7	2,394	6.8
26–30	553	2.0	101	1.3	654	1.9
31–35	598	2.2	96	1.2	694	2.0
36–40	405	1.5	46	.6	451	1.3
≥41	382	1.4	41	.5	423	1.2
Total reporting	27,051	100.0	7,985	100.0	35,036	100.0

TABLE 1—(*Continued*)

Characteristic	Members		Eligible nonmembers		Members and nonmembers	
	N	%	*N*	%	*N*	%
Age						
≤24	54	.2	227	2.9	281	.8
25–29	2,401	9.0	2,432	30.9	4,833	14.0
30–34	4,919	18.4	1,670	21.2	6,589	19.0
35–39	4,111	15.4	925	11.8	5,036	14.6
40–44	4,399	16.4	782	9.9	5,181	15.0
45–49	4,103	15.3	690	8.8	4,793	13.9
50–54	2,885	10.8	448	5.7	3,333	9.6
55–59	1,748	6.5	311	4.0	2,059	6.0
60–64	1,059	4.0	209	2.7	1,268	3.7
65–69	567	2.1	108	1.4	675	2.0
≥70	507	1.9	63	.8	570	1.6
Total reporting	26,753	100.0	7,865	100.0	34,600	100.0
Racial/ethnic identity						
White	19,143	93.6	5,867	93.2	25,010	93.5
Black	287	1.4	109	1.7	396	1.5
Oriental	152	.7	71	1.1	223	.8
American Indian	22	.1	14	.2	36	.1
Middle Eastern	19	.0	8	.1	27	.1
Indian	39	.1	15	.2	54	.2
Hawaiian-Polynesian	23	.1	15	.2	38	.1
Other	758	3.7	199	3.2	957	3.6
Total reporting	20,443	100.0	6,298	100.0	26,741	100.0

[a] *N* is the number of full-time-equivalent psychologists.
[b] The number of years since highest degree awarded.

The 1972 Simulated National Register

The first presentation of the data, in Table 1, depicts the numbers of psychologists having various demographic and other characteristics. The table contains separate columns for members, eligible nonmembers, and the total of the two columns, the latter figure corresponding roughly to the sample from previous National Registers.

Note that the total number of respondents in each column is given at the beginning of the table, as *Total*, the figures 27,371, 7,990, and 35,361 for members, nonmembers, and total, respectively. In the body of this table, as in all tables in this report, the numbers responding to any particular item will not, in general, add to the grand total. Respondents discriminated among items for which they provided information. Percentages are based on the total responses to each particular item. A few of the highlights follow:

- Seventy-eight percent of the membership in the sample holds the doctorate. By level of highest degree, however, the eligible nonmembers are distinctly different from the membership of the Association with respect to the proportion of doctorates. Further light on this discrepancy is shed by years of experience. Over half of the nonmember psychologists have four or fewer years of experience since obtaining the highest degree. Furthermore, other data indicate that about 1,500 of the member-eligibles are still in graduate school, presumably working toward the doctorate. Clearly, the eligible nonmembers are a younger, less experienced group of psychologists consistent with a well-established tendency for young psychologists to take a few years before joining with their cohorts in supporting and identifying with the Association. Overall, however, 69% of the respondents possess the doctorate.
- The membership is primarily male: 76%. Overall the sample is 75% male, the difference being attributed to a higher proportion of females in the eligible nonmember portion of the sample.
- Of those reporting a subfield, the largest number are in clinical: 33%. The cluster of human service subfields—clinical, community, counseling, and school—comprises 56% of the total sample.
- Fifty percent of the respondents in the total sample are employed in institutions of higher education, and an additional 12% are employed in

precollege settings. Although the preponderance of psychologists are identified with human service subfields, employment in traditional human service settings is not the rule. Only 16% of the members sampled are employed in hospitals and clinics, and an additional 7% are employed in independent or group practice. The corresponding figures are 15% and 6% for the total sample.

- The largest single source of support for employment is state governments, comprising 38% of the total sample.
- As has frequently been observed, the employment settings of psychologists are predominantly in the public sector, including nonprofit organizations, with 85% of the total being so employed. The remainder are employed in private-for-profit settings (industry, etc.) or are self-employed.
- As a profession, psychologists spend the largest proportion of their effort in application and practice and the least in research, with sizable portions of teaching and management included.[5] The balance among the four major classes of activities is to be noted. In another way of looking at activities, about 75% of the respondents indicated that at least some time was spent in each of the four major activities categories.
- The median number of years of experience of the member sample is 8.4. For the total sample there is a much more pronounced mode at the 1- and 2-year level, and the median number of years of experience for this group is 7.0. In 1972, almost half of all respondent psychologists had received their highest degree in 1965 or later!
- The distribution of ages throughout the middle range of the membership sample is reasonably uniform, and it can be clearly seen that the eligible nonmembers are a youthful group. As for medians, that of the membership sample reporting is 41.6 years; 40.0 for the total group.
- Ninety-four percent of the membership is white, according to these data, and the same proportion obtains for the total sample. Only 287 members indicated that they were black, a figure which if extrapolated to the total membership would yield about 370 black members as of 1972. This question was left blank by almost a third of the respondents.
- Association members and North American psychologists in general are primarily citizens of the United States, 97% of both the membership and total respondents so indicating.

A Backward Look before Moving On

During the 20 years that have now elapsed since 1954, the year of the first NRSTP, a number of major world events have had significant effects on psychological and other scientific manpower (among other things, to be sure). In 1957, in the midst of the continuing Cold War hysteria, Russia, by launching the first known orbital satellite, Sputnik, demonstrated to the American public a significant technological sophistication and extensive manufacturing capability in the area of rockets and missiles. Not since Pearl Harbor (even including Korea) had the threat of foreign power mobilized the American spirit into such unity of purpose that the goal of putting a man on the moon in 10 years was accepted as a popular mandate. Cost was no object. Federal funds poured into the general scientific enterprise at all levels—from educating the public; to training scientists; to greatly expanding the scientific research capability of the universities and institutes; to research, development, and production activities. Concomitantly, the post World War II "baby boom" exploded upon the colleges and universities, and the ensuing demand for a college education led to a major expansion in university physical plant and faculty. The enrollments in colleges almost doubled from 3,039,000 in 1960 to 5,956,000 in 1970 (U.S. Dept. of Commerce, 1973, p. 131).

Ten years ago Allan Cartter (1965) predicted that such rapid growth was a sometime thing, that a leveling off of the growth of the college-age cohort would inevitably curb higher education expansion and stabilize university and faculty size. Such a prognostication was borne out by 1970 in most disciplines, the exceptions being the social sciences, medicine, and law. The slowdown was exacerbated by an economic recession affecting the aerospace

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[5] The NSF National Register questionnaire asked respondents to specify a single primary work activity. Our knowledge that psychologists engage in many activities prompted us to revise the APA work-activity question to provide estimates of time percentages spent by our respondents in several work activities. In analyzing these data we have multiplied proportion of time spent in a specified activity by the number of psychologists reporting that proportion, summing these products for each activity. Our analysis provides estimates of the manpower effort in each activity stated in terms of an equivalent number of individuals engaged 100% of their time in that activity. Remarkably, our equivalent-effort figures are virtually identical with recent National Register proportions.

and related industries and notably the fields of engineering and the physical sciences when the achievement of reaching the moon had been put into perspective.

Other events of the late sixties—the disenchantment with the Vietnam war and a growing concern for mankind's predicament—seem to have led students out of the sciences and humanities and into fields seen as "socially relevant."

Psychology's growth partook deeply of this whole vast development. Funds were readily available for psychological training, for research, and for physical plant expansion in the universities. Training capabilities were vastly increased. Only 665 doctorates in psychology were awarded in 1954 (NAS–NRC, 1963). In 1974 this figure will be about 2,600 (USDHEW, 1973, p. 58). In 1954, graduate enrollment in psychology was about 3,600 (Moore, 1955); in 1972, the figure was 38,000 (Cuca, 1974). Undergraduate baccalaureates in psychology have soared to 100,000 per year. Because of this growth, the educational system has placed great demands on the supply; half of all psychologists, as we have seen above, were employed in institutions of higher education according to our 1972 survey.

Between 1954 and 1974, the growth of the Association has soared from 12,380 to 37,371 members. The number of psychologists responding to the 1954 National Register questionnaire was 10,163. Our total 1972 questionnaire return numbered 35,361. One can be struck by other impacts of the events of the 18 years between 1954 and 1972. In Table 2 we have abstracted by six-year intervals a sample of interesting trends that give some of the fine-grain detail of psychology's growth. These data are taken from NSF publications[6] for the years 1954, 1960, and 1966. The APA data are used for 1972.

The figures showing growth are among the most striking, of course, including the growth in median salary,[7] but signs of stability are noteworthy in their own way. In comparison with other sciences, psychology's growth is typical, not unusual. Psychologists comprised 8.8% of the scientific manpower pool in 1954 and 8.4% in 1970, the last year for which NSF comparison figures are available. Psychologists' median salary was 90.4% that of the total in 1954; 100% (or equality) in 1970. In terms of doctorates only, psychology's share decreased from 12.4% (NSF, 1959) to 10.9% (NAS, 1974) of the total pool in science fields in 1973.

TABLE 2

Some Interesting Trends in the Growth of Psychology since 1954

	1954	1960	1966	1972
No. psychologists in APA	12,380	18,215	24,473	33,629
No. psychologists reporting	10,163	15,257	19,027	37,361
Proportion of psychologists employed in:				
Clinical psychology	.34	.39	.36	.36
Human service subfields	.51	.56	.53	.56
Educational institutions	.51	.46	.51	.62
Industry-business/ self-employed	.11	.16	.14	.15
Proportion of psychologists with doctorates	.56	.59	.66	.69
Proportion of male psychologists	.78	.78	.78	.76
Median salary of male psychologists				
Total	5,900	8,000	11,500	19,500
Doctorate	6,600	9,000	12,100	20,000
Masters	5,000	7,000	9,800	17,000
% of total doctoral scientists	12.4	14.5	13.9	10.9
% of total scientific manpower pool	8.8	7.6	7.8	NA

Note. Data for 1954, 1960, and 1966 are taken from National Science Foundation publications. APA data are used for 1972. NA = not available.

A few additional comments about the data displayed in Table 2 are worth noting. Undoubtedly, the influence of government training subventions underlies the gradual increase in the percentage of doctorates from 56% in 1954 to 69% in 1972. Perhaps the early Veterans Administration and National Institute of Mental Health programs funding clinical psychology training account for the rise between 1954 and 1960 in the percentages of individuals in clinical (from 34% to 39%) and in the human service subfields (from 51% to 56%). After 1960, the availability of other government training funds must have attenuated that relative growth and expanded all fields of psychology relatively uniformly.

The effect of the rapid expansion of the schools and colleges is reflected in the changes of psycholo-

[6] Data from the periodic canvases of scientific fields of the National Register are presented in extensive form in NSF biyearly publications entitled *American Science Manpower* for the years 1954 through 1970.

[7] In terms of constant dollars, the 1954 median of $6,600 is equivalent to $12,000 in 1972 dollars as determined using the implicit price deflators of the Bureau of Economic Analysis (U.S. Department of Commerce, 1973).

TABLE 3

Number of APA Members by Various Characteristics, Level of Highest Earned Degree, and Sex

Characteristic	Doctorates						Master's					
	Men		Women		Total		Men		Women		Total	
	N	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%
Total *N* responding	17,081		4,129		21,210		3,497		2,155		5,652	
Subfield												
General	207	1.4	45	1.3	252	1.4	85	2.8	44	2.4	129	2.7
Systems, methodology, and technique	247	1.6	45	1.3	292	1.6	20	.7	14	.8	34	.7
Experimental	1,467	9.7	219	6.3	1,686	9.1	91	3.0	36	2.0	127	2.6
Physiological	402	2.7	50	1.4	452	2.4	16	.5	3	.2	19	.4
Comparative/animal	103	.7	9	.3	112	.6	5	.2	2	.1	7	.1
Developmental	529	3.5	354	10.1	883	4.8	30	1.0	76	4.2	106	2.2
Social	975	6.5	202	5.8	1,177	6.3	75	2.5	35	1.9	110	2.3
Personality	282	1.9	60	1.7	342	1.8	29	1.0	35	1.9	64	1.3
Psychometrics	162	1.1	41	1.2	203	1.1	36	1.2	40	2.2	76	1.6
Educational	1,094	7.3	327	9.4	1,421	7.7	129	4.3	86	4.7	215	4.5
Engineering	162	1.1	5	.1	167	.9	76	2.5	7	.4	83	1.7
Industrial and organizational	1,090	7.2	36	1.0	1,126	6.1	370	12.4	45	2.5	415	8.6
Clinical	5,374	35.7	1,405	40.2	6,779	36.5	936	31.3	599	32.9	1,535	32.0
Community	306	2.0	43	1.2	349	1.9	57	1.9	23	1.3	80	1.7
Counseling	1,746	11.6	334	9.6	2,080	11.2	438	14.7	192	10.6	630	13.1
School	558	3.7	239	6.8	797	4.3	531	17.8	562	30.9	1,093	22.8
Other	358	2.4	82	2.3	440	2.4	62	2.1	19	1.0	81	1.7
Total reporting	15,062	100.0	3,496	100.0	18,558	100.0	2,986	100.0	1,818	100.0	4,804	100.0
Employment setting												
Hospitals	1,360	8.5	309	8.6	1,669	8.5	262	8.3	155	8.9	417	8.5
Clinics	904	5.7	318	8.8	1,222	6.2	301	9.5	186	10.7	487	9.9
Independent/group practice	1,083	6.8	339	9.4	1,422	7.3	148	4.7	100	5.8	248	5.1
Government agency	680	4.3	129	3.6	809	4.1	322	10.1	108	6.2	430	8.8
Private or quasi-government agency	107	.7	24	.7	131	.7	66	2.1	34	2.0	100	2.0
Research establishment	466	2.9	76	2.1	542	2.8	86	2.7	24	1.4	110	2.2
Consulting firm	418	2.6	30	.8	448	2.3	125	3.9	27	1.6	152	3.1
Business or industry	443	2.8	28	.8	471	2.4	268	8.4	29	1.7	297	6.0
Association or society	56	.4	16	.4	72	.4	23	.7	8	.5	31	.6
Religious institution	44	.3	18	.5	62	.3	18	.6	11	.6	29	.6
Military service	92	.6	3	.1	95	.5	42	1.3	1	0.0	43	.9
Law enforcement agency	9	0.0	2	.1	11	0.0	4	.1	3	.2	7	.1
Judicial system	16	.1	4	.1	20	.1	7	.2	7	.4	14	.3
Correctional system	94	.6	12	.3	106	.5	77	2.4	15	.9	92	1.9
Other noneducational	61	.4	24	.7	85	.4	24	.8	19	1.1	43	.9
Subtotal noneducational	5,833	36.5	1,332	36.9	7,165	36.5	1,773	55.9	727	41.9	2,500	50.9
Universities, excluding medical schools	6,786	42.4	1,280	35.4	8,066	41.0	302	9.5	152	8.8	454	9.2
Medical schools	647	4.0	182	5.0	829	4.2	39	1.2	32	1.8	71	1.4
Four-year colleges	1,683	10.5	393	10.9	2,076	10.6	177	5.6	89	5.1	266	5.4
Two-year colleges	274	1.7	96	2.7	370	1.9	205	6.5	75	4.3	280	5.7
Regional school district	277	1.7	124	3.4	401	2.0	285	9.0	261	15.0	546	11.1
Secondary school	153	1.0	46	1.3	199	1.0	149	4.7	106	6.1	255	5.2
Elementary schools	133	.8	74	2.0	207	1.1	166	5.2	235	13.5	401	8.2
Other educational	210	1.3	87	2.4	297	1.5	77	2.4	59	3.4	136	2.8
Subtotal educational	10,163	63.5	2,282	63.1	12,445	63.5	1,400	44.1	1,009	58.1	2,409	49.1
Total reporting	15,996	100.0	3,614	100.0	19,610	100.0	3,173	100.0	1,736	100.0	4,909	100.0

TABLE 3—(*Continued*)

Characteristic	Doctorates						Master's					
	Men		Women		Total		Men		Women		Total	
	N	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%
Departmental setting in education												
Psychology	5,125	53.6	990	47.1	6,115	52.4	513	40.1	346	36.5	859	38.6
Education	1,488	15.6	399	19.0	1,887	16.2	158	12.4	119	12.6	277	12.4
Sociology	81	.8	9	.4	90	.8	9	.7	6	.6	15	.7
Social work	38	.4	12	.6	50	.4	1	.1	5	.5	6	.3
Behavioral sciences	302	3.2	59	2.8	361	3.1	58	4.5	17	1.8	75	3.4
Medicine	647	6.8	182	8.7	829	7.1	39	3.0	32	3.4	71	3.2
Business	251	2.6	7	.3	258	2.2	12	.9	3	.3	15	.7
Administrative staff	563	5.9	128	6.1	691	5.9	151	11.8	86	9.1	237	10.6
Student counseling center	858	9.0	239	11.4	1,097	9.4	302	23.6	293	30.9	595	26.7
Other	206	2.2	76	3.6	282	2.4	36	2.8	40	4.2	76	3.4
Total reporting	9,559	100.0	2,101	100.0	11,660	100.0	1,279	100.0	947	100.0	2,226	100.0
Type of employer												
Federal government	1,345	8.4	238	6.6	1,583	8.1	273	8.7	74	4.2	347	7.1
State government	6,594	41.4	1,277	35.5	7,871	40.3	885	28.1	432	24.5	1,317	26.8
Local government	1,252	7.9	508	14.1	1,760	9.0	766	24.4	623	35.4	1,389	28.3
Private, for profit	1,605	10.1	284	7.9	1,889	9.7	504	16.0	132	7.5	636	13.0
Independent, nonprofit	3,453	21.7	805	22.4	4,258	21.8	482	15.3	315	17.9	797	16.2
Self-employed	985	6.2	328	9.1	1,313	6.7	139	4.4	114	6.5	253	5.2
Religious supported	702	4.4	158	4.4	860	4.4	96	3.0	72	4.1	168	3.4
Total reporting	15,936	100.0	3,598	100.0	19,534	100.0	3,145	100.0	1,762	100.0	4,907	100.0
Work activity[a]												
Research	2,799	20.0	545	17.4	3,344	19.5	317	11.5	114	7.5	431	10.1
Application/practice	4,757	33.9	1,239	39.6	5,996	34.9	1,434	52.2	987	65.0	2,421	56.8
Teaching	3,759	26.8	887	28.3	4,646	27.1	359	13.1	196	12.9	555	13.0
Management/administration	2,712	19.3	461	14.7	3,173	18.5	635	23.1	221	14.6	856	20.1
Years of experience[b]												
<1	654	3.8	205	5.0	859	4.1	69	2.0	32	1.5	101	1.8
1–2	2,476	14.6	708	17.3	3,184	15.1	400	11.6	194	9.1	594	10.6
3–4	2,328	13.7	635	15.5	2,963	14.1	469	13.6	238	11.1	707	12.7
5–6	1,789	10.5	475	11.6	2,264	10.7	362	10.5	222	10.4	584	10.5
7–8	1,491	8.8	346	8.5	1,837	8.7	259	7.5	157	7.4	416	7.4
9–10	1,243	7.3	304	7.4	1,547	7.3	228	6.6	139	6.5	367	6.6
11–12	1,024	6.0	237	5.8	1,261	6.0	206	6.0	131	6.1	337	6.0
13–14	1,019	6.0	198	4.8	1,217	5.6	202	5.9	100	4.7	302	5.4
15–16	967	5.7	163	4.0	1,130	5.4	182	5.3	112	5.2	294	5.3
17–18	995	5.9	153	3.7	1,148	5.4	166	4.8	69	3.2	235	4.2
19–20	939	5.5	142	3.5	1,081	5.1	232	6.7	99	4.6	331	5.9
21–25	1,113	6.5	191	4.7	1,304	6.2	470	13.6	276	12.9	746	13.4
26–30	235	1.4	93	2.3	328	1.6	51	1.5	149	7.0	200	3.6
31–35	298	1.8	83	2.0	381	1.8	96	2.8	102	4.8	198	3.5
36–40	224	1.3	67	1.6	291	1.4	39	1.1	59	2.8	98	1.7
≥41	208	1.2	85	2.1	293	1.4	18	.5	57	2.7	75	1.3
Total reporting	17,003	100.0	4,085	100.0	21,088	100.0	3,449	100.0	2,136	100.0	5,585	100.0
Age												
≤24	6	0.0	1	0.0	7	0.0	30	.9	16	.8	46	.8
25–29	1,298	7.7	288	7.3	1,586	7.6	527	15.3	230	11.2	757	13.7
30–34	3,333	19.8	640	16.3	3,973	19.1	638	18.5	243	11.8	881	16.0
35–39	2,808	16.6	572	14.6	3,380	16.3	481	13.9	192	9.3	673	12.2
40–44	2,978	17.6	592	15.1	3,570	17.2	500	14.5	278	13.5	778	14.1
45–49	2,570	15.2	596	15.2	3,166	15.2	520	15.1	356	17.3	876	15.9
50–54	1,708	10.1	471	12.0	2,179	10.5	373	10.8	282	13.7	655	11.9

TABLE 3—(*Continued*)

Characteristic	Doctorates						Master's					
	Men		Women		Total		Men		Women		Total	
	N	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%
Age (*Continued*)												
55–59	1,018	6.0	301	7.7	1,319	6.3	197	5.7	198	9.6	395	7.2
60–64	587	3.5	215	5.5	802	3.9	111	3.2	125	6.1	236	4.3
65–69	301	1.8	121	3.1	422	2.0	46	1.3	80	3.9	126	2.3
≥70	266	1.6	127	3.2	393	1.9	27	.8	61	3.0	88	1.6
Total reporting	16,873	100.0	3,924	100.0	20,797	100.0	3,450	100.0	2,061	100.0	5,511	100.0
Racial/ethnic identity												
White	11,925	93.7	2,939	93.4	14,864	93.6	2,456	93.9	1,527	94.0	3,983	93.9
Black	130	1.0	59	1.9	189	1.2	57	2.2	37	2.3	94	2.2
Oriental	98	.8	30	1.0	128	.8	19	.7	4	.2	23	.5
American Indian	13	.1	3	.1	16	.1	4	.2	2	.1	6	.1
Middle Eastern	14	.1	1	0.0	15	.1	1	0.0	3	.2	4	.1
Indian	25	.2	2	0.0	27	.2	8	.3	2	.1	10	.2
Hawaiian-Polynesian	14	.1	4	.1	18	.1	5	.2	0	0.0	5	.1
Other	520	4.1	108	3.4	628	4.0	66	2.5	49	3.0	115	2.7
Total reporting	12,739	100.0	3,146	100.0	15,885	100.0	2,616	100.0	1,624	100.0	4,240	100.0

[a] *N* is the number of full-time-equivalent psychologists.
[b] The number of years since highest degree awarded.

gists employed in educational institutions. From 1954 to 1960, this figure actually decreased from 51% to 46%, possibly because of the relative increase of service-oriented clinical psychologists. By 1972, however, the percentage of psychologists employed in educational institutions had increased to 62%, of whom the bulk, 50% of all psychologists, were employed in institutions of higher education.

One further aspect of the data in Table 2 merits comment. The relative increase of those employed in private industry or self-employed between 1954 and 1960 is probably due to two factors: (*a*) increase in the relative percentage of clinical psychologists and (*b*) the potential for private practice brought about by statutory recognition of psychologists for such practice, a significant development of that time for psychology.

A Closer Look at APA Psychologists

Having spent some time reviewing results from the APA Manpower Data System that provide a sequel to the NSF National Register, let us now focus on APA membership. As we have mentioned, it is likely that future detailed statistical information will be available only on Association members. Thus, a baseline of current member characteristics is needed against which future state-of-the-field comparisons can be made.

Level of education must be an integral part of any membership analysis because membership categories are defined by academic degree. Gender is an additional factor of importance for the analysis due to current concerns over the extent to which women are represented in the psychological community and because of their relative representation within the fine structure we have been observing. In order to provide data illuminating these several aspects of the membership, we have chosen to present a breakdown of the membership data by degree level and by sex but otherwise in essentially the same format as was employed earlier in considering the total responding sample.

Table 3 contains membership characteristics data for doctorate and master's-level individuals and for men and women separately in each of these educational levels. Because of the small number of bachelor members, we have not considered them in these fine-focus analyses. The numbers reported in each column are the numbers of members of the particular subgroup responding for the specific characteristic.[8] The percentages for each characteristic are based upon the number responding.

[8] The total membership information is given by characteristic in Table 1. In most instances, the subtotals in Table 3 will not sum to the corresponding totals in Table 1 because some respondents chose not to provide gender information.

With these considerations in mind, then, we can proceed to examine in more detail the APA membership characteristics. We shall attempt no exhaustive analysis of the data. They are presented for the information of the reader whose own interests will provide more specific questions which hopefully the data illuminate.

There are, however, some interesting differences to point up among categories of members. For example, it seems clear that a much higher percentage of master's-level members are in the applied subfields—engineering through school psychology—than are doctoral members (90% versus 61%). Moreover, it seems that doctorate men resemble doctorate women in their choice of subfields with the exception of developmental and industrial/organizational where major differences exist. Similarly, the men and women masters are alike except for a relative preponderance of master's men in industrial/organizational psychology and master's women in school psychology.

Looking at employment setting, one must again be impressed with the percentage of members who are employed in educational settings. The moderator, educational level, however, has a strong influence as doctoral men and women are employed predominantly in colleges, whereas the master's-level men and women, and particularly the women, are employed in precollege settings.

Employment in a school setting is also reflected in the employment auspices, with local government being the primary employer of master's degree holders, while for the doctoral individuals the state government and independent nonprofit institutions, both major university support categories, are significant employers.

The large influx of younger psychologists into the total manpower pool and into the ranks of the members of the Association is clearly seen in the virtually monotonic increments of all categories of the years of experience section. In 1972, 60% of the doctorates and 50% of the masters in the membership had received their highest degree within the last 10 years. Moreover, it would appear that the proportion of younger women doctorates is increasing faster than that of the men, with some 65% of the women doctorates having 10 or fewer years since receiving their highest degree.

In terms of age, psychology seems a young profession, with 43% of both doctorates and masters being less than 40 years old in 1972.

Finally, with respect to racial/ethnic identity, there emerges a picture similar to that seen earlier for our total group of psychologists. The percentage of black members with the master's degree is slightly higher than the percentage with the doctorate, although the percentages at both educational levels are still, unfortunately, trivial relative to the total psychology manpower.

Employment of Psychologists

As indicated in the introduction to this article, the employment status of professionals, particularly scientists, engineers, and academics, is a matter of continuing policy concern. As further noted, the continued firmness of the market for psychologists has been encouraging. One is tempted to add that the demand seems not linked straightforwardly to salary levels. Psychologists have not yet priced themselves out of the market. They may not even have gotten their fair share! Following up on such speculation is not the subject of this article, however.

What emerges from the manpower data is an encouraging picture for psychology in 1972. The pertinent data are presented in Table 4. Of a total of 33,575 member and nonmember respondents, 2,811 or 8.3% reported that they were not employed at the time of filling out the questionnaire. The bulk of these were students (1,686); and only 682 or 2% of the total were actually seeking employment. Since many of those seeking were undoubtedly students, the ranks of the unemployed in psychology were very small. On the other hand, 11.3% of those already employed were seeking other employment. Much of this can be seen as the inevitable consequence of a university up-or-out system, but some may represent mismatching, perceived underemployment, or simply dislike for one's present employment. The operant level for that variable is unknown!

Also indicating demand is the substantial number of psychologists who are engaged in employment other than a full-time job. Of the total, for example, 28% have a part-time position in addition to full-time employment. Another 8% piece together a career out of one or more part-time positions. As reported elsewhere (Boneau, 1974), it is preponderantly men who have a part-time job in addition to full-time employment, while it is women who are predominantly engaged in part-time-only careers. One is tempted to conjecture that the

TABLE 4

Employment Status of Psychologists

Employment status	Members				Total	Eligible nonmembers	Members and nonmembers
	Doctorates		Master's				
	Men	Women	Men	Women			
Presently employed	15,830	3,591	3,148	1,761	24,692	6,072	30,764
Full-time	9,598	2,058	1,970	1,085	14,931	4,168	19,099
Full and part-time	5,331	744	920	252	7,330	1,145	8,475
One or more part-time	658	688	203	367	1,963	562	2,525
Retired	115	73	22	47	262	57	319
Other	128	28	33	10	206	140	346
Seeking other employment	1,328	502	422	226	2,527	943	3,470
Not presently employed	312	262	157	231	1,002	1,809	2,811
Student/trainee	25	16	96	49	194	1,492	1,686
Retired	144	73	16	39	284	105	389
Other	143	173	45	143	524	212	736
Seeking employment	79	75	49	69	280	402	682

level of remuneration of psychology's professional salaries may not be sufficient to support aspirations, hence the moonlighting. The data are not available to test such a hypothesis, nor is there information from other fields.

The Salaries of Psychologists

About 21,000 member respondents (77% of the respondents) provided the salary information solicited on the questionnaire.[9]

In Table 5 we have chosen to present the median, the first and third quartiles, and the first and ninth deciles in those subcategories in which 20 or more respondents provided salary information. For categories having between 10 and 20 respondents, we have given the median only, and for subcategories containing fewer than 5 respondents we have presented no data. The *N*s upon which distributions are based are not presented.

A few further comments are called for. Controlling for years of experience (data not presented here) makes inconsequential the difference between member and nonmember data. It may be assumed that member data are representative of psychologists in general, at least for the purpose of presenting salary information.

The data of Table 5 are from those individuals who indicated full-time employment (see Table 4) and are based only upon the financial remuneration from that full-time employment excluding fringe benefits. Academic salaries on a nine-month base were normalized by adding two ninths of salary prior to obtaining the distribution. Those self-employed full time are also included. Presumably, their "salary" represents net of income less business expenses.

The data of Table 5 are presented essentially without comment. We cannot avoid, however, remarking on the general similarity between the salaries of doctorate women and those of master's men, not doctorate men. A look at this comparison a few years hence should prove of interest.

An additional comment is prompted by the stated rationale of the federal government to phase out funds for graduate training on the grounds that it is inequitable to provide free training for those who will have a high income potential because of that training (Edwards, 1973). One might argue that with average hourly wages for journeymen electricians and plumbers now in the $8–$11 per hour range depending upon location (Bureau of National Affairs, 1974), figures which put them around the median for our doctoral salaries, it seems difficult to understand that psychologists have

[9] Since salary depends significantly upon the degree level and the sex of the respondent (as well as upon other factors, of course), we have provided salary information for members in essentially the same format as Table 3.

TABLE 5

Annual Salaries of APA Members by Various Characteristics, Level of Highest Earned Degree, and Sex

Characteristic	Doctorates										Master's									
	Men					Women					Men					Women				
	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9
Total *N* responding	14.6	16.6	20.0	25.0	31.0	13.0	15.0	17.6	21.2	25.1	11.4	14.0	17.0	21.2	25.8	13.2	12.4	15.3	19.0	21.4
Subfield																				
General	13.8	16.0	19.7	23.9	27.7	14.6	15.0	17.3	19.2	25.4	9.6	11.9	13.8	17.8	21.7	9.3	10.4	12.7	16.1	20.1
Systems and methodology	15.3	17.3	21.0	25.0	29.5	13.8	15.2	18.1	21.5	25.6	14.2	16.0	19.0	21.5	26.3	—	—	15.7	—	—
Experimental	14.2	16.0	19.0	24.0	29.7	12.5	14.6	16.6	19.8	22.4	11.0	12.4	15.5	18.7	26.0	9.6	10.4	12.6	15.6	17.4
Physiological	13.8	16.1	19.3	24.6	31.0	10.0	13.4	17.1	20.5	22.9	13.7	14.4	15.0	18.7	20.4	—	—	—	—	—
Comparative/animal	14.7	16.5	19.2	22.9	29.0	—	—	—	—	—	12.9	13.2	13.8	14.4	14.8	—	—	—	—	—
Developmental	14.0	16.0	18.9	24.5	29.8	12.8	14.9	18.0	21.3	26.5	9.4	11.2	17.9	22.3	27.4	10.8	12.7	13.8	16.0	20.1
Social	14.4	16.0	19.3	24.7	30.8	13.2	14.6	16.8	20.5	24.8	12.0	13.2	16.0	18.8	23.1	11.8	12.9	16.8	18.8	20.0
Personality	14.3	17.2	21.3	26.6	32.9	13.4	15.5	16.8	20.3	23.7	10.9	13.4	15.5	19.0	22.1	11.9	12.5	14.0	15.7	17.3
Psychometrics	15.8	18.0	21.6	26.3	31.8	14.2	16.0	18.5	23.0	26.9	12.3	13.5	18.0	21.0	25.1	7.0	9.0	10.4	12.9	16.3
Educational	15.2	17.3	20.5	25.0	28.8	13.3	15.0	17.5	20.0	24.5	12.0	14.0	16.8	20.0	24.9	10.0	12.0	14.6	18.0	20.8
Engineering	17.2	20.0	23.7	27.5	32.6	—	—	—	—	—	16.9	19.0	20.6	23.7	26.4	—	—	—	—	—
Industrial and organizational	16.0	19.0	23.7	30.0	38.0	15.9	17.3	19.6	24.1	28.2	13.6	16.9	20.0	25.0	31.4	11.6	14.4	16.8	20.9	26.4
Clinical	14.6	16.6	20.0	25.0	33.3	12.9	15.1	17.5	21.0	25.3	10.6	12.4	15.0	18.0	23.1	9.9	11.5	13.4	16.4	20.3
Community	14.8	16.8	20.4	25.3	30.0	14.4	15.0	17.7	19.6	21.5	12.0	14.1	16.2	18.2	23.2	11.6	12.0	13.8	15.3	23.0
Counseling	14.0	15.8	18.5	22.0	26.3	13.0	14.3	16.5	20.0	23.0	10.5	12.5	16.0	19.0	23.2	9.4	10.8	13.4	16.0	17.8
School	14.7	17.3	20.0	23.9	27.0	14.0	17.0	20.5	23.0	25.6	13.4	15.5	18.6	22.2	25.3	13.0	15.3	18.0	21.3	23.9
Other psychology	15.1	17.0	18.0	22.9	33.2	12.9	14.8	16.9	23.5	24.0	12.5	14.5	16.9	20.9	23.0	—	—	17.3	—	—
Employment setting																				
Hospitals	14.3	16.2	19.0	22.0	24.9	13.3	15.0	17.3	20.3	22.0	10.4	12.0	14.6	17.0	20.0	9.7	10.8	12.1	14.2	16.5
Clinics	14.0	16.0	18.0	21.0	25.0	12.0	14.4	16.0	18.0	21.2	10.0	12.0	13.8	16.4	19.5	9.5	11.4	12.7	15.0	18.0
Independent/group practice	18.0	24.0	30.0	40.0	50.0	12.0	17.0	22.0	29.6	36.2	13.2	16.7	24.0	30.0	38.9	12.0	15.0	18.0	24.0	30.0
Government agency	15.8	18.5	22.7	27.8	30.7	13.5	15.0	18.3	24.1	27.3	11.2	14.5	17.9	22.0	26.5	10.4	12.2	14.6	17.5	22.4
Private or quasi-government agency	15.0	16.8	20.0	23.0	28.6	15.1	15.6	18.2	21.9	26.8	11.9	12.8	16.5	18.8	22.5	9.4	11.9	13.2	16.0	19.4
Research establishment	15.5	18.0	22.0	27.0	34.0	12.0	15.0	18.0	21.5	26.0	13.0	15.7	18.0	22.5	29.4	12.6	12.7	14.7	16.8	19.2
Consulting firm	17.0	20.0	26.8	35.0	50.0	12.4	15.1	17.0	22.0	25.0	14.5	18.0	22.0	28.0	37.6	9.2	10.0	12.2	17.2	25.0
Business or industry	18.0	20.6	25.0	33.0	41.0	14.8	17.4	20.0	23.0	27.4	14.3	17.7	20.6	25.5	35.0	11.6	14.5	17.0	19.0	21.5
Association or society	18.2	22.4	24.6	30.0	35.3	14.0	14.8	19.0	22.8	24.0	14.1	15.8	21.5	25.0	31.0	—	—	—	—	—
Religious institution	4.8	12.8	16.0	18.9	20.3	1.7	2.4	6.0	11.0	11.6	6.4	10.4	14.2	17.4	22.9	—	—	—	—	—
Military service	11.9	13.0	15.0	20.0	25.8	—	—	—	—	—	12.0	13.1	16.4	20.1	22.2	—	—	—	—	—
Law enforcement agency	—	—	—	—	—	—	—	—	—	—	14.6	14.8	15.2	17.6	19.0	—	—	—	—	—
Judicial system	—	—	17.5	—	—	—	—	—	—	—	12.2	13.4	13.8	15.5	19.5	—	—	—	—	—
Correctional system	14.0	15.8	18.2	21.0	23.9	—	—	15.0	—	—	11.6	13.6	15.4	17.8	21.0	—	—	13.0	—	—
Other noneducational	14.6	16.0	19.5	25.0	31.5	11.9	12.7	16.1	19.5	27.0	11.3	13.1	15.8	16.8	23.2	—	—	13.6	—	—

TABLE 5—(*Continued*)

Characteristic	Doctorates										Master's									
	Men					Women					Men					Women				
	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9
Universities, excluding medical schools	14.8	16.6	20.0	24.7	29.9	13.3	15.0	17.3	20.6	25.2	10.0	12.6	14.6	18.0	22.3	9.1	10.8	13.3	16.0	19.1
Medical schools	13.5	16.7	20.0	25.0	30.0	13.1	15.3	17.1	20.0	23.0	10.1	11.1	14.0	16.0	21.4	11.0	12.6	14.0	15.1	17.6
Four-year colleges	13.9	15.4	18.0	21.9	25.5	13.0	14.6	16.8	20.0	23.3	10.7	12.8	15.0	17.3	20.6	9.8	12.0	14.0	15.6	18.1
Two-year colleges	14.0	16.2	20.0	23.9	26.6	13.0	15.4	17.9	21.3	24.0	11.4	14.0	17.1	20.8	23.9	10.5	13.2	15.8	18.6	21.3
Regional school districts	15.5	17.4	20.7	23.4	27.0	14.0	15.9	20.0	22.9	24.7	13.3	15.7	18.5	21.7	24.6	12.8	15.0	17.8	21.3	23.1
Secondary schools	15.0	17.8	22.0	25.2	27.6	15.0	18.9	21.2	23.9	26.6	14.8	16.1	18.6	22.4	25.2	11.3	14.5	17.9	21.5	23.2
Elementary schools	15.0	17.2	20.4	23.9	26.8	15.7	17.8	20.6	23.9	30.6	13.4	15.9	19.0	21.9	24.6	12.8	15.5	18.2	21.3	23.9
Other educational	14.7	17.1	19.5	25.0	33.3	13.9	15.5	18.0	22.4	30.2	11.9	14.0	15.8	18.8	26.1	11.4	13.7	15.6	17.2	18.6
Departmental setting in education																				
Psychology	14.4	16.0	19.1	23.9	29.3	13.0	15.0	17.3	20.6	25.3	11.5	14.0	16.6	20.2	24.0	10.6	13.1	15.6	20.0	22.6
Education	15.4	17.0	20.0	24.0	28.2	14.0	16.0	18.6	22.0	25.7	12.8	15.0	18.0	21.1	23.9	10.6	14.2	17.0	20.0	22.8
Sociology	15.8	17.7	21.0	25.7	33.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Social work	17.6	19.0	20.7	25.3	27.3	—	—	16.8	—	—	—	—	—	—	—	—	—	—	—	—
Behavioral sciences	14.9	16.5	20.7	26.0	30.1	13.9	15.8	17.3	20.0	21.7	12.3	14.6	17.2	18.9	22.6	9.0	11.6	16.0	18.6	23.4
Medicine	13.6	16.8	20.0	25.0	30.0	12.8	15.0	17.0	19.9	22.6	10.3	11.6	14.2	17.4	26.5	11.0	11.9	14.9	15.1	17.7
Business	16.6	18.8	21.3	25.6	31.8	—	—	—	—	—	13.8	14.7	16.5	20.0	30.5	—	—	—	—	—
Administrative staff	16.2	18.9	22.2	26.7	31.2	14.2	17.0	20.0	23.0	25.6	14.1	16.6	19.5	22.0	25.2	12.3	14.5	17.3	21.3	23.8
Student counseling center	13.8	15.2	17.5	20.9	25.0	13.0	14.0	16.4	20.2	23.9	11.2	13.7	16.4	20.0	23.7	11.5	13.6	17.3	20.3	22.5
Type of employer																				
Federal government	14.5	18.0	21.7	24.9	22.5	14.0	15.9	19.4	22.4	21.0	12.8	16.5	20.0	23.0	20.6	10.6	13.0	16.2	19.8	17.0
State government	14.7	16.3	19.0	23.3	27.9	13.1	15.0	17.0	20.0	23.2	10.5	12.2	15.0	17.4	21.1	10.0	11.5	14.0	16.1	19.3
Local government	15.0	17.3	20.0	24.6	29.3	14.6	16.8	19.8	23.0	27.7	12.4	15.0	18.0	21.5	24.5	12.2	14.8	17.8	21.3	23.2
Private, for profit	15.6	19.3	25.0	34.5	45.0	12.6	15.0	18.0	22.7	27.2	13.8	17.0	20.2	25.5	35.0	9.2	10.6	13.7	17.0	20.0
Independent, nonprofit	14.5	16.5	20.0	24.6	30.0	12.7	14.6	17.2	20.0	24.0	10.7	12.5	15.5	18.5	23.0	9.5	11.0	13.1	16.0	19.3
Self-employed	17.9	24.0	30.0	40.0	53.2	11.7	16.2	22.5	30.0	40.0	15.0	19.9	25.0	32.0	43.2	12.0	15.0	20.0	25.0	36.6
Religious supported	12.6	14.6	16.8	20.0	24.2	11.7	13.3	15.1	17.5	20.5	9.9	10.6	13.6	16.3	21.2	4.4	9.5	12.5	14.0	15.6
Years of experience																				
<1	12.0	13.9	15.3	17.3	21.3	11.9	13.3	14.9	16.5	19.9	9.8	10.4	12.0	15.1	19.6	8.6	10.0	12.0	14.6	80.7
1–2	12.9	14.5	16.0	18.0	20.6	12.0	13.6	15.3	17.3	20.0	9.4	10.9	12.5	14.8	18.1	9.0	10.8	12.6	15.3	17.9
3–4	14.0	15.6	17.3	19.9	22.3	12.8	14.6	16.5	18.9	21.5	10.0	11.6	13.6	16.0	18.6	9.4	10.6	12.5	15.3	18.5
5–6	15.3	16.8	18.6	21.0	25.0	13.0	15.0	17.3	20.0	22.9	11.3	13.2	15.9	18.1	20.8	10.3	11.7	13.6	17.0	20.0
7–8	16.0	17.9	20.0	23.5	27.8	14.6	16.0	18.5	22.0	26.4	12.7	14.2	16.3	19.1	22.7	10.5	12.8	15.0	18.2	21.3
9–10	16.6	18.9	21.3	24.4	29.0	15.0	16.8	19.3	[illegible]	25.0	12.6	15.3	17.8	20.6	24.6	11.0	13.3	16.0	18.0	21.3
11–12	17.3	19.8	22.3	26.4	31.9	15.7	17.5	20.1	23.0	26.3	14.0	15.5	17.7	20.7	25.9	12.0	14.0	16.0	19.0	22.7

TABLE 5—(*Continued*)

Characteristic	Doctorates										Master's									
	Men					Women					Men					Women				
	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9	D_1	Q_1	*Mdn*	Q_3	D_9
Years of experience																				
13–14	18.0	20.2	23.8	28.0	35.0	15.7	18.0	20.0	22.7	26.6	14.0	16.6	19.0	23.3	27.3	11.1	13.9	16.6	21.2	23.0
15–16	18.0	20.5	24.2	29.4	35.0	15.3	18.0	21.3	25.3	31.6	14.0	16.5	20.0	22.6	26.2	12.0	14.3	18.0	21.3	23.8
17–18	19.0	21.9	25.5	30.0	36.0	16.0	18.5	21.2	25.4	29.5	14.5	16.8	21.0	25.0	28.8	11.3	13.9	15.8	20.1	22.6
19–20	18.6	21.9	25.0	30.0	36.0	14.9	17.3	21.4	24.0	26.5	14.5	16.9	20.0	24.0	28.2	12.1	14.4	16.4	19.6	21.8
21–25	19.4	23.5	27.0	32.4	39.9	15.3	17.3	21.0	23.9	29.5	14.3	16.2	20.2	24.6	30.4	12.0	14.0	17.2	22.0	24.9
26–30	20.0	22.6	27.2	34.3	40.4	15.2	18.4	22.1	29.2	34.2	14.0	16.0	19.0	25.0	32.6	11.7	15.0	17.5	21.2	24.7
31–35	18.0	22.9	27.3	33.8	38.6	12.9	19.2	23.9	26.6	30.7	14.4	17.2	21.1	26.7	33.2	12.0	14.9	18.6	23.0	25.4
36–40	19.6	23.4	27.9	32.8	37.6	15.0	17.3	22.1	30.5	41.5	15.8	17.5	20.0	25.5	29.5	12.9	15.4	18.0	21.1	24.7
≥41	16.3	20.4	26.0	33.1	41.7	12.3	15.8	19.6	25.8	31.4	—	—	22.0	—	—	11.4	13.0	19.3	23.7	26.8
Age																				
≤24	—	—	—	—	—	—	—	—	—	—	9.4	10.0	11.0	12.4	13.2	—	—	11.6	—	—
25–29	12.5	14.0	15.4	16.8	18.6	11.1	13.1	14.7	16.0	18.0	9.7	11.0	12.5	14.7	17.4	9.0	10.2	12.0	14.7	16.6
30–34	13.8	15.2	17.0	19.2	22.0	12.1	14.0	15.8	17.8	20.0	10.6	12.7	15.0	17.5	20.0	9.4	11.7	13.8	16.4	18.6
35–39	14.9	16.8	19.3	22.5	26.6	13.4	15.0	16.8	19.8	22.6	12.0	14.5	17.4	20.4	25.2	10.4	12.1	14.6	18.0	21.5
40–44	16.0	18.5	21.7	26.0	31.9	13.3	15.6	18.0	21.1	25.0	13.7	16.0	19.2	22.6	27.0	10.6	12.8	15.8	18.2	21.4
45–49	17.3	20.0	23.9	29.0	35.0	14.0	16.0	19.3	22.3	25.3	13.8	16.0	19.5	24.0	30.0	11.4	13.0	16.0	20.0	23.1
50–54	17.3	20.1	24.1	29.8	35.6	14.0	16.5	19.3	22.8	25.7	14.0	16.1	20.0	24.0	29.7	11.0	13.4	17.0	21.3	24.0
55–59	17.4	20.9	25.1	30.0	36.4	14.5	17.0	20.0	25.0	31.4	14.0	16.5	19.5	23.9	29.9	10.5	13.6	16.1	20.3	24.0
60–64	17.3	20.5	24.9	30.0	35.9	15.0	17.5	21.3	25.2	30.5	13.6	15.3	18.0	22.5	29.2	14.0	15.4	17.8	21.8	23.9
65–69	16.2	21.1	25.0	30.6	39.3	12.3	16.7	20.6	24.1	30.0	15.5	17.5	20.6	24.2	25.3	9.8	12.9	16.2	20.2	22.8
≥70	12.8	17.9	21.7	27.6	40.0	12.8	14.5	18.4	22.8	24.0	16.8	17.0	19.5	30.4	52.2	12.9	13.8	16.5	17.7	15.9
Racial/ethnic identity																				
White	14.6	16.6	20.0	25.0	30.5	13.0	15.0	17.6	21.1	25.1	11.3	13.8	17.0	21.0	25.8	10.3	12.3	15.3	18.7	22.5
Black	15.2	18.0	21.9	24.2	28.0	15.2	16.9	18.0	21.0	23.6	12.6	15.0	18.6	20.5	24.0	12.6	14.2	15.9	20.0	22.0
Oriental	13.0	15.0	17.0	20.0	22.6	10.9	14.5	16.0	17.0	19.6	10.6	11.6	14.4	22.1	25.0	—	—	—	—	—
American Indian	—	—	22.5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Middle Eastern	—	—	20.2	—	—	0.0	—	—	—	0.0	—	—	—	—	—	—	—	—	—	—
Indian	12.8	16.0	17.6	21.7	24.5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Hawaiian-Polynesian	—	—	20.7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Other	15.0	17.3	21.3	26.6	32.8	15.1	17.3	19.1	24.8	32.5	11.4	14.8	17.3	21.5	25.8	12.1	13.5	17.4	20.2	24.1

Note. In thousands. D_1 = 1st decile, Q_1 = 1st quartile, Q_3 = 3rd quartile, D_9 = 9th decile. Deciles, quartiles, and medians are not given for *N*s less than 10. Only medians are given for *N*s greater than or equal to 10 and less than 20.

TABLE 6

Salary Distribution of APA Members in Most Frequent Combinations of Employment Setting and Subfield by Level of Highest Earned Degree

Employment setting	Subfield	Doctorates						Master's					
		N	D_1	Q_1	*Mdn*	Q_3	D_9	*N*	D_1	Q_1	*Mdn*	Q_3	D_9
Hospitals	Clinical	1,212	14.2	16.0	19.0	21.9	24.0	275	10.3	11.4	14.0	16.0	18.4
	Counseling	123	14.1	15.9	19.0	21.9	22.5	34	9.6	10.0	12.2	15.8	18.9
Clinics	Clinical	891	14.0	15.8	17.7	20.6	24.2	317	10.0	11.8	13.5	15.5	18.1
	Community	75	14.4	16.1	18.0	20.3	23.1	19	10.5	11.9	15.1	16.5	17.2
	Counseling	77	13.3	14.2	16.0	19.2	21.5	52	7.8	10.2	13.1	16.0	19.0
Independent/group practice	Clinical	1,089	17.5	23.0	30.0	40.0	50.0	141	12.1	18.0	25.0	30.0	39.0
	Counseling	84	17.3	20.0	25.0	32.0	43.5	35	11.9	15.0	18.0	20.7	30.5
	Industrial and organizational	36	19.5	21.9	28.0	37.4	47.0	11	13.4	17.5	20.0	27.8	38.0
Government agency	Clinical	218	15.0	17.0	20.5	25.0	29.9	100	10.4	11.8	14.0	16.6	19.2
	Counseling	77	14.9	16.3	20.8	24.4	28.1	79	11.0	13.5	17.5	20.0	23.7
	Community	60	15.8	18.0	22.5	26.4	29.0	14	12.3	14.1	15.0	18.0	20.0
	Experimental	60	16.2	18.1	23.0	28.1	29.9	8	13.6	16.7	18.7	22.0	29.9
	Industrial and organizational	54	14.1	18.2	24.2	28.0	29.7	56	14.0	16.6	19.4	24.9	28.1
	Social	55	13.6	17.2	24.0	27.0	29.4	8	13.3	14.1	16.8	18.7	20.7
Research establishment	Experimental	79	15.9	18.5	21.1	26.1	33.6	11	11.8	13.4	16.2	16.9	19.6
	Educational	70	16.1	18.5	22.0	26.0	30.4	9	15.7	16.5	18.5	24.6	30.0
	Industrial and organizational	64	15.7	18.0	22.5	26.5	32.3	15	15.3	17.5	19.7	23.5	26.6
	Social	45	14.9	17.3	22.0	29.0	32.8	6	13.3	13.5	14.7	15.9	16.4
	Clinical	42	15.0	17.0	20.2	23.2	27.4	10	10.8	12.0	14.6	19.2	22.7
Consulting firm	Industrial and organizational	241	17.3	20.2	30.0	37.5	52.0	76	12.9	18.0	21.0	28.0	36.8
	Clinical	56	17.0	20.0	27.6	35.0	40.0	12	12.6	14.7	15.0	25.6	36.0
Business/industry	Industrial and organizational	216	18.0	20.6	25.3	33.7	40.0	161	14.0	17.4	20.4	25.3	33.3
	Engineering	48	18.2	20.3	23.2	25.7	30.0	33	16.5	18.4	20.0	21.8	24.4
Correctional system	Clinical	66	13.6	15.8	18.4	21.0	23.0	57	12.0	14.0	15.6	17.3	19.6
Medical schools	Clinical	366	15.0	16.4	18.8	23.1	29.1	26	10.3	11.6	14.2	16.7	17.3
	Physiological	60	8.2	12.0	18.0	24.0	32.0	0	—	—	—	—	—
Universities, excluding medical schools	Clinical	1,350	14.6	16.2	19.3	24.3	29.3	67	9.9	11.6	13.6	17.4	19.9
	Counseling	968	14.0	15.6	18.0	21.3	26.0	77	9.2	10.5	13.9	16.0	18.6
	Experimental	930	14.4	16.1	19.0	24.0	29.9	14	9.4	10.8	15.0	17.1	24.8
	Educational	734	15.0	16.8	20.0	24.3	27.9	17	10.6	13.1	14.6	17.6	19.9
	Social	653	14.4	16.0	18.6	23.9	30.6	10	7.2	13.6	16.0	19.2	21.3
	Developmental	485	14.0	15.9	18.9	23.4	30.0	18	11.2	13.1	16.0	18.6	22.1
	Industrial and organizational	357	15.7	17.5	21.3	25.6	30.0	14	11.0	12.6	15.5	17.9	21.4
	Physiological	215	13.9	16.2	18.7	23.9	30.8	2	8.6	10.9	14.8	17.5	19.1
	Personality	180	15.6	17.2	21.3	27.9	33.1	7	13.4	14.5	16.0	18.5	20.7
	School	165	14.7	17.3	19.2	22.2	26.3	9	11.9	12.2	14.8	16.6	18.0
	Other	159	14.1	16.0	20.0	24.5	30.4	8	12.2	12.7	14.2	15.4	17.0
	Systems, methodology, and techniques	133	14.7	16.5	20.5	23.9	27.9	5	14.6	15.7	16.0	20.6	80.1
	Psychometrics	94	14.9	17.4	20.3	25.2	30.6	4	12.2	13.6	16.0	16.2	16.2
	General	93	14.7	16.5	19.8	23.9	31.7	13	10.8	12.2	13.4	18.5	81.3
	Community	84	14.5	16.0	20.5	26.7	31.1	2	15.6	16.5	17.5	19.4	21.8
	Comparative/animal	75	14.7	16.5	19.4	23.6	29.5	1	—	—	—	—	—
	Engineering	36	15.8	17.3	21.6	27.6	32.6	4	12.0	17.3	21.1	25.1	30.8
Four-year colleges	Clinical	392	13.6	15.3	18.6	22.1	26.6	50	10.0	12.0	15.0	16.6	19.8
	Experimental	340	13.6	15.3	16.8	20.0	24.0	26	11.8	12.3	13.4	15.0	17.4
	Counseling	290	13.5	15.0	17.3	20.3	23.9	44	10.7	12.1	14.9	17.3	18.0
	Educational	211	14.0	16.0	18.6	21.8	25.7	21	10.4	12.4	14.9	16.5	18.6
	Social	147	13.3	14.6	16.8	20.5	22.6	19	10.3	12.2	15.1	16.5	17.8
	Developmental	130	13.3	15.1	17.2	21.5	25.3	13	10.4	11.1	14.4	16.0	21.4
	General	75	13.6	15.0	17.8	22.1	26.6	18	12.9	13.2	14.0	15.6	17.4
	Personality	56	13.6	14.9	18.6	22.3	24.6	15	10.2	12.1	13.6	17.6	20.1
	Physiological	50	15.0	15.6	16.5	22.0	26.3	2	14.6	14.7	14.8	15.0	15.1
	Industrial and organizational	47	15.0	16.6	20.5	24.0	25.8	7	11.9	12.8	13.6	15.0	17.1
	School	45	13.9	17.0	19.5	22.4	24.8	8	12.6	13.5	14.2	14.7	19.4
Two-year colleges	Counseling	102	13.4	15.8	18.9	22.1	24.1	66	11.1	14.6	17.0	20.0	23.8
	Clinical	81	14.5	17.0	21.7	24.9	26.6	42	10.3	12.4	16.6	18.8	22.9
	Educational	33	14.5	16.4	20.0	21.5	24.2	17	10.2	13.1	15.5	19.7	22.5
	General	23	15.6	17.3	20.0	22.3	25.6	48	9.6	12.2	14.4	18.0	21.3
Regional school district	School	216	15.2	17.3	20.8	22.9	26.0	399	12.9	15.2	18.2	21.3	24.1
	Educational	49	15.6	17.8	20.0	23.0	26.0	22	14.2	17.2	19.9	22.6	24.0
	Clinical	45	15.3	16.6	21.8	24.2	27.6	48	11.8	13.7	17.7	20.5	22.8
Secondary school	School	85	16.5	18.6	22.0	24.8	27.2	139	15.0	16.6	20.0	22.7	25.3
	Clinical	35	14.8	15.6	22.0	26.3	26.6	29	14.0	15.5	18.4	21.2	23.2
	Counseling	29	14.4	15.7	20.4	23.9	25.8	31	11.5	13.7	16.0	19.7	25.5
Elementary school	School	118	15.0	17.6	21.1	23.9	26.7	292	14.0	16.0	19.0	22.1	25.5
	Clinical	45	15.0	16.0	20.0	23.9	32.0	37	13.0	14.7	16.5	20.0	21.8

Note. In thousands. D_1 = 1st decile, Q_1 = 1st quartile, Q_3 = 3rd quartile, D_9 = 9th decile. *N*s are the number in a cell, not the number on which the salaries are based.

attained the "high income" warranted by their education.

Such a generalization is hazardous, even if tempting. A glance at the years of experience section tempers the generalization by showing median salaries in the $27,000 range for doctorate men after 20 years of experience. The preponderance of young psychologists with fewer years of experience keeps the median salary low. One, of course, might argue that $27,000 after 20 years is not necessarily a demonstration of a "high earning potential," particularly with entrants to the field earning $15,000. The $27,000, however, is perhaps five times the 1952 entrance salary (albeit not in constant dollars) of those entering the field at that time. For what it is worth, the same reasoning implies that those now in graduate school would seem to have a high probability with a doctorate of earning better than $75,00 per year by 2000. We wonder what it will buy!

In Table 6 is presented one of a virtually inexhaustible combination of variables. Because subfield and work setting constitute a sociologically significant combination of occupational variables, however, it seems important and interesting enough to warrant presentation. Table 6 provides an enumeration of the number of individuals in selected subfield/work-setting combinations. To keep the size of the table within reasonable bounds, we have presented only those particular combinations having a sizable *N*.

A demographic variable of interest is geographic location. In Table 7 we have presented the numbers of psychologists having business addresses in the states and other geographical locations listed. Also provided is an abbreviated salary distribution by state containing the median salary and first and ninth deciles. These data are presented without comment except to note that some 27% of the membership is located in the two states of New York and California, states that comprised about 19% of the total population of the United States in 1972.

Salary is not the only source of professional income for psychologists. Consulting, royalties, part-time teaching positions, part-time fee-for-service activities, all serve to supplement the income of those employed full time. Referring back to Table 4, we see that nearly one third of those fully employed also have a part-time position. Because the questionnaire solicited information about total professional income, we can calculate the incremental income from these sources for those who report it.

In general, the results (not tabulated here) are reasonably uniform across characteristics, with a median of about $4,000 for male doctorates with five or more years of experience who have extra income and report it. There is a time cost for extra income, of course. Like many professionals, psychologists spend considerably more than 40 hours per week in their professional activities, as data from the survey reveal, a median of 46.6 hours per week for doctorate males (Boneau, 1973).

Is the APA Representative of Psychology in General?

The American Psychological Association is the largest society of psychologists in the United States. It is a major voice for the concerns of psychologists and a major vehicle for representing psychology to the public and to the makers of public policy. For this reason it is important to have some idea of the degree to which the various identifiable segments of psychology have access to and influence on the APA governance. Does the APA, in fact, speak for all psychology, or are some segments represented disproportionately or not at all?

This question, as phrased, is the result of rumination by an APA insider. A related question—perhaps the same question—couched in terms meaningful to an outsider asks for the *total* number of psychologists, APA members as well as nonmembers, and for the number of psychologists of various kinds. A description of the APA membership is an insufficient answer to those questions, but how insufficient? Clearly the 1972 APA Manpower Data base precludes definitive statements even of APA membership since only 77% of the members returned questionnaires. On the other hand, we have argued that the 77% sample is trivially different from the 1972 member population (see footnote 3).

Let us consider the doctorate holders first. Assuming a relatively unbiased sample, we can extrapolate our 77% return to the total membership and estimate the total doctorate population to be about 27,700. The additional 2,900 nonmember doctorates in the file yield a total of 30,600 doctorates in psychology in 1972. The National Academy of Sciences (1974) on a sampling basis has concluded the number of doctoral psychologists in 1972

TABLE 7

Annual Salaries of APA Members by Geographical Location, Level of Highest Earned Degree, and Sex

State	Doctorates												Master's												Total *N*
	Men						Women						Men						Women						
	N	D_1	Q_1	*Mdn*	Q_3	D_9	*N*	D_1	Q_1	*Mdn*	Q_3	D_9	*N*	D_1	Q_1	*Mdn*	Q_3	D_9	*N*	D_1	Q_1	*Mdn*	Q_3	D_9	
Alabama	123	14.4	16.0	18.6	23.1	25.8	25	13.1	13.6	15.5	17.0	18.0	17	9.7	—	13.2	—	—	13	8.9	—	12.0	—	12.7	178
Alaska	9	—	—	—	—	—	4	—	—	—	—	—	4	—	—	18.0	—	—	5	—	—	—	—	—	22
Arizona	174	16.0	17.3	20.0	22.6	27.9	34	13.8	15.4	17.3	20.0	22.8	20	11.5	15.0	17.4	18.5	20.1	15	—	—	16.3	—	—	243
Arkansas	69	14.5	15.3	18.0	20.5	23.2	11	—	—	16.8	—	—	15	—	—	14.6	—	—	4	—	—	—	—	—	99
California	2,012	14.9	17.0	20.6	25.5	32.0	579	13.0	15.0	17.5	20.7	23.6	391	13.0	15.7	19.1	22.2	25.7	228	11.8	15.3	18.0	21.4	23.3	3,210
Colorado	276	14.5	16.1	19.2	23.1	28.8	64	12.4	14.6	16.9	20.0	24.1	49	10.8	14.6	16.4	19.0	23.9	27	11.5	12.4	13.1	15.2	16.0	416
Connecticut	375	15.6	17.3	21.0	25.3	30.3	92	12.7	14.5	16.2	20.0	23.7	64	14.8	16.1	19.4	21.8	24.5	62	13.9	15.6	19.0	21.5	23.4	593
Delaware	35	13.2	16.0	18.0	20.7	24.7	10	—	—	16.1	—	—	23	12.1	14.6	16.0	19.2	25.2	14	—	—	13.3	—	—	82
District of Columbia	338	15.5	18.7	24.3	29.0	34.0	112	14.3	17.1	21.6	24.7	28.0	85	15.9	18.5	21.2	26.7	29.2	58	12.2	13.7	15.5	18.5	23.5	593
Florida	459	14.5	16.5	19.6	24.4	32.0	106	14.2	15.5	17.3	19.0	22.8	84	11.3	13.2	15.7	20.0	24.6	44	10.1	11.4	14.8	16.7	18.3	693
Georgia	261	14.9	16.6	20.0	24.0	30.0	46	12.4	14.4	16.4	20.0	22.9	28	10.4	12.7	15.0	17.2	21.1	13	—	—	12.5	—	—	348
Hawaii	76	14.9	16.2	19.4	24.5	31.6	18	—	—	18.0	—	—	12	—	—	16.7	—	—	9	—	—	—	—	—	115
Idaho	25	12.7	14.8	15.8	19.8	21.6	4	—	—	—	—	—	5	—	—	—	—	—	3	—	—	—	—	—	37
Illinois	855	15.0	17.3	20.6	25.9	32.0	191	14.5	16.0	18.0	21.6	24.0	232	12.4	14.6	18.0	21.7	25.9	138	11.6	13.4	15.8	19.1	21.8	1,416
Indiana	351	14.7	16.6	19.0	23.0	28.0	57	13.2	14.0	16.5	19.7	25.3	52	11.0	13.5	16.7	20.1	24.0	20	4.8	10.3	12.0	14.0	21.3	480
Iowa	178	14.7	16.0	18.6	22.5	27.3	38	14.6	15.3	17.2	19.5	21.8	39	12.4	14.8	16.0	18.1	20.9	18	—	—	14.0	—	—	273
Kansas	215	14.5	16.8	20.0	24.8	30.3	30	14.9	15.3	17.0	20.2	23.0	28	10.0	12.4	13.5	16.2	18.3	17	—	—	12.5	—	—	290
Kentucky	160	14.4	16.0	18.6	21.3	27.0	18	14.2	—	16.7	—	20.8	26	10.0	11.5	13.0	17.5	21.8	14	—	—	10.5	—	—	218
Louisiana	104	13.9	15.3	17.0	21.2	27.3	34	12.0	13.5	15.8	18.9	22.2	26	10.0	12.8	13.7	16.0	17.4	10	—	—	13.6	—	—	174
Maine	67	14.1	15.9	17.5	21.1	24.8	8	—	—	—	—	—	19	—	—	15.4	—	—	5	—	—	—	—	—	99
Maryland	495	16.0	18.0	22.1	27.0	30.9	130	14.4	16.8	20.0	23.1	27.5	90	12.2	15.0	17.3	20.0	23.0	60	10.4	13.4	17.0	21.0	23.4	775
Massachusetts	699	13.5	16.0	20.0	25.0	30.1	210	12.6	14.3	17.2	20.8	24.9	86	9.9	11.1	14.6	19.1	25.0	73	9.6	11.7	15.3	17.6	19.9	1,068
Michigan	658	15.5	17.7	21.3	26.0	32.0	127	12.4	15.6	18.6	21.9	24.8	158	12.7	15.0	17.6	22.5	26.0	89	12.0	14.0	16.0	20.6	22.8	1,032
Minnesota	320	16.0	18.0	20.5	24.5	30.2	53	14.1	16.0	17.8	21.3	23.5	55	10.6	13.7	16.5	21.9	30.0	28	11.7	13.6	17.0	21.8	23.7	456
Mississippi	78	15.5	16.4	18.0	20.8	24.0	10	—	—	15.8	—	—	8	—	—	—	—	—	3	—	—	—	—	—	99
Missouri	263	14.5	16.5	19.2	22.9	28.7	65	12.1	13.7	15.6	18.8	21.3	56	11.7	13.5	16.0	19.3	24.2	27	5.5	9.7	11.0	14.0	16.6	411
Montana	27	12.7	14.8	16.0	20.0	24.0	7	—	—	—	—	—	9	—	—	—	—	—	3	—	—	—	—	—	46
Nebraska	98	13.3	15.3	17.2	20.5	24.9	17	—	—	16.3	—	—	15	—	—	14.8	—	—	9	—	—	—	—	—	139
Nevada	34	14.7	17.8	20.1	23.6	31.0	5	—	—	—	—	—	4	—	—	—	—	—	4	—	—	—	—	—	47
New Hampshire	61	13.7	15.0	17.6	21.3	25.3	10	—	—	16.4	—	—	10	—	—	19.6	—	—	6	—	—	—	—	—	87
New Jersey	553	15.0	17.9	21.3	26.6	35.0	161	14.1	16.0	19.4	23.8	28.0	188	12.3	15.0	18.2	22.6	26.5	104	12.0	14.3	17.0	20.0	23.0	1,006
New Mexico	72	13.7	15.4	17.7	21.3	25.4	22	12.0	14.5	16.1	18.2	19.9	5	—	—	—	—	—	6	—	—	—	—	—	105
New York	2,252	15.5	18.0	22.6	29.0	38.6	815	14.0	16.0	19.8	24.0	30.6	509	12.8	15.6	19.0	24.6	29.2	407	12.0	14.0	17.0	20.6	24.5	3,983
North Carolina	310	14.1	15.9	18.8	23.7	28.4	59	12.0	14.6	16.0	18.6	21.0	41	10.8	12.2	14.2	17.9	30.1	27	10.1	11.4	12.6	13.1	15.9	437
North Dakota	29	14.8	15.9	18.0	20.0	24.0	6	—	—	—	—	—	4	—	—	—	—	—	3	—	—	—	—	—	42
Ohio	668	14.5	16.1	19.9	25.0	30.5	144	12.7	15.0	16.7	19.4	24.0	144	22.0	13.0	15.3	18.6	22.0	91	9.8	11.0	14.6	18.9	21.4	1,047
Oklahoma	143	14.0	15.9	18.0	21.7	29.1	25	13.4	14.1	16.5	18.4	22.8	25	8.2	10.4	13.2	17.8	20.6	6	—	—	-	—	—	199
Oregon	186	13.3	16.0	18.9	22.0	25.9	36	13.8	16.4	17.4	18.6	22.5	19	—	—	15.0	—	—	10	—	—	14.0	—	—	251
Pennsylvania	992	14.7	16.5	20.0	24.0	30.0	200	13.5	15.0	17.7	20.1	23.8	266	11.0	12.7	15.5	18.7	21.8	167	10.5	11.6	13.5	16.0	20.0	1,625
Rhode Island	60	15.0	17.1	20.6	23.9	27.9	14	—	—	16.0	—	—	11	—	—	13.8	—	—	14	—	—	14.7	—	—	99
South Carolina	92	13.5	15.1	17.3	20.7	27.0	17	—	—	16.0	—	—	31	10.1	10.6	13.0	16.0	17.6	9	—	—	—	—	—	149
South Dakota	28	14.5	15.6	17.3	21.3	25.4	2	—	—	—	—	—	4	—	—	—	—	—	0	—	—	—	—	—	34
Tennessee	232	14.1	16.0	18.0	21.9	26.4	39	12.4	14.3	16.1	18.7	22.2	44	10.0	12.0	13.9	16.0	18.1	17	—	—	12.0	—	—	332
Texas	646	14.4	16.0	18.7	23.1	30.0	140	12.5	15.0	16.4	20.5	25.0	159	10.5	11.3	14.0	18.4	24.4	94	9.4	10.8	12.0	14.0	16.3	1,039
Utah	119	14.9	16.0	18.1	21.8	26.5	9	—	—	—	—	—	12	—	—	11.2	—	—	3	—	—	—	—	—	143
Vermont	49	13.6	15.6	16.6	21.4	23.9	8	—	—	—	—	—	14	—	—	14.0	—	—	3	—	—	—	—	—	74
Virginia	363	14.7	16.5	20.0	24.6	30.0	46	14.9	16.0	17.6	19.2	22.4	80	13.3	14.5	17.9	22.0	26.4	66	10.4	12.5	14.6	16.5	21.5	555
Washington	258	14.0	15.3	17.8	22.0	26.9	52	12.6	13.8	16.2	18.4	21.5	46	11.2	13.3	18.5	20.0	21.9	20	12.0	13.0	17.2	18.3	19.8	376
West Virginia	74	14.1	15.5	17.5	20.9	24.3	16	—	—	16.5	—	—	24	11.6	12.3	14.8	16.8	18.7	7	—	—	—	—	—	121
Wisconsin	356	15.0	17.0	20.0	23.1	28.1	65	14.3	15.7	16.9	19.0	21.3	73	12.9	14.0	17.0	20.0	22.8	35	10.9	13.9	15.6	20.2	22.1	529
Wyoming	36	14.9	16.2	19.1	23.0	26.5	1	—	—	—	—	—	2	—	—	—	—	—	1	-	—	—	-	—	40
Territories and possessions	14	—	—	17.7	—	—	7	—	—	—	—	—	2	—	—	—	—	—	5	—	—	—	—	—	28
Canada	428	13.5	16.0	18.5	23.9	28.5	77	12.1	14.0	16.8	20.6	25.6	42	12.5	15.0	16.2	21.4	26.0	13	—	—	16.0	—	—	560
Total	16,855						4,076						3,455						2,127						26,513

Note. In thousands. D_1 = 1st decile, Q_1 = 1st quartile, Q_3 = 3rd quartile, D_9 = 9th decile. Deciles, quartiles, and medians are not given for *N*s less than 10. Only medians are given for *N*s greater than or equal to 10 and less than 20.

to be between 24,000 and 27,000 depending upon the definition.[10]

The NSF, for its own purposes, from NAS data leans to a total of 28,286 doctorate holders who either possess a PhD in psychology or who are now working in psychology. In general, the NAS and NSF definitions ignore psychologists who received a degree in education. These include educational, counseling, and school psychologists, perhaps others, who are eligible for membership and identify themselves with psychology, whether or not they join the Association. The number of these individuals is unknown and is inextricably entwined in the statistical summaries of all extant data systems including that of APA.[11] A reasonable guess is that the number of education-trained psychologists is in the neighborhood of 4,000, of whom an unknown fraction but probably about 75% are already in our files. Committing *petit legerdemian*[12] with these figures yields an estimate in the range of 31,000–32,000 as the total population of doctorate psychologists APA-style in 1972, and a conclusion that about 90% are members of the APA.[13] Ignored in such manipulation are such factors as the knowledge that 10% of the doctorate psychologists are employed in another field and that some 10% of those employed in psychology have a doctorate in another field (NAS, 1974). Hopefully, such complications are either built into the gross estimates or cancel themselves out.

A slightly different approach yields essentially the same gross conclusion but provides a needed fine-grain discrimination among subfields. The data come from the NAS Doctorate Record file compiled from a yearly survey of new doctorate recipients in all fields. Published data include subfields within fields and permit us to compare the actual number of recipients in various subfields with the numbers of respondents who received a doctorate in those subfields. Such a comparison in selected subfields reveals that about 87% of the doctorates are APA members.[14] According to these data virtually 100% of the clinical psychologists are members but only, perhaps, 75% of the experimental psychologists. We may conclude on the basis of the two arguments that APA membership includes about 90% of the eligible doctorate holders.

As for master's-level members, the data are much harder to find. Our extrapolated total of 7,300 in the membership is a percentage of some unknown base. According to figures from the Office of Education (HEW, 1972) and the American Council on Education (1970), approximately 60,000 master's degrees were awarded in psychology between 1932 and 1972. According to our estimates, some 24,000 of these were awarded the PhD at some later date, leaving 36,000 having the master's as their highest degree. Assuming 6,000 have died and one third have left the field, we arrive at a figure of some 20,000 masters. Of our 7,500 master's holders, perhaps 5,000 received their MA in psychology departments, the remainder elsewhere. Thus, our 5,000 psychology masters provide the basis for a guesstimate that approximately one fourth of those eligible are members. The remaining master's-level members are predominantly school and counseling psychologists, whose number are unknown but must be in the neighborhood of 30,000–40,000 depending upon how they are defined.

In short, we may conclude that the APA contains a preponderance of the doctorate psychologists in the country and may be considered to represent them. The Association, however, would seem not to represent master's-level psychologists and, in particular, those from the educational subfields.

[10] The NAS counts doctorate degree in a field, identification with a field, or employment in a field as separate defining characteristics. By these definitions, the 1972 NAS sample produces estimates of 24,483, 26,655, and 24,598, respectively (NAS, 1974).

[11] Fortunately, Mark II of the APA system will permit a differentiation between EdDs and PhDs, thus shedding light upon the collegial origins of psychologists.

[12] A technical procedure apparently utilized by most, if not all, manpower experts, at least in Washington, D.C. This exercise may serve to enlighten those who wonder where the numbers come from.

[13] By 1974 the number of doctorate psychologists has increased by some 5,000. There are now some 37,000 doctorate psychologists. The prospect seems to be for an even more rapid increase in the next few years (Cuca, 1974).

[14] Data were compared for the subfields experimental-comparative-physiological, personality-social, and clinical to concentrate on those areas in which NAS and APA might be expected to have the same individuals. Over a 20-year period from 1952 to 1972, the number of doctorate recipients in these subfields was 12,979. The total extrapolated 1972 APA membership in these subfields was about 11,300 or 87%. The basic reference is NAS (1967). NAS updates the figures yearly in a pamphlet entitled "Summary Report."

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Proposed Bylaws Amendment

At the meeting of August 31 and September 2, 1974, the Council of Representatives, on recommendation of the Board of Directors, approved submission of the following Bylaws amendment to the APA membership for approval:

Amendment to Article I of the APA Bylaws to add the words "and profession" after the word "science."

Article I—Objects [1]

The objects of the American Psychological Association shall be to advance psychology as a science *and profession* and as a means of promoting human welfare by the encouragement of psychology in all its branches in the broadest and most liberal manner; by the promotion of research in psychology and the improvement of research methods and conditions; by the improvement of the qualifications and usefulness of psychologists through high standards of education and achievement; by the establishment and maintenance of the highest standards of professional ethics and conduct of the members of the Association; by the increase and diffusion of psychological knowledge through meetings, professional contacts, reports, papers, discussions, and publications; thereby to advance scientific interests and inquiry, and the application of research findings to the promotion of the public welfare.

[1] Italicized material is added.

APA-Approved Internships for Doctoral Training in Clinical and Counseling Psychology: 1974

AMERICAN PSYCHOLOGICAL ASSOCIATION

The Committee on Accreditation has approved for doctoral training in professional psychology the internships offered by the agencies listed below. The criteria for evaluating these programs can be obtained from the Educational Affairs Office.

The current criteria for accreditation of internship training programs no longer make the distinction between clinical and counseling internship programs; therefore, the separate listing of approved programs by professional specialty has been dropped. Readers desiring information on training goals offered at specific agencies are encouraged to write directly to the agencies.

Each of the agencies listed has met the same qualitative standards with respect to adequacy of training. The list is alphabetical by agencies.

Fully Approved Programs

University of Alabama Medical Center, Box 314, University Station, Birmingham, Alabama 35294.

Albert Einstein College of Medicine, 1300 Morris Park Avenue, Bronx, New York 10461.

American University Counseling Center, Mary Graydon Center, Nebraska and Massachusetts Avenues, N.W., Washington, D.C. 20016.

Area A Community Mental Health Center, 3246 P Street, N.W., Washington, D.C. 20007.

Area C Community Mental Health Center, 1905 E Street, S.E., Washington, D.C. 20003.

University of Arkansas Medical Center, 4301 W. Markham Street, Little Rock, Arkansas 72205.

Astor Home for Children, 36 Mill Street, Rhinebeck, New York 12572.

Baylor College of Medicine, Department of Psychiatry, 1200 Moursund Avenue, Houston, Texas 77025.

University of California at Los Angeles, School of Medicine, Division of Medical Psychology, Department of Psychiatry, Los Angeles, California 90024.

Camarillo State Hospital, Box A, Camarillo, California 93010.

Carl V. Morrison Center for Youth and Family Service, 3355 S.E. Powell Boulevard, Portland, Oregon 97202.

Central Louisiana State Hospital, P.O. Box 31, Pineville, Louisiana 71360.

Chicago-Read Mental Health Center, 6500 W. Irving Park Road, Chicago, Illinois 60634.

Children's Hospital Medical Center, Division of Psychology, Department of Psychiatry, 300 Longwood Avenue, Boston, Massachusetts 02115.

Children's Memorial Hospital, Division of Child Psychiatry, 710 W. Fullerton, Chicago, Illinois 60614.

Children's Psychiatric Center, Inc., 59 Broad Street, Eatontown, New Jersey 07724.

Children's Village, Dobbs Ferry, New York 10522.

University of Colorado Medical Center, 4200 E. Ninth Avenue, Denver, Colorado 80220.

Colorado State University Counseling Center, Fort Collins, Colorado 80521.

Columbia-Presbyterian Medical Center, Psychiatric Institute, 722 W. 168th Street, New York, New York 10032.

Community Child Guidance and Child Evaluation Clinics, Washington University School of Medicine, 369 N. Taylor Avenue, St. Louis, Missouri 63108.

Community Guidance Center of Mercer County, 532 W. State Street, Trenton, New Jersey 08618.

Connecticut Valley Hospital, P.O. Box 351, Middletown, Connecticut 06457.

Convalescent Hospital for Children, 2075 Scottsville Road, Rochester, New York 14623.

Cornell University Medical College & New York Hospital-Cornell Medical Center, Department of Psychiatry, Westchester Division, 21 Bloomingdale Road, White Plains, New York 10605.

Dede Wallace Center, 700 Craighead Avenue, Nashville, Tennessee 37204.

Des Moines Child Guidance Center, 1206 Pleasant Street, Des Moines, Iowa 50309.

Devereux Foundation, Institute of Clinical Training, Devon, Pennsylvania 19333.

Duke University Medical Center, Division of Medical Psychology, Department of Psychiatry, Box 2995, Durham, North Carolina 27710.

Eastern Pennsylvania Psychiatric Institute, Adult Unit, Henry Avenue and Abbottsford Road, Philadelphia, Pennsylvania 19129.

Eastern Pennsylvania Psychiatric Institute, Children's Unit, Henry Avenue and Abbottsford Road, Philadelphia, Pennsylvania 19129.

Emma Pendleton Bradley Hospital, 1011 Veterans Memorial Parkway, Riverside, Rhode Island 02915.

Fort Logan Mental Health Center, 3520 W. Oxford Avenue, Denver, Colorado 80236.

Galesburg State Research Hospital, Galesburg, Illinois 61401.

University of Hawaii, Counseling and Testing Center, 2327 Dole Street, Honolulu, Hawaii 96822.

Hillcrest Children's Center, 1325 W Street, N.W., Washington, D.C. 20009.

University of Illinois at the Medical Center, Department of Psychiatry, 912 South Wood Street, P.O. Box 6998, Chicago, Illinois 60680.

Illinois State Psychiatric Institute, 1601 W. Taylor Street, Chicago, Illinois 60612.

Indiana University School of Medicine, Section of Psychology of the Department of Psychiatry, 1100 W. Michigan Street, Indianapolis, Indiana 46202.

Institute of Living, 200 Retreat Avenue, Hartford, Connecticut 06106.

Irving Schwartz Institute for Children and Youth, Ford Road and Monument Avenue, Philadelphia, Pennsylvania 19131.

J. Hillis Miller Health Center, University of Florida, Department of Clinical Psychology, Box 765, Teaching Hospital & Clinics, Gainesville, Florida 32601.

Jewish Board of Guardians, 120 W. 57th Street, New York, New York 10019.

Jewish Hospital of St. Louis, 216 S. Kingshighway, St. Louis, Missouri 63110.

Judge Baker Guidance Center, 295 Longwood Avenue, Boston, Massachusetts 02115.

Kaiser-Permanente Medical Care Program, 1515 North Vermont Avenue, Los Angeles, California 90027.

Kennedy Child Study Center, 1339 20th Street, Santa Monica, California 90404.

Lafayette Clinic, 951 E. Lafayette, Detroit, Michigan 48207.

Langley Porter Neuropsychiatric Institute, University of California School of Medicine, San Francisco, California 94143.

Larue D. Carter Memorial Hospital, 1315 W. 10th Street, Indianapolis, Indiana 46202.

Letterman Army Medical Center, Psychology Service, San Francisco, California 94129.

Los Angeles County, University of Southern California Medical Center, 1934 Hospital Place, Los Angeles, California 90033.

Los Angeles Psychiatric Service, 8770 Whitworth Drive, Los Angeles, California 90035.

Malcolm Bliss Mental Health Center, 1420 Grattan Street, St. Louis, Missouri 63104.

University of Maryland School of Medicine, Institute of Psychiatry and Human Behavior, Baltimore, Maryland 21201.

Massachusetts Mental Health Center, Harvard Medical School, Department of Psychiatry, 74 Fenwood Road, Boston, Massachusetts 02115.

McLean Hospital, 115 Mill Street, Belmont, Massachusetts 02178.

Medical Department Activity (MEDDAC), Psychology Service, Fort Ord, California 93941.

Mendota Mental Health Institute, 301 Troy Drive, Madison, Wisconsin 53704.

Michael Reese Hospital, 29th Street & Ellis Avenue, Chicago, Illinois 60616.

University of Minnesota Health Sciences Center, Division of Health Care Psychology, Box 393-Mayo, Minneapolis, Minnesota 55455.

University of Mississippi School of Medicine, 2500 N. State Street, Jackson, Mississippi 39216.

University of Missouri Testing and Counseling Service, 205 Parker Hall, Columbia, Missouri 65201.

Mount Zion Hospital and Medical Center, 1600 Divisadero Street, San Francisco, California 94115.

Napa State Hospital, Box A, Imola, California 94558.

Nassau County Medical Center, Hempstead Turnpike, East Meadow, New York 11554.

National Naval Medical Center, Bethesda, Maryland 20014.

Nebraska Psychiatric Institute, University of Nebraska College of Medicine, 602 S. 45th Street, Omaha, Nebraska 68105.

New York University-Bellevue Medical Center, Department of Psychiatry, New York, New York 10016.

New York University Medical Center, Institute of Rehabilitation Medicine, 400 E. 34th Street, New York, New York 10016.

Norfolk Regional Center, Box 1209, Norfolk, Nebraska 68701.

Norristown State Hospital, Psychology Department, Norristown, Pennsylvania 19401.

North Carolina Memorial Hospital, Psychological Services, Department of Psychiatry, Chapel Hill, North Carolina 27514.

Northwestern University Medical School, 303 E. Chicago Avenue, Chicago, Illinois 60611.

Norwich Hospital, Department of Psychological Services, P.O. Box 508, Norwich, Connecticut 06360.

Ohio State University Health Center, Division of Clinical Psychology, Upham Hall, 473 W. 12th Avenue, Columbus, Ohio 43210.

University of Oklahoma Health Sciences Center, Department of Psychiatry and Behavioral Sciences, P.O. Box 26901, Oklahoma City, Oklahoma 73190.

University of Oregon Medical School, Department of Medical Psychology, 3181 S.W. Sam Jackson Park Road, Portland, Oregon 97201.

Patton State Hospital, Drawer B, Patton, California 92369.

Pittsburgh Child Guidance Center, 201 De Soto Street, Pittsburgh, Pennsylvania 15213.

Postgraduate Center for Mental Health, 124 E. 28th Street, New York, New York 10016.

University of Rochester School of Medicine and Dentistry, Department of Psychiatry, 260 Crittenden Boulevard, Rochester, New York 14642.

Roosevelt Hospital, 428 W. 59th Street, New York, New York 10019.

Rush-Presbyterian-St. Luke's Medical Center, Department of Psychology and Social Sciences, 1750 W. Harrison Street, Chicago, Illinois 60612.

St. Elizabeth's Hospital, Division of Clinical and Community Services, Overholser Training and Research Division, Clinical Psychology Training Section, 2700 Martin Luther King, Jr. Avenue, S.E., Washington, D.C. 20032.

St. Vincent's Hospital and Medical Center of New York, 144 W. 12th Street, New York, New York 10011.

San Fernando Valley Child Guidance Clinic, 7335 Van Nuys Boulevard, Van Nuys, California 91405.

South Shore Mental Health Center, 77 Parking Way, Quincy, Massachusetts 02169.

Spring Grove State Hospital, Baltimore, Maryland 21228.

Springfield Hospital Center, Sykesville, Maryland 21784.

SUNY, Upstate Medical Center, 750 E. Adams Street, Syracuse, New York 13210.

University of Tennessee College of Medicine, Department of Psychiatry, 42 N. Dunlap Street, Memphis, Tennessee 38103.

University of Texas, Counseling-Psychological Services Center, West Mall Office Building 303, Austin, Texas 78712.

University of Texas Health Science Center at Dallas, 5323 Harry Hines Boulevard, Dallas, Texas 75235.

University of Texas Medical Branch, Division of Psychology, Galveston, Texas 77550.

University of Texas Medical School at San Antonio, 7703 Floyd Curl Drive, San Antonio, Texas 78284.

Texas Research Institute of Mental Sciences, Texas Medical Center, 1300 Moursund Avenue, Houston, Texas 77025.

Topeka State Hospital, 2700 W. Sixth Street, Topeka, Kansas 66606.

University Hospitals of Cleveland and School of Medicine, Case Western Reserve University, 2065 Adelbert Road, Cleveland, Ohio 44106.

Vanderbilt-Peabody Program, Vanderbilt University, Department of Psychiatry, Nashville, Tennessee 37232.

Veterans Administration Hospital, 2200 Gage Boulevard, Topeka, Kansas 66622.

Walter Reed Army Medical Center, Psychology Service, Washington, D.C. 20012.

University of Washington, Department of Psychiatry & Behavioral Sciences, Seattle, Washington 98195.

West Virginia University Medical Center, Department of Behavioral Medicine and Psychiatry, Morgantown, West Virginia 26505.

Western Missouri Mental Health Center, 600 E. 22nd Street, Kansas City, Missouri 64108.

Wichita Guidance Center, Wichita Collaborative Psychology Internship Program, 3422 E. Douglas, Wichita, Kansas 67208.

Wilford Hall USAF Medical Center (SGHHC), Department of Mental Health, Lackland AFB, Texas 78236.

William S. Hall Psychiatric Institute, P.O. Box 119, Columbia, South Carolina 29202.

University of Wisconsin Medical School, Department of Psychiatry, 427 Lorch Street, Madison, Wisconsin 53706.

Worcester State Hospital, 305 Belmont Street, Worcester, Massachusetts 01604.

Worcester Youth Guidance Center, 275 Belmont Street, Worcester, Massachusetts 01604.

Yale University School of Medicine, 333 Cedar Street, New Haven, Connecticut 06510.

Programs on Probation

Atascadero State Hospital, P.O. Drawer A, Atascadero, California 93422.

Central State Hospital, Milledgeville, Georgia 31062.

Fairfield Hills Hospital, Newtown, Connecticut 06470.

Hawaii State Hospital, 45-710 Keehala Road, Kaneohe, Hawaii 96744.

Scientific Awards Announcement

The Committee on Scientific Awards is accepting nominations for its award program. The Committee selects up to three persons as recipients of the **Distinguished Scientific Contribution Award** who, in its opinion, have made the most distinguished theoretical or empirical contributions to scientific psychology in recent years.

A new award, the **Distinguished Contribution for Applications in Psychology,** has been authorized by the Board of Directors and will be given for the third time this year. This is presented to an individual who, in the Committee's opinion, has engaged in a program of research which is systematic and applied in character.

The awards are subject to the following limitations: (*a*) members of the Committee, former recipients of the awards, the President and the President-elect of the APA shall be ineligible. (*b*) The Committee shall seek diversity in selecting recipients, avoiding as far as possible the selection of more than one person representing a specialized topic, a specific material, a given method, or a particular application.

Names and appropriate information which will guide the Committee on Scientific Awards in conducting an intensive career review and evaluation should be forwarded to Office of Scientific Affairs, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. *Deadline for nominations is January 15, 1975.*

APA-Approved Doctoral Programs in Clinical, Counseling, and School Psychology: 1974

AMERICAN PSYCHOLOGICAL ASSOCIATION

The Committee on Accreditation has approved the doctoral training programs in clinical, counseling, and school psychology that are conducted by the institutions listed below. In the institutions listed, the approved programs are directed by the department of psychology unless otherwise indicated. Programs that have not requested evaluation, and programs that have been evaluated but not approved, are not included in the list. The criteria for evaluating these programs can be obtained from the Educational Affairs Office.

The Committee on Accreditation has dropped the "initial" category of accreditation. Therefore, programs that meet all of the criteria, including newly approved programs, are classified as fully approved.

Inclusion of an institution in this list indicates approval of doctoral programs in clinical, counseling, and school psychology only. Inclusion or noninclusion carries no implications for other graduate programs in psychology or for programs of graduate education in other disciplines.

The institutions listed below have been reported to the United States Public Health Service, to the Veterans Administration, and to the Surgeon General's Office, Department of the Army, as conducting at the present time approved programs of doctoral training in the areas indicated.

Fully Approved Programs

CLINICAL PSYCHOLOGY

Adelphi University, Institute of Advanced Psychological Studies
Alabama, University of
American University
Arizona, University of
Arkansas, University of
Boston University
Bowling Green State University
Brigham Young University
California, University of (Berkeley)
California, University of (Los Angeles)
Case Western Reserve University
Catholic University of America
Cincinnati, University of
City University of New York (City College)
Clark University
Colorado, University of
Connecticut, University of
Denver, University of
Duke University
Emory University
Florida State University
Florida, University of
Fordham University
Fuller Theological Seminary, Graduate School of Psychology
George Peabody College for Teachers
George Washington University
Georgia State University
Georgia, University of
Hawaii, University of
Houston, University of
Illinois, University of
Illinois, University of, PsyD Program
Indiana University
Iowa, University of
Kansas, University of
Kent State University
Kentucky, University of
Long Island University
Louisiana State University
Louisville, University of
Loyola University (Chicago)
Manitoba, University of
Maryland, University of
Massachusetts, University of
McGill University
Memphis State University
Miami, University of (Florida)
Miami University (Ohio)
Michigan State University
Michigan, University of
Minnesota, University of
Mississippi, University of
Missouri, University of
Montana, University of
Nebraska, University of
Nevada, University of
New Mexico, University of
New York University, Graduate School of Arts and Sciences
North Carolina, University of
North Dakota, University of
Northern Illinois University
Ohio State University
Ohio University
Oklahoma State University

Oregon, University of
Pennsylvania State University
Pennsylvania, University of
Pittsburgh, University of
Purdue University
Rochester, University of
Rutgers—The State University
St. Louis University
South Carolina, University of
South Dakota, University of
Southern California, University of
Southern Illinois University
State University of New York at Buffalo
State University of New York at Stony Brook
Syracuse University
Teachers College, Columbia University
Temple University
Tennessee, University of
Texas Tech University
Texas, University of
Utah, University of
Vanderbilt University
Vermont, University of
Washington State University
Washington, University of
Washington University (St. Louis)
Waterloo, University of
Wayne State University
West Virginia University
Wisconsin, University of
Wyoming, University of
Yale University
Yeshiva University

COUNSELING PSYCHOLOGY

Arizona State University, Department of Counselor Education
Catholic University of America
Colorado State University
Florida, University of
George Peabody College for Teachers
Illinois, University of
Kansas, University of, Interdepartmental Committee on Counseling Psychology
Maryland, University of
Michigan, University of
Minnesota, University of, Department of Psychology, College of Liberal Arts
Minnesota, University of, Department of Educational Psychology, College of Education
Missouri, University of, Department of Psychology and Counseling and Personnel Services
Nebraska, University of, Department of Educational Psychology and Measurements
Notre Dame, University of, Department of Graduate Studies in Education
Ohio State University
Oregon, University of, Department of Counseling
Southern Illinois University
Teachers College, Columbia University
Texas Tech University
Texas, University of, Department of Educational Psychology
Utah, University of, Department of Educational Psychology

SCHOOL PSYCHOLOGY

Hofstra University
Minnesota, University of, Division of Educational Psychology
Rutgers—The State University, Department of Psychological Foundations
South Carolina, University of
Teachers College, Columbia University
Texas, University of, Department of Educational Psychology

Provisionally Approved Programs

Provisional approval is a candidacy category for programs that have not met all of the current standards but have a reasonable expectation of meeting standards within three years.

CLINICAL PSYCHOLOGY

Delaware, University of
Illinois at Chicago Circle, University of
Northwestern University Medical School, Department of Psychiatry
Rhode Island, University of

COUNSELING PSYCHOLOGY

Temple University, Department of Counselor Education and Educational Psychology

Comment

A Courageous Man

Beit-Hallahmi (February 1974) is a courageous man. Psychologists who take on the prevailing theories about intelligence and its distribution face the wrath of leaders in one large sector of our science. To confront as well the swelling ranks of clinical, counseling, and community psychology is tantamount to setting up a lonely high noon encounter.

No wonder Beit-Hallahmi concluded his article with a poignant note about the role of the critic. Standing against the mainstream in one's own field is alienating, as Darwin revealed about himself (Gruber, 1973). By the time he sufficiently overcame his anxiety to be able to reveal his theories, Darwin at least had the necessary evidence to document them. In a science like psychology, however, which is both ahistorical and dominated by the canons of the British empiricist traditions, logical inferences about changes in human behavior, formulated under conditions different from those of today, carry no weight whatsoever.

The hard-nosed psychologist values here-and-now data. Predictions made by American and British psychologists in the 1920s and 1930s based on theories of intelligence and contemporary test results proved to be grossly inaccurate.

Those historical facts are either forgotten or ignored today as other psychologists using the same theories and current test data make similar highly publicized statements, again with serious consequences for the well-being of people.

Let no psychologist be deluded into believing that these here-and-now comparative data do not leave their imprint on the minds of teachers, parents, and children, as well as psychologists. The pity of it is that psychology permits only defensive measures in counteracting the use of contemporary data; one must go to other disciplines for more powerful intellectual weapons. It is regrettable but true that the nature of psychology makes it more amenable to sustain than to alter the social-political status quo; it is not simply to agree with Kuhn (1970) that psychologists work within the limits of the prevailing "paradigm." Beyond that, the possibilities of generating radically different paradigms from within psychology are remote.

REFERENCES

Beit-Hallahmi, B. Salvation and its vicissitudes: Clinical psychology and political values. *American Psychologist*, 1974, 29, 124–129.

Gruber, H. Courage and cognitive growth in children and scientist. In M. Schwebel & J. Raph (Eds.), *Piaget in the classroom*. New York: Basic Books, 1973.

Kuhn, T. S. *The structure of scientific revolutions*. Chicago, Ill.: University of Chicago Press, 1970.

Milton Schwebel
Rutgers—The State University

In Response to "Up with Our Foremothers"

I enjoyed the article by Bernstein and Russo (February 1974). I generally agree with the purpose of the article and believe the situation described needs correction. However, as indicated by the following quotation, the article included a reflection upon our profession which may or may not be warranted:

> We will probably never know how much work was done by women but credited to men: how many footnotes of appreciation should rightfully have been coauthorship, how many times junior authorship should have been senior authorship, or how many times it was the male coauthor who should have received the footnote [p. 131].

It seems to me that this statement should be made only if there is a record of cases of the type described. As one of the women listed in an opening question in the article, I might indicate that my own experience has been quite the opposite of the practices suggested in the quotation. I was, in actuality, my husband's student before and for some years after I took my degree. I also worked as his assistant and statistical consultant for some six or seven years, and enjoyed being associated with his interesting research and theoretical system. During that time he made a practice of including my name on publications which did not call for that type of recognition.

My own thing later became the development of a career in clinical psychology, which suits my own interests. I prefer a separate career and recommend this type of arrangement. However, I learned a great deal in those early years which I now value very much. I have thus described my experiences for the record to point out that not all man–woman relationships are of the type Bernstein and Russo described. We should not conclude that our profession can generally or even occasionally be characterized in this manner without some evidence.

REFERENCE

Bernstein, M. D., & Russo, N. F. The history of psychology revisited: Or, up with our foremothers. *American Psychologist*, 1974, 29, 130–134.

Carolyn K. Staats
Honolulu, Hawaii

On Bernstein and Russo

Let us not forget that the "McCollough effect" in vision research was

first described by Celeste McCollough, nor forget the early contributions of Christine Ladd-Franklin to color theory.

REFERENCE

Bernstein, M. D., & Russo, N. F. The history of psychology revisited: Or, up with our foremothers. *American Psychologist*, 1974, 29, 130–134.

CATHARINE H. BROWN
Rochester, New York

The Arrival of Women's Lib

In her discussion of studies of achievement motivation in college women, Alper (March 1974) skirted a much more general phenomenon than the inconsistent findings of her research. The generality was that social-psychological-motivational theories of "all" sorts can be applied better to men than to women.

A speculative explanation of this sex difference is that, by and large, theories are spun by men, the implications of the theories are derived by men, and the experimental tests are designed by men. If the theories were tight, if the derivations of hypotheses were strictly logical, and if the operationalization of definitions for experimental tests involved necessary criteria (or if men understood women), the differences would disappear. The notion that achievement motivation in a society produces technological progress is, indeed, more notion than theory. That making good grades indexes achievement more than, or rather than, docility is a hunch—reasonable, but arguable. When the theorizing and experimentation are done by men or by women whose selection and training were dominated by men, tests are masculinized to a point that more often than not misses the .05 level of confidence when applied to women.

A second speculation is that when these differences no longer plague the psychological literature, women's lib will have arrived.

REFERENCE

Alper, T. G. Achievement motivation in college women: A now-you-see-it–now-you-don't phenomenon. *American Psychologist*, 1974, 29, 194–203.

PAUL B. PORTER
University of Utah

On "Learning Theory"

Freedman, Cohen, and Hennessy (March 1974) essentially stated the general idea that people act rationally as they (the people) see it, even in laboratory-type learning experiments. They reported some difficulties in devising an experiment that would support the contention but eventually did so by presenting a display to which the subjects could apply their past experience intelligently. They concluded that the behavior of people in the laboratory depends upon the expectancies people bring into the laboratory as well as what happens to them thereafter.

Gladstone anticipated the report and went beyond it in several ways. Gladstone used a standard Skinnerian operant conditioning and extinction design except that the rewards were open to observation and, therefore, the subjects could see when they had obtained the last reward. Under those conditions: (*a*) fewer college students yielded extinction responses and the group as a whole yielded fewer extinction responses than the group that could not see the rewards (Gladstone, 1966b); (*b*) most college students yielded extinction responses even when they could observe the exhaustion of the rewards (Gladstone, 1966b); (*c*) the number of students yielding extinction responses in the reward-visible situation was cut down a great deal when punishment was added for each response (Gladstone, 1966a; Gladstone & Miller, 1968); (*d*) almost no subjects yielded extinction responses when they were forced to pay attention to the visible rewards in addition to being punished for responding (Gladstone, 1968); (*e*) older children are more prone than college students to stop abruptly when the rewards are gone (Gladstone, 1966a, 1969); (*f*) young children are more likely than older children to yield extinction responses (Gladstone, 1966a, 1969); and (*g*) almost all young children act completely rationally (yield no extinction responses) when the rewards are visible and the situation they are asked to deal with is very simple (Gladstone, manuscript in preparation).

I am happy to see the idea make the big time.

REFERENCES

Freedman, P. E., Cohen, M., & Hennessy, J. Learning theory: Two trials and tribulations. *American Psychologist*, 1974, 29, 204–206.

Gladstone, R. *Effect of extrinsic rewards on motivation.* (Cooperative Research Project No. 5-8164-2-12-1) Washington, D.C.: U.S. Department of Health, Education, and Welfare, 1966. (a)

Gladstone, R. The significance of the extinction situation. *Journal of Psychology*, 1966, 64, 23–28. (b)

Gladstone, R. An attempt at complete control (complete elimination) of extinction responses. *Journal of Psychology*, 1968, 70, 241–247.

Gladstone, R. Age, cognitive control and extinction. *Journal of Experimental Child Psychology*, 1969, 7, 31–35.

Gladstone, R., & Miller, M. Rationality, motivation, and extinction. *Journal of Psychology*, 1968, 68, 33–38.

ROY GLADSTONE
Oklahoma State University

The Observer's Temporal Scope

Freedman, Cohen, and Hennessy (March 1974) hypothesized that "initially, in the learning situation, when an organism makes a response and that response is followed by reinforcement, there is an increase in the probability that the response will *not* be repeated under the same stimulus conditions [p. 204]." Moreover, it is their contention that if the

above hypothesis is confirmed, its confirmation narrows the boundaries under which the most basic law of learning theory, the law of effect, holds. Their argument seems to be based on the assumption that in accordance with the law of effect, when an organism makes a response and that response is followed by reinforcement, there is an increase in the probability that that *particular* response will be repeated under the same stimulus conditions. Clearly, then, a critical and central issue emerges: How does one identify the reinforced response? An attempt will be made in this reply to shed some light on this issue.

The fact that when given two alternatives from which to choose there is a tendency among human and nonhuman organisms to select on the second opportunity the opposite alternative from that chosen on the first opportunity, even if the first was reinforced, can be interpreted in at least two ways. An initial interpretation, favored by Freedman and his associates, is that under these particular circumstances the law of effect is inoperative because a response followed by reinforcement did not increase in its probability of occurrence under the same stimulus conditions. Another, more parsimonious interpretation becomes available when the observer's temporal scope is expanded. To begin with, one may examine the potential impact of an observer's temporal scope under circumstances that are different from the study of Freedman et al.

An observer may look at a pigeon that is continuously pecking a green light until the pigeon collapses from exhaustion. He may also note that each peck is not followed by any conceivable reinforcement. This observer may then tentatively conclude that a basic law of learning does not hold in this particular situation because a response that is not reinforced should not increase in its probability of occurrence under the same stimulus configuration. And yet this is precisely what he is witnessing. Or is it? What is the response? And what is being reinforced?

If the observer extends his temporal scope he may note that before he entered the laboratory the pigeon was intermittently reinforced for a successively larger number of pecks. If a long series of pecks, indeed only a long and continuously expanding series of pecks, were previously reinforced, then the law of effect is clearly not violated (Ferster & Skinner, 1957). The response that was followed by reinforcement did increase in its probability of occurrence under the same stimulus conditions.

Let us return to the study of Freedman, Cohen, and Hennessy. Freedman et al.'s interpretation of the confirmation of their hypothesis is derived from a particular temporal scope. They observe a subject who is choosing between two boxes. Underneath one box there is a quarter. Luckily the subject chooses this particular box. Nevertheless, on the next occasion, they observe that he selects the other, previously unreinforced box. At this particular point, they claim that the law of effect is clearly inoperative.

Now, had the same investigators expanded their temporal scope, had they conducted a longitudinal study with the same subject, their view of what the reinforced response is might have changed. They might have observed that in his daily life the subject has learned that the supply of reinforcers from a specific source is, at times, finite. Accordingly, this subject understands that he may ultimately receive the greatest number of reinforcers when he adopts a behavioral strategy that will assure exposure to as many potential sources of reinforcement as possible. The probability that a strategy will be maintained increases when it proves to be a reinforcing strategy. Any instance in which a particular strategy is maintained because it is followed by reinforcement, albeit intermittently, is clearly a good example of the applicability of the law of effect.

The organism's assumptions to the effect that (*a*) once a reward is consumed or removed, given no other information, it may no longer be present on the next exposure; and (*b*) other organisms may be deceptive, could have emerged, and in all probability did emerge, because they were intermittently reinforced. Consequently, when an individual who was reinforced for one response then chooses not to repeat that response, one potential inference is that this behavior or response constitutes a part of a strategy that has been previously reinforced.

A response, viewed in isolation, apart from the role that it plays in a strategy, may be meaningless. Its definition must take into account this response's role, if any, in a more global strategy. Given that a response is an integral part of a particular strategy, then the relevant reinforcement should be viewed with respect to that particular strategy and not in relation to that specific response.

Viewing human and nonhuman behavior from a broad, evolutionary perspective, an observer may note that a flexible strategy, rather than a specific, highly predictable, and rigid response, may lead to more reinforcements. This flexible strategy may involve the continuous shifting and alternating of behavior. This continuous shift may be reinforced by the fact that the organism's opponents and/or predators may become confused because their target's impending behavior will remain in doubt, camouflaged by uncertainty.

REFERENCES

Ferster, C. B., & Skinner, B. F. *Schedules of reinforcement.* New York: Appleton-Century-Crofts, 1957.

Freedman, P. E., Cohen, M., & Hennessy, J. Learning theory: Two trials and tribulations. *American Psychologist,* 1974, 29, 204–206.

Jacob Beiser
State University of New York
Orange County Community College

Organization of Central Office

It was very informative to read in the June issue about the Central Office organization of the APA (Little, 1974). Most interesting of all was finding out who the people are who head the different offices, departments, and divisions.

I was most surprised to discover that of the 23 persons identified as heading a unit, only 4 are women and none occupies a position higher than head of department. This figure is only 16% of all positions identified (two males occupy two positions each). If the minority representation for graded staff has been increased from 15% in 1970 to 23% in 1973, why is there not an equivalent representation on the executive side?

Is the distribution of women in the governing body of the Central Office representative of the practices proposed in the Affirmative Action plan that was prepared in 1973? Is this the reason why the praise from the Board of Social and Ethical Responsibility for Psychology was not wholehearted but qualified with "faint damns"?

I hope that the first function of the Committee on Women in Psychology, following its stated purpose according to Rule of Council 60-13.1, will be to make strong recommendations to the Central Office regarding the composition of its governing body.

REFERENCE

Little, K. B. Report of the Executive Officer: 1973. *American Psychologist*, 1974, 29, 365–377.

Angela Ginorio
Fordham University

Notice to APA Authors

The American Psychological Association announces publication of the second edition of the *Publication Manual* in August 1974. This new edition, which supersedes the 1967 Manual, will be adopted by the 14 APA journals in 1975.

The new Manual is more comprehensive than the previous edition. It updates APA publication policies and procedures and incorporates changes in editorial practice since 1967. For instance, APA now sends many authors their copy-edited manuscripts for review before they are set into type, and some editors now use blind review procedures. The second edition also includes up-to-date statements of the coverage of each APA journal including the *Journal of Experimental Psychology* which will be published in four separate sections in 1975.

The new *Publication Manual* initiates several changes in APA style. These changes are announced in the August 1974 *American Psychologist* and will be introduced in the APA journals in January 1975. During the period of transition to the new style, authors should note that (*a*) all manuscripts published in 1974 will be copy-edited according to the 1967 Manual, (*b*) manuscripts accepted in 1974 and published in 1975 will be copy-edited to conform to the new Manual. Starting in 1975, accepted manuscripts that depart significantly from the Manual will be returned to authors for correction.

Authors will be encouraged by the changes in the second edition. The new APA style simplifies reference forms; eliminates unnecessary underlines, brackets, and other devices; supports appropriate use of "I" and "we"; and generally clarifies typing requirements. Material is arranged for maximum convenience to authors and typists, and all sections are cross-referenced and indexed.

The new *Publication Manual* is available after August 1 for $3. Send orders to APA Publication Sales, 1200 Seventeenth Street, N.W., Washington, D. C. 20036. Orders of $15 or less must include payment unless they are submitted on institutional purchase order forms.

Psychology in Action

USEFULNESS OF IN-SERVICE TRAINING WORKSHOPS FOR COUNSELORS

STEPHEN I. ABRAMOWITZ *Vanderbilt University*
LAWRENCE J. WEITZ *George Peabody College for Teachers*
CHRISTINE V. ABRAMOWITZ *George Peabody College for Teachers*
HOWARD BERTHOLD *Union College*
FRANK M. CALABRIA *Union College*
MIRIAM CONABLE *Union College*
ROBERT GIBLIN *State University of New York at Albany*
FRED F. SELFRIDGE *Valley Central Middle School* [1]

The desirability of continuing education and training for school counselors is well recognized (Ohlsen, 1969). According to the Commission on Guidance in American Schools (Wrenn, 1962), the school counselor should "consider professional updating a continuous process . . . and . . . attempt to understand himself better through counseling or other professional help [pp. 180–181]." The recurrent upheavals within the youth culture in the ensuing decade underscore the relevance of the Commission's charge.

Consistent with this view, O'Hara (1968) pointed out the responsibility of counseling educators to cooperate with school officials in providing postgraduate on-the-job monitoring of former students: "That counseling educators should know what their graduates are doing and provide them with opportunities for further professional growth is obvious [p. 213]." Gust (1969) believes that on-site supervision of the counselor's care-giving activities could ultimately bolster the profession's public image by enhancing the quality of services delivered.

However, in-service supervision or training is more frequently preached than practiced and rarely evaluated when it has been implemented. Controlled experimental investigations of its impact are essentially nonexistent, which is unfortunate from an applied as well as a scientific perspective. Financially beleaguered administrators can hardly be faulted for being reluctant to subsidize a new spending adventure whose effectiveness is substantiated only by polemic exhortation and anecdotal report. Legislators attuned to the pulse of a public disenchanted with university-connected innovation also are unlikely to be enthusiastic, especially when greater emphasis is being accorded experientially oriented and other novel teaching modalities (Association for Counselor Education and Supervision, 1964; Hill, 1968). Despite the fact that their popularity in some circles outpaces their scientific credibility (Lieberman, Yalom, & Miles, 1973), sensitivity, encounter, and other formerly avant-garde techniques of relationship-skill acquisition and personal growth now regularly complement more conventional, didactic instruction.

Developing Maslow's (1954) writings on the fully functioning person, Rogers (1961) championed the notion that a care-giver's personal qualities are at least as important as his accumulated conceptual and technical skills in determining counseling effectiveness. Others within the client-centered tradition have recently advanced a more extreme position, asserting that a counselor's professional success cannot transcend the quality of his everyday life (Carkhuff & Berenson, 1967). Polemics aside, do more self-actualized persons in the client-centered sense make better counselors? The meager evidence tends to be affirmative. Combs and Soper (1963) found that supervisor ratings of counseling ability were positively associated with the degree to which a counselor's written reports of personal incidents suggested that he possessed certain qualities characteristic of self-actualized persons. Foulds (1969) likewise obtained overall modest positive correlations between self-report measures of self-actualized functioning and judges' impressions of counseling-facilitation levels offered to clients.

The group setting's practicality has made it a popular vehicle for incorporating experiential modes of learning and personal development into training curricula, and positive sequelae to such groups have been reported in two studies (Betz, 1969; Gazda & Ohlsen, 1961).

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[1] This report is based on data collected as part of an in-service training program for secondary school counselors funded to the second author by the New York State Bureau of Guidance and sponsored by the Capital District Personnel and Guidance Association of Albany. First and second authorship was determined by a coin toss.

Requests for reprints should be sent to Stephen I. Abramowitz, Interuniversity Psychological Counseling Center, 300 Oxford House, 1313 21st Avenue South, Nashville, Tennessee 37212.

Extrapolation of the group experience concept to the in-service training of counselors has been urged by Truax and Mitchell (1971). The aim would be "to reinforce the habit of relating to clients with high levels of interpersonal skill and to discourage the counselor's tendency to fall back into his lifelong style of relating to others [p. 340]." Accordingly, the present investigation was undertaken to assess the personal and professional outcomes of such an in-service training workshop for secondary school counselors. Participants received intensive instruction in the core dimensions of facilitative interpersonal process (Carkhuff, 1969) and *either* an interpersonally oriented group experience in the encounter motif or one structured around deep relaxation and other intrapersonal concentration exercises. Hence, in addition to an overall evaluation of the program's effectiveness, determination of the differential merits of these two contrasting approaches to promoting personal development was possible.

Method

SUBJECTS AND PROCEDURE

Thirty-five secondary school counselors expressed a desire to enroll in an in-service training workshop sponsored by the Capital District Personnel and Guidance Association and conducted by Union College Counseling Center staff and consultants. The counselors represented 11 school systems and a broad spectrum of orientations. Age and experience ranges were from 21 to 53 years and from 1 to 30 years, respectively. From two to six pupil-clients of each prospective inductee also participated in the research.

Fortuitously from an experimental standpoint, about one third of the original training seekers had schedules that could not be accommodated within the workshop's time framework, providing a control group against which to compare the performance of persons exposed to either of the two experimental conditions. Those individuals able to attend the training sessions were assigned randomly to the intrapersonal self-control group or to the interpersonal encounter group.

Each counselor in the experimental treatments received up to ten 3-hour training sessions held over a 3-month period. Half of each meeting was devoted to intensive didactic-based learning in the core facilitative constructs of empathy, regard, genuineness, concreteness, and self-disclosure (Carkhuff, 1969). In addition, self-control trainees were exposed to 15 hours of deep relaxation experiences, whereas their interpersonal encounter counterparts were offered 15 hours of sensitivity experiences (Otto, 1970). The prescribed orientation for the former group was the development of the creative imagination for problem solving through the achievement of deeper levels of physical and mental relaxation. To illustrate, during one meeting members were given the following suggestion: "When you meet with your client and take several deep breaths, you will experience a deep sense of relaxation, physically and mentally. In this state of deep relaxation, you will be receptive to what your client is thinking and feeling. Each time you respond to your client, you will be functioning at very high levels of the facilitative dimensions."

By contrast, the goal of the sensitivity awareness group was to promote more authentic and meaningful interpersonal relationships within and outside the group via examination of one's interactions in both settings. As an exercise exemplar, members role-played various group and family interactions, alternately assuming the parts of placator, preacher, avoider, blamer, and real-self, and then explored their reactions (Satir, 1972).

Both treatment groups were directed by the same PhD psychologist, who was familiar and comfortable with both approaches. He and the two psychologists (one PhD, one MA) who orchestrated the Carkhuff-based didactic component of the workshop were each regarded as high therapy-facilitators in the client-centered sense and were accustomed to earning ratings of 4 on the standard 5-point continuum of empathic communication (Carkhuff, 1969).

Pretesting occurred during an orientation session in which the counselors were introduced to the training staff and materials. Posttraining data were gathered at the close of each group's final meeting. Counseling process data were collected anonymously by a neutral party from the workshop participants' clients before and after the counselors' immersion in the in-service training program.

INSTRUMENTS

Personal Orientation Inventory (POI). This measure was employed to assess the degree to which the counselors' personal qualities met the criteria for positive mental health articulated within self-actualization theory (Maslow, 1954; Shostrom, 1966). For each of the Inventory's 150 statement pairs, the respondent selects the more self-descriptive alternative. Although few data pertinent to the psychometric adequacy of the POI's component scales are yet available, the evidence suggests that they discriminate reasonably well between persons functioning at differential levels of experiential well-being (Shostrom, 1966).

Tape Excerpt Response Procedure (TERP). The counselors' ability to communicate empathically was gauged, in vitro, by an adaptation of the TERP (Selfridge et al., 1972), consisting of eight simulated interview scenes presented on videotape. This version of the TERP demonstrated construct validity in previous research with trainees (Abramowitz, Abramowitz, Weitz, & Hester, 1974). Each half-minute excerpt depicts an individual whose verbal or nonverbal behavior suggests psychological distress. Immediately after projection of each segment, the subjects responded in writing with their most helpful reaction to the hypothetical client.

Mixed together and group-unspecified, the pre- and post-protocols were rated by six trained judges along the standard 5-point scale of empathic understanding. A reasonable level of interrater correspondence was indicated by a Kendall coefficient of concordance (W) of .70 ($p < .01$) derived from the judges' evaluations of a selected response form.

Relationship Orientation Inventory (ROI). The ROI was completed by the participants' long-term counselees to provide an index of the counselors' relatability from the client's perspective. This well-known instrument is comprised of four 16-item completely counterbalanced subscales designed to tap the Rogerian dimensions of regard, empathy, unconditionality, and congruence (plus global relationship quality; Barrett-Lennard, 1962). In recent research utilizing the revision of the ROI used here, the estimated reliability (coefficient alpha) of each component scale exceeded .75 and the estimated reliability of total relationship scores was .92 (Abramowitz & Jackson, 1974). A counselor received the mean of the scores given by all of his originally designated clients, who also returned a postworkshop form.

Additional variables. Demographic and consumer satisfaction data were also gathered.

Results

SATISFACTION WITH TRAINING PROGRAM

Several questions written to tap the degree to which the workshop participants (excluding controls) considered their training experience to have been worthwhile formed part of the postbattery. To assure anonymity and thereby reduce the likelihood of artificially inflated evaluations, this section of the questionnaire was administered separately, uncoded by condition or name. Appraisals were uniformly enthusiastic, as indicated by the following representative mean ratings from a strongly disagree–strongly agree (1–5) Likert response continuum: I was able to gain information about myself, 4.6; I have deepened and broadened my personal horizons, 4.2; I have become more sensitized to problem areas within myself, 4.4; I feel encouraged to tackle problem areas within myself, 4.4; I was able to resolve conflict within myself, 3.8; My clients have benefited from my training, 4.1; and, Everything considered, I am satisfied with the results of my training experience, 4.5.

MAIN RESULTS: COMPARATIVE TRAINING OUTCOMES

The randomness of a counselor's assignment to an experimental condition was constrained by scheduling conflicts, the subsamples themselves were modest in size, and some attrition occurred. Hence, checks were made to insure unbiased distributions within the resulting subgroups. No significant between-condition differences were revealed by any of the one-way analyses of variance done on the change-criteria prescores, tending to discount subject artifacts as confounds. Data reflecting on the impact of the program on the participants' personality and ability to communicate facilitatively were submitted to t tests appropriate when both sets of measures are taken on the same subjects.

Trainee self-actualization. Descriptive data and analysis summaries by training condition are shown for each POI scale in Table 1. Inspection of the information exhibited suggests that both the self-control and encounter workshops tended to promote members' personal growth relative to that of individuals whose previous time commitments precluded their enrollment in the program. In general, however, the most impressive gains were registered in the deep relaxation group. Counselors exposed to the concentration exercises evinced improvement in time competence ($p < .05$) and inner directedness ($p < .05$), the POI's two main indexes, as well as in self-actualizing value ($p < .05$), acceptance of aggression ($p < .01$), and capacity for intimate contact ($p < .05$). On the other hand, sensitivity workshop participants' existentiality ($p < .05$), feeling reactivity ($p < .05$), and spontaneity ($p < .01$) were enhanced. The self-acceptance postscores of the controls were significantly higher than their prescores ($p < .05$); this was largely a function of within-group consistency in, rather than size of, gains on this measure.

Trainee facilitativeness. Descriptive data and analysis summaries by subgroup are presented for the TERP in Table 2 and for the ROI in Table 3. Data yielded by the simulated clinical interview situations indicate that counselors undergoing both training programs, but not the controls, responded more empathically to the

TABLE 1

POI Scores by Training Condition and Testing Occasion

Scale	Pretraining		Posttraining		t
	M	*SD*	*M*	*SD*	
Time competence					
Intrapersonal ($n = 7$)	17.0	2.8	19.7	1.6	2.11*
Interpersonal ($n = 9$)	18.4	2.6	19.0	2.4	.84
Control ($n = 8$)	19.3	2.7	19.5	2.1	.51
Inner directedness					
Intrapersonal	86.0	9.3	94.3	7.5	2.17*
Interpersonal	89.4	9.5	92.6	13.7	1.59
Control	92.5	3.5	94.9	7.0	.96
Self-actualizing value					
Intrapersonal	19.7	2.9	21.9	1.9	2.58*
Interpersonal	21.3	2.6	21.8	2.6	.79
Control	20.8	2.3	21.6	1.2	1.11
Existentiality					
Intrapersonal	22.3	2.9	23.3	2.4	.68
Interpersonal	22.0	3.9	24.1	5.2	2.56*
Control	21.1	3.0	22.8	3.2	1.29
Feeling reactivity					
Intrapersonal	15.6	2.1	16.7	2.6	1.55
Interpersonal	15.6	2.4	16.8	2.5	2.34*
Control	16.3	2.4	16.8	2.8	.74
Spontaneity					
Intrapersonal	13.3	2.3	15.0	2.2	1.82
Interpersonal	12.6	3.2	14.2	3.6	3.17**
Control	14.0	.9	15.3	2.8	1.28
Self-regard					
Intrapersonal	12.3	2.8	13.4	1.1	1.54
Interpersonal	12.6	2.4	12.3	3.2	−.61
Control	14.0	1.3	13.0	1.1	−1.87
Self-acceptance					
Intrapersonal	16.1	2.9	17.9	3.9	.91
Interpersonal	17.2	2.6	17.3	3.2	.15
Control	17.6	3.1	18.5	2.9	2.20*
Nature of man					
Intrapersonal	12.7	1.4	13.1	1.6	.63
Interpersonal	13.0	1.5	13.2	1.6	.55
Control	13.6	1.3	13.4	1.8	−.39
Synergy					
Intrapersonal	7.4	1.4	7.9	1.1	.81
Interpersonal	7.4	1.0	7.6	.9	.31
Control	6.8	1.3	7.4	.9	1.05
Acceptance of aggression					
Intrapersonal	15.4	2.6	18.7	1.7	4.82**
Interpersonal	16.7	2.4	18.1	3.8	1.72
Control	16.5	1.2	17.6	2.1	1.52
Capacity for intimate contact					
Intrapersonal	17.4	2.6	20.0	2.2	2.06*
Interpersonal	19.1	3.6	20.4	3.8	1.15
Control	19.3	2.1	20.3	2.4	1.67

Note. POI = Personal Orientation Inventory.
* $p < .05$, one-tailed.
** $p < .01$, one-tailed.

TABLE 2

TERP Empathy Scores by Training Condition and Testing Occasion

Condition	Pretraining		Posttraining		t
	M	*SD*	*M*	*SD*	
Intrapersonal ($n = 7$)	2.24	.6	2.99	.3	4.22[a*]
Interpersonal ($n = 8$)	2.20	.3	2.74	.4	11.68[a*]
Control ($n = 9$)	2.31	.3	2.37	.4	.52

Note. TERP = Tape Excerpt Response Procedure.
[a] Differential between-condition gain not significant.
* $p < .01$, one-tailed.

hypothetical help-seeker after the program's completion ($p < .01$, in both cases). However, from the clients' vantage point (Table 3), the deep relaxation techniques were generally more effective than interpersonal encounter experiences in honing counselors' relationship skills. Counselors instructed in self-control were perceived as having become more empathic ($p < .05$), more unconditional in their expressions of regard ($p < .05$), and more facilitative overall ($p < .05$). The small increment in unconditionality among the control subjects ($p < .05$) was the only other statistically reliable result from the ROI. In neither case in which significant change in the level of a therapist-offered condition was detected for more than one treatment group was the amount of change differential.

Discussion

Marked improvement in the ability to communicate empathically, in vitro, was forged in both workshops, although, according to the clients, radiation of this skill to actual counseling interviews was confined to relaxation-concentration group members. Participants also showed gains in various aspects of self-actualization, this progress again most striking among members of the self-control rather than the interpersonal-sensitivity group. That these person-outcomes are on occasion transmitted to work associates not exposed directly to training is suggested by the incidental increments in functioning posted by the controls. Longer-term follow-up unfortunately lies beyond the study's scope, leaving open the issue of the extent to which the new learning will, with time, succumb to the comfort of old habits. Nevertheless, the evaluation's ground-breaking implication is that substantial enthusiasm and discernible self-development and skill acquisition can be stimulated among practicing counselors by a relatively brief, inexpensive training module.

The slight but overall superior effectiveness of the self-control techniques is intriguing and warrants exploration. One distinction between the intrapersonal and the interpersonal workshops was the degree to which, in the former, particular reference was made to actual counseling situations, perhaps accounting for self-control participants' exclusively raised posttraining relationship ratings from clients. Related research has identified "job-related" facets of training as performance predictors (Paul, McInnis, & Mariotto, 1973). The relaxation-concentration exercises' generally greater potency could, however, also be attributed to their relative novelty or to an interaction of the hypnoticlike suggestions of the workshop leader (a charismatic individual) and counselors' trustfulness. Whether such a program emphasis would be as successful among PhD practitioners cast in the intellectual-skeptical mold is a question for future research to address.

Another interpretive task is to explore the possible reasons for the muted benefits accruing to counselors who underwent the interpersonal relations experience. Despite its avowed focus, this condition failed to inspire positive movement along the POI's two "interpersonal sensitivity" dimensions (acceptance of aggression and capacity for intimate contact; Shostrom, 1966), whereas the self-control condition did promote such growth. This opaque but sobering finding is consistent with other recent data hinting that the effects of sensitivity training on relations with others are less spectacular than sometimes claimed (Hurley & Force, 1973; Lieberman, Yalom, & Miles, 1973).[2]

Visual inspection of the interpersonal-encounter

TABLE 3

ROI Scores by Training Condition and Testing Occasion

Scale	Pretraining		Posttraining		t
	M	*SD*	*M*	*SD*	
Regard					
Intrapersonal ($n = 6$)	24.3	9.6	25.8	9.4	1.34
Interpersonal ($n = 8$)	20.1	8.0	21.4	12.4	.64
Control ($n = 8$)	24.5	6.6	24.8	7.3	.20
Empathy					
Intrapersonal	14.3	13.0	19.0	15.9	2.09*
Interpersonal	15.6	5.5	19.1	9.1	1.75
Control	18.4	6.7	17.4	7.2	−.48
Unconditionality					
Intrapersonal	2.7	7.8	8.5	11.0	2.39[a*]
Interpersonal	7.6	3.6	7.6	6.7	.00
Control	3.9	3.3	6.3	4.2	2.24[a*]
Congruence					
Intrapersonal	21.5	9.2	22.5	15.3	.39
Interpersonal	18.1	8.6	20.6	10.8	1.47
Control	22.3	8.4	23.4	4.4	.52
Total relationship					
Intrapersonal	62.8	38.2	75.8	50.0	2.25*
Interpersonal	61.5	22.6	68.8	32.6	1.22
Control	69.0	21.9	71.8	20.7	.54

Note. ROI = Relationship Orientation Inventory.
[a] Differential between-condition gain not significant.
* $p < .05$, one-tailed.

[2] However, in another investigation (Selfridge et al., 1974), an encounter group proved to be a relatively successful in-service training modality. Interestingly, in that study, the encounter experience provided a learning context more (rather than less) similar to the actual counseling situation than that offered by the alternative training condition (i.e., didactic).

group's descriptive data revealed several instances wherein increased postworkshop variability prevented the realization of statistical significance and masked otherwise sound gains (e.g., inner directedness on POI, total relationship on ROI). Subsequent analysis disclosed flat or mildly negative reactions among two persons who entered the training program conspicuously less self-actualized (on the POI) than their fellow group members. Previous evidence (Carkhuff, 1969) also intimates that less well functioning persons are the least responsive to didactic-experiential training. However, more fully functioning self-control workshop members did not demonstrate differentially favorable outcome relative to lower-POI starters.

No group of counselors underwent didactic-based training in facilitative interpersonal process *only, without an adjunctive experiential component.* This imposes a formidable constraint on our freedom to pinpoint sources of gain (i.e., didactic versus experiential), except in those comparatively few instances of differential intrapersonal-workshop versus interpersonal-workshop impact. Thus, a substantial part of the program's success could well have been due to the primarily didactic-plus-role-playing training in the core conditions or to elements common to both experiential encounters (e.g., leader personality, group cohesion, etc.). The noteworthy increase in the ability of all program participants to respond empathically to contrived help-seekers (on the TERP) playing roles similar to those enacted as part of the training in facilitative offering once again seems to highlight the importance of congruity between job and training situation in promoting transfer of training gains (cf. Paul et al., 1973).

The absence of independent observations as checks to ascertain that the leader of the experiential modules did, in fact, structure them as prescribed (cf. Roback, 1974), the notorious unreliability of process notes (which were available), and the possibility of condition-differential facilitator expectations of success (cf. Goldstein, 1962) combine to discourage conjecture at finer levels of analysis. Attention to such lacunae would seem a reasonable expectation of future evaluators concerned with whether educators, administrators, and on-line service deliverers can collaborate in offering, at moderate cost, a stimulating training experience whose sequelae are detectable by standard assessment devices.

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Announcements

APA Representation

Gerald P. Ginsburg was the official APA representative to the centennial convocation and the inauguration of Max Milam as President of the University of Nevada, Reno, on October 12, 1974.

Donald H. Kausler was the official APA representative at the inauguration of Daniel Craig O'Connell as the President of St. Louis University on October 25, 1974.

Correction

Bruce E. Sandler, whose article appeared in the October issue on pages 767–773, is now at InterStudy, 123 East Grant Street, Minneapolis, Minnesota 55403. Send requests for reprints to him at this address.

Deaths

Arsen Aylaian, November 28, 1973
Ralph F. Berdie, August 21, 1974
Barry W. Fagin, July 5, 1974
Louis Fliegler, July 16, 1974
Robert P. Friedman, July 31, 1974
Gordon Hendrickson, May 17, 1974
William H. Hooper, April 30, 1974
Paul E. Johnson, September 2, 1974
Isabelle Kendig, August 29, 1974
Robert H. Knapp, September 8, 1974
Regis Leonard, date unknown
Sheldon P. Peizer, date unknown
M. Gertrude Reiman, July 31, 1974
Rene A. Spitz, September 14, 1974
Edward M. Westburgh, August 9, 1974
Bob J. Williams, August 4, 1974
John P. Zubek, August 20, 1974

Fellowships and Postdoctoral Programs

American Association for the Advancement of Science: Announces a program supporting up to 10 outstanding advanced graduate students in the social and natural sciences as intern reporters, researchers, or production assistants in a variety of media for the summer of 1975. Salaries will be paid by AAAS at a level based on the current rate for interns in the participating media. Travel expenses are paid also. *Deadline for receipt of completed applications is February 1, 1975.* For information, contact Wendy Weisman-Dermer, Communications Office, AAAS, 1776 Massachusetts Avenue, N.W., Washington, D.C. 20036.

Cleveland Metropolitan General Hospital, Departments of Psychiatry and Pediatrics: Now accepting applications for predoctoral internships in clinical psychology (combined adult and child program) for 1975–1976. Stipend $7,000 plus $600 per dependent up to two dependents. *Apply before January 10, 1975*, to John Shen, Department of Psychiatry, Cleveland Metropolitan General Hospital, 3395 Scranton Road, Cleveland, Ohio 44109.

Cornell University, New York State College of Human Ecology: Now accepting applications for graduate assistantships in applied areas of the social and behavioral sciences. For information, write to Director of Graduate Studies, N116 MVR, New York State College of Human Ecology, Cornell University, Ithaca, New York 14853.

Department of Psychiatry, Mount Zion Hospital and Medical Center, San Francisco: Three intensive advanced postdoctoral programs in psychology: (*a*) child or adult community mental health, (*b*) psychotherapy research, (*c*) general clinical. For further information, write to Nanette Heiman, Chief Psychologist, Department of Psychiatry, Mount Zion Hospital and Medical Center, 1600 Divisadero Street, San Francisco, California 94115.

Department of Psychiatry, Mount Zion Hospital and Medical Center, San Francisco: Now accepting applications for 1975–1976 one-year predoctoral fellowships in clinical psychology and in clinical child psychology. *Deadline for applications is January 1, 1975.* For further information, write to Donald P. Cliggett, Mount Zion Hospital and Medical Center, 1600 Divisadero Street, San Francisco, California 94115.

Division of Clinical Psychology, Upstate Medical Center, State University of New York at Syracuse: Now accepting applications for APA-approved internships in clinical psychology for 1975–1976: both clinical-adult and clinical-child predoctoral internships. Stipends $6,000. For more information, write to Sidney A. Orgel, Division of Clinical Psychology, Upstate Medical Center, 750 East Adams Street, Syracuse, New York 13210.

Section of Clinical Psychology, University of Manitoba School of Medicine: Now accepting applications for predoctoral internships in clinical psychology for 1975–1976. Stipend $8,000. For further information, write to Robert H. Martin, or Robert de von Flindt, Section of Clinical Psychology, Faculty of Medicine, University of Manitoba, 770 Bannatyne Avenue, Winnipeg, Manitoba, R3E OW3.

National Conventions

American Psychological Association: August 30–September 3, 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco; 1978, Toronto; 1979, Atlanta

For information write to:

Doris Entwisle
c/o Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Southeastern Psychological Association: March 26–29, 1975, Atlanta; March 17–20, 1976, New Orleans; May 4–7, 1977, Hollywood, Florida

For information write to:

Edward H. Loveland
School of Psychology
Georgia Institute of Technology
Atlanta, Georgia 30332

Eastern Psychological Association: April 3–5, 1975, New York City; April 22–24, 1976, New York City

For information write to:

Murray Benimoff
Department of Psychology
Glassboro State College
Glassboro, New Jersey 08028

Southwestern Psychological Association: April 17–19, 1975; Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Western Psychological Association: April 24–26, 1975; Sacramento, California

For information write to:

George Parrott
Department of Psychology
California State University, Sacramento
6000 J Street
Sacramento, California 95819

Midwestern Psychological Association: May 1–3, 1975; Chicago, Illinois

For information write to:

Rudolph W. Schulz
Department of Psychology
University of Iowa
Iowa City, Iowa 52242

Rocky Mountain Psychological Association: May 7–10, 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque, New Mexico

For information write to:

William Prokasy
205 Spencer Hall
University of Utah
Salt Lake City, Utah 84112

Southern Society for Philosophy and Psychology: March 27–29, 1975, New Orleans; 1976, Atlanta; 1977, Nashville, Tennessee

For information write to:

Michel Loeb
Department of Psychology
University of Louisville
Louisville, Kentucky 40208

Conference: Rape—Action, Prevention, Research: January 20–22, 1975; Tuscaloosa, Alabama

For information write to:

Marcia Walker
Rape Research Group
Center for Correctional Psychology
P.O. Box 2968
University, Alabama 35486

New York Academy of Sciences: "Developmental Psycholinguistics and Communication Disorders": January 24–25, 1975; New York City

For information write to:

Conference Department
New York Academy of Sciences
2 East 63rd Street
New York, New York 10021

Biofeedback Research Society: January 31–February 5, 1975; Monterey, California

For information write to:

Francine Butler
Department of Psychiatry #202
University of Colorado Medical Center
4200 East Ninth Avenue
Denver, Colorado 80220

Conference on Psychotherapy in the Arts: February 15–17, 1975; New York City

For information write to:

Michael Schulman
Lee Strasberg Theatre Institute
34 West 13 Street
New York, New York 10011

Conference about the Teaching of Group: Theory and Practice: February 20–22, 1975; New Brunswick, New Jersey

For information write to:

Kenneth G. Roy
Group Studies Consortium
c/o Counseling Center
50 College Avenue
New Brunswick, New Jersey 08903

American Psychopathological Association: March 6–7, 1975; New York City

For information write to:

Edward Sachar
Bronx Municipal Hospital Center
Eastchester Road and Pelham Parkway
Bronx, New York 10461

Society for Research in Child Development: April 10–13, 1975; Denver, Colorado

For information write to:

Anne D. Pick
Institute of Child Development
University of Minnesota
Minneapolis, Minnesota 55455

Christian Association for Psychological Studies: April 13–15, 1975; Oklahoma City, Oklahoma

For information write to:

William L. Hiemstra
6850 South Division Avenue
Grand Rapids, Michigan 49508

National Center on Black Aged: "Aging: Black Women and Federal Policies: 1950–2080": April 13–15, 1975; Washington, D.C.

For information write to:

Jacquelyne J. Jackson
Box 3003
Duke University Medical Center
Durham, North Carolina 27710

New England Educational Research Organization: April 30–May 3, 1975; Provincetown, Maine

For information write to:

Marvin D. Lynch
College of Education
Northeastern University
102 The Fenway
Boston, Massachusetts 02115

International Conventions

Fifth Symposium on Behavior Modification: December 9–12, 1974; Caracas, Venezuela

For information write to:

Edmundo Chirinos
Sociedad Venezolana de Psicologia Clinica
Apartado 70.006
Caracas, Venezuela

Fifteenth International Congress of Psychology: December 14–19, 1974; Bogotá, Colombia

For information write to:

Luiz F. S. Natalicio
Secretary General
Interamerican Society of Psychology
P.O. Box 88 UTEP
El Paso, Texas 79968

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29–January 1, 1975; Nairobi, Kenya

For information write to:

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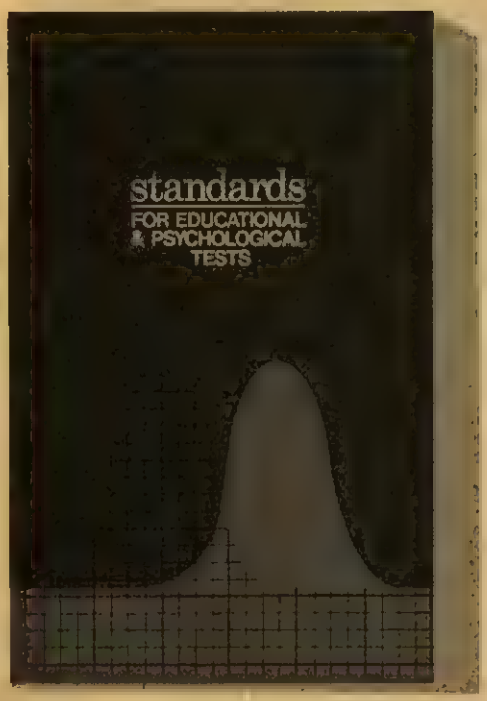

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Behavior Theory and the Models of Man

ALBERT BANDURA *Stanford University*[1]

The views about the nature of man conveyed by behavior theory require critical examination on conceptual and social grounds. What we believe man to be affects which aspects of human functioning we study most thoroughly and which we disregard. Premises thus delimit research and are, in turn, shaped by it. As knowledge gained through study is put into practice, the images of man on which social technologies rest have even vaster implications. This is nowhere better illustrated than in growing public concern over manipulation and control by psychological methods. Some of these fears arise from expectations that improved means of influence will inevitably be misused. Other apprehensions are aroused by exaggerated claims of psychological power couched in the language of manipulation and authoritarian control. But most fears stem from views of behaviorism, articulated by popular writers and by theorists themselves, that are disputed by the empirical facts of human behavior.

In the minds of the general public, and of many within our own discipline, behavior theory is equated with "conditioning." Over the years, the terms *behaviorism* and *conditioning* have come to be associated with odious imagery, including salivating dogs, puppetry, and animalistic manipulation. As a result, those who wish to disparage ideas or practices they hold in disfavor need only to label them as behavioristic or as Pavlovian precursors of a totalitarian state.

Contrary to popular belief, the fabled reflexive conditioning in humans is largely a myth. *Conditioning* is simply a descriptive term for learning through paired experiences, not an explanation of how the changes come about. Originally, conditioning was assumed to occur automatically. On closer examination it turned out to be cognitively mediated. People do not learn despite repetitive paired experiences unless they recognize that events are correlated (Dawson & Furedy, 1974; Grings, 1973). So-called conditioned reactions are largely self-activated on the basis of learned expectations rather than automatically evoked. The critical factor, therefore, is not that events occur together in time, but that people learn to predict them and to summon up appropriate anticipatory reactions.

The capacity to learn from correlated experiences reflects sensitivity, but because Pavlov first demonstrated the phenomenon with a dog, it has come to be regarded as a base animalistic process. Had he chosen to study physiological hyperactivity in humans to cues associated with stress, or the development of empathetic reactions to expressions of suffering, conditioning would have been treated in a more enlightened way. To expect people to remain unaffected by events that are frightening, humiliating, disgusting, sad, or pleasurable is to require that they be less than human. Although negative effects such as fears and dislikes can arise from paired experiences of a direct or vicarious sort, so do some of the enobling qualities of man. The pejorative accounts of learning principles, which appear with regularity in professional and lay publications, degrade both the science of psychology and the audiences that the offensive rhetoric is designed to sway.

It is well documented that behavior is influenced by its consequences much of the time. The image of man that this principle connotes depends on the types of consequences that are acknowledged and on an understanding of how they operate. In theories that recognize only the role of proximate external consequences and contend they shape behavior automatically, people appear as mechanical pawns of environmental forces. But external consequences, influential as they often are, are not the sole determinants of human behavior, nor do they operate automatically.

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[1] Presidential Address presented at the meeting of the American Psychological Association, New Orleans, August 1974.

Requests for reprints should be sent to Albert Bandura, Department of Psychology, Stanford University, Stanford, California 94305.

Response consequences serve several functions. First, they impart information. By observing the effects of their actions individuals eventually discern which behaviors are appropriate in which settings. The acquired information then serves as a guide for action. Contrary to the mechanistic metaphors, outcomes change behavior in humans through the intervening influence of thought.

Consequences motivate, through their incentive value, as well as inform. By representing foreseeable outcomes symbolically, future consequences can be converted into current motivators of behavior. Many of the things we do are designed to gain anticipated benefits and to avert future trouble. Our choices of action are largely under anticipatory control. The widely accepted dictum that man is ruled by response consequences thus fares better for anticipated than for actual consequences. Consider behavior on a fixed-ratio schedule (say, 50:1) in which only every fiftieth response is reinforced. Since 96% of the outcomes are extinctive and only 4% are reinforcing, behavior is maintained despite its dissuading consequences. As people are exposed to variations in frequency and predictability of reinforcement, they behave on the basis of the outcomes they expect to prevail on future occasions. When belief differs from actuality, which is not uncommon, behavior is weakly controlled by its actual consequences until repeated experience instills realistic expectations (Bandura, 1971b; Kaufman, Baron, & Kopp, 1966).

Had humans been ruled solely by instant consequences, they would have long become museum pieces among the extinct species. Not that our future is unquestionably secure. The immediate rewards of consumptive life-styles vigorously promoted for short-term profit jeopardize man's long-term chances of survival. But immediate consequences, unless unusually powerful, do not necessarily outweigh deferred ones (Mischel, 1974). Our descendants shall continue to have a future only because those who foresee the aversive long-term consequences of current practices mobilize public support for contingencies that favor survival behavior. Hazardous pesticides, for example, are usually banned before populations suffer maladies from toxic residues. The information-processing capacities with which humans are endowed provide the basis for insightful behavior. Their capacity to bring remote consequences to bear on current behavior by anticipatory thought supports foresightful action.

Explanations of reinforcement originally assumed that consequences increase behavior without conscious involvement. The still prevalent notion that reinforcers can operate insidiously arouses fears that improved techniques of reinforcement will enable authorities to manipulate people without their knowledge or consent. Although the empirical issue is not yet completely resolved, there is little evidence that rewards function as automatic strengtheners of human conduct. Behavior is not much affected by its consequences without awareness of what is being reinforced (Bandura, 1969; Dulany, 1968). After individuals discern the instrumental relation between action and outcome, contingent rewards may produce accommodating or oppositional behavior depending on how they value the incentives, the influencers and the behavior itself, and how others respond. Thus reinforcement, as it has become better understood, has changed from a mechanical strengthener of conduct to an informative and motivating influence.

People do not function in isolation. As social beings, they observe the conduct of others and the occasions on which it is rewarded, disregarded, or punished. They can therefore profit from observed consequences as well as from their own direct experiences (Bandura, 1971c). Acknowledgment of vicarious reinforcement introduces another human dimension—namely, evaluative capacities—into the operation of reinforcement influences. People weigh consequences to themselves against those accruing to others for similar behavior. The same outcome can thus become a reward or a punishment depending upon the referents used for social comparison.

Human conduct is better explained by the relational influence of observed and direct consequences than by either factor alone. However, behavior is not fully predictable from a relational coefficient because social justifications alter the impact of outcome disparities. Inequitable reinforcement is willingly accepted when people are graded by custom into social ranks and rewarded according to position rather than by performance. Arbitrary inequities are also likely to be tolerated if the underrewarded are led to believe they possess attributes that make them less deserving of equal treatment. Persuasively justified inequities have more detrimental personal effects than acknowledged unfairness because they foster self-de-

valuation in the maltreated. Negative reactions to inequitable reinforcement, which is acknowledged to be unwarranted, can likewise be diminished by temporizing. If people are led to expect that unfair treatment will be corrected within the foreseeable future, it becomes less aversive to them.

Theories that explain human behavior as the product of external rewards and punishments present a truncated image of man because people partly regulate their actions by self-produced consequences (Bandura, 1971c; Thoresen & Mahoney, 1973). Example and precept impart standards of conduct that serve as the basis for self-reinforcing reactions. The development of self-reactive functions gives humans a capacity for self-direction. They do things that give rise to self-satisfaction and self-worth, and they refrain from behaving in ways that evoke self-punishment.

After self-reinforcing functions are acquired, a given act produces two sets of consequences: self-evaluative reactions and external outcomes. Personal and external sources of reinforcement may operate as supplementary or as opposing influences on behavior. Thus, for example, individuals commonly experience conflicts when rewarded for conduct they personally devalue. When self-condemning consequences outweigh rewarding inducements, external influences are relatively ineffective. On the other hand, if certain courses of action produce stronger rewards than self-censure, the result is cheerless compliance. Losses in self-respect for devalued conduct can be abated, however, by self-exonerating justifications. I shall return to this issue shortly.

Another type of conflict between external and self-produced consequences arises when individuals are punished for behavior they regard highly. Principled dissenters and nonconformists often find themselves in this predicament. Personally valued conduct is expressed provided its costs are not too high. Should the threatened consequences be severe, one inhibits self-praiseworthy acts under high risk of penalty but readily performs them when the chances of punishment are reduced. There are individuals, however, whose sense of self-worth is so strongly invested in certain convictions that they will submit to prolonged maltreatment rather than accede to what they regard as unjust or immoral.

External consequences exert greatest influence on behavior when they are compatible with those that are self-produced. These conditions obtain when rewardable acts are a source of self-pride and punishable ones are self-censured. To enhance compatibility between personal and social influences, people select associates who share similar standards of conduct and thus ensure social support for their own system of self-reinforcement.

Individualistic theories of moral action assume that internalization of behavioral standards creates a permanent control mechanism within the person. Restraints of conscience thereafter operate as enduring controls over reprehensible conduct. The testimony of human behavior, however, contradicts this view. Much human maltreatment and suffering are, in fact, inflicted by otherwise decent moral people. And some of the most striking changes in moral conduct, as evidenced, for example, in political and military violence, are achieved without altering personality structures or moral standards. Personal control is clearly more complex and flexible than the theorizing implies.

Although self-reinforcing influences serve as regulators of conduct, they can be dissociated from censurable deeds by self-exonerating practices (Bandura, 1973). One device is to make inhumane behavior personally and socially acceptable by defining it in terms of high moral principle. People do not act in ways they ordinarily consider evil or destructive until such activities are construed as serving moral purposes. Over the years, much cruelty has been perpetrated in the name of religious principles, righteous ideologies, and regulatory sanctions. In the transactions of everyday life, euphemistic labeling serves as a handy linguistic device for masking reprehensible activities or according them a respectable status. Self-deplored conduct can also be made benign by contrasting it with more flagrant inhumanities. Moral justifications and palliative comparisons are especially effective because they not only eliminate self-generated deterrents but engage self-reward in the service of reprehensible conduct. What was morally unacceptable becomes a source of self-pride.

A common dissociative practice is to obscure or distort the relationship between one's actions and the effects they cause. People will perform behavior they normally repudiate if a legitimate authority sanctions it and acknowledges responsibility for its consequences. By displacing responsibility elsewhere, participants do not hold themselves accountable for what they do and are thus spared self-prohibiting reactions. Exemption

from self-censure can be facilitated additionally by diffusing responsibility for culpable behavior. Through division of labor, division of decision making, and collective action, people can contribute to detrimental practices without feeling personal responsibility or self-disapproval.

Attribution of blame to the victim is still another exonerative expedient. Victims are faulted for bringing maltreatment on themselves, or extraordinary circumstances are invoked as justifications for questionable conduct. One need not engage in self-reproof for committing acts prescribed by circumstances. A further means of weakening self-punishment is to dehumanize the victim. Inflicting harm upon people who are regarded as subhuman or debased is less likely to arouse self-reproof than if they are looked upon as human beings with sensitivities.

There are other self-disinhibiting maneuvers that operate by misrepresenting the consequences of actions. As long as detrimental effects are ignored or minimized, there is little reason for self-censure. If consequences are not easily distortable, distress over conduct that conflicts with self-evaluative standards can be reduced by selectively remembering the benefits and forgetting the harm of one's acts.

Given the variety of self-disinhibiting devices, a society cannot rely on control by conscience to ensure moral and ethical conduct. Though personal control ordinarily serves as a self-directive force, it can be nullified by social sanctions conducive to destructiveness. Indoctrination and social justifications give meaning to events and create anticipations that determine one's actions. Control through information, which is rooted in cognitive processes, is more pervasive and powerful than conditioning through contiguity of events. Cultivation of humaneness therefore requires, in addition to benevolent personal codes, safeguards built into social systems that counteract detrimental sanctioning practices and uphold compassionate behavior.

A conceptual orientation not only prescribes what facets of man will be studied in depth but also how one goes about changing human behavior. Early applications of reinforcement principles, for example, were guided by the then prevalent belief that consequences alter behavior automatically and unconsciously. Since the process supposedly operated mechanically, the reinforcers had to occur instantly to be effective. Participants in change programs were, therefore, uninformed about why they were being reinforced, and, in an effort to ensure immediacy of effects, reinforcers were presented intrusively as soon as the requisite responses were emitted. The net effect was a tedious shaping process that produced, at best, mediocre results in an ethically questionable manner. In many public and professional circles, reinforcement still connotes furtive control even though reinforcement theory and practices have progressed well beyond this level.

Realization that reinforcement is an unarticulated way of designating appropriate conduct prompted the use of cognitive factors in the modification of behavior. Not surprisingly, people change more rapidly if told what behaviors are rewardable and punishable than if they have to discover it from observing the consequences of their actions. Competencies that are not already within their repertoires can be developed with greater ease through the aid of instruction and modeling than by relying solely on the successes and failures of unguided performance.

As further research revealed that reinforcers function as motivators, consequences were recognized as sources of motivation that depend heavily for their effectiveness upon the incentive preferences of those undergoing change. Hence, people do not indiscriminately absorb the influences that impinge upon them. Outcomes resulting from actions need not necessarily occur instantly. Humans can cognitively bridge delays between behavior and subsequent reinforcers without impairing the efficacy of incentive operations.

At this second evolutionary stage, reinforcement practices changed from unilateral control to social contracting. Positive arrangements affirm that if individuals do certain things they are entitled to certain rewards and privileges. In the case of negative sanctions, reprehensible conduct carries punishment costs. The process is portrayed in reinforcement terms, but the practice is that of social exchange. Most social interactions are, of course, governed by conditional agreements, though they usually are not couched in the language of reinforcement. Describing them differently does not change their nature, however.

Contingencies vary in the human qualities they embody and in the voice individuals have in decisions concerning the social arrangements that affect their lives. Reflecting the salient values of our society, reinforcement practices have traditionally favored utilitarian forms of behavior. But

conditions are changing. With growing reservations about materialistic life-styles, reinforcement practices are being increasingly used to cultivate personal potentialities and humanistic qualities. These emerging changes in value commitments will probably accelerate as people devote fewer hours to working for income and have more leisure time for self-development.

Another change of some consequence is the renewed concern for individual rights. People are seeking a collaborative role in the development of societal contingencies that affect the course and quality of their lives. As part of this social trend, even the actions taken in the name of psychotherapy are being examined for their ethics and social purposes. These concerns have provided the impetus for prescripts to ensure that reinforcement techniques are used in the service of human betterment rather than as instruments of social control.

A closely related issue is the relative attention devoted to changing individuals or to altering the institutions of society to enrich life. If psychologists are to have a significant impact on common problems of life, they must apply their corrective measures to detrimental societal practices rather than limit themselves to treating the casualties of these practices. This, of course, is easier said than done. Practitioners, whatever their specialty, are reinforced more powerfully for using their knowledge and skills in the service of existing operations than for changing them. Socially oriented efforts are hard to sustain under inadequate reinforcement supports.

The methods of change discussed thus far draw heavily upon external consequences of action. Evidence that people can exercise some control over their own behavior provided the impetus for further changes in reinforcement practices. Interest began to shift from managing conduct to developing skills in self-regulation. In the latter approach, control is vested to a large extent in the hands of individuals themselves: They arrange the environmental inducements for desired behavior; they evaluate their own performances; and they serve as their own reinforcing agents (Goldfried & Merbaum, 1973; Mahoney & Thoresen, 1974). To be sure, the self-reinforcing functions are created and occasionally supported by external influences. Having external origins, however, does not refute the fact that, once established, self-influence partly determines what actions one performs. Citing historical determinants of a generalizable function cannot substitute for contemporaneous influences arising through exercise of that function.

The recognition of self-directing capacities represents a substantial departure from exclusive reliance upon environmental control. But the emerging self-influence practices are still closely rooted in physical transactions—the self-administered consequences are, for the most part, material. Eventually changes in form, as well as source, of reinforcement will appear as the insufficiency of material outcomes is acknowledged. Most people value their self-respect above commodities. They rely extensively on their own self-demands and self-approval as guides for conduct. To ignore the influential role of covert self-reinforcement in the regulation of behavior is to disavow a uniquely human capacity of man.

Proponents who recognize only external consequences restrict their research and practice to such influences and thus generate evidence that reinforces their conceptions. Those who acknowledge personal influences as well tend to select methods that reveal and promote self-directing capabilities in man. The view of man embodied in behavioral technologies is therefore more than a philosophical issue. It affects which human potentialities will be cultivated and which will be underdeveloped.

The preceding remarks addressed the need to broaden the scope of research into the reinforcement processes regulating human behavior. Much the same might be said for the ways in which human learning is conceptualized and investigated. Our theories have been incredibly slow in acknowledging that man can learn by observation as well as by direct experience. This is another example of how steadfast adherence to orthodox paradigms makes it difficult to transcend the confines of conceptual commitment. Having renounced cognitive determinants, early proponents of behaviorism advanced the doctrine that learning can occur only by performing responses and experiencing their effects. This legacy is still very much with us. The rudimentary form of learning based on direct experience has been exhaustively studied, whereas the more pervasive and powerful mode of learning by observation is largely ignored. A shift of emphasis is needed.

The capacity to represent modeled activities symbolically enables man to acquire new patterns of behavior observationally without reinforced enactment. From observing others, one forms an idea of how certain behavior is performed, and on

later occasions the coded information serves as a guide for action. Indeed, research conducted within the framework of social learning theory shows that virtually all learning phenomena resulting from direct experience can occur on a vicarious basis by observing other people's behavior and its consequences for them (Bandura, 1969). The abbreviation of the acquisition process through observational learning is, of course, vital for both development and survival. Modeling reduces the burden of time-consuming performance of inappropriate responses. Since errors can produce costly, if not fatal, consequences, the prospects of survival would be slim indeed if people had to rely solely on the effects of their actions to inform them about what to do.

In many instances the behavior being modeled must be learned in essentially the same form. Driving automobiles, skiing, and performing surgery, for example, permit little, if any, departure from essential practices. In addition to transmitting particular response patterns, however, modeling influences can create generative and innovative behavior. In the latter process, observers abstract common features from seemingly diverse responses and formulate generative rules of behavior that enable them to go beyond what they have seen or heard. By synthesizing features of different models into new amalgams, observers can achieve through modeling novel styles of thought and conduct. Once initiated, experiences with the new forms create further evolutionary changes. A partial departure from tradition eventually becomes a new direction.

Some of the limitations commonly ascribed to behavior theory are based on the mistaken belief that modeling can produce at best mimicry of specific acts. This view is disputed by growing evidence that abstract modeling is a highly effective means of inducing rule-governed cognitive behavior (Bandura, 1971a; Zimmerman & Rosenthal, 1974). On the basis of observationally derived rules, people alter their judgmental orientations, conceptual schemes, linguistic styles, information-processing strategies, as well as other forms of cognitive functioning. Nevertheless, faulty evaluations continue to be mistaken for weaknesses inherent in theory.

Observational learning has recently come to be accepted more widely, but some theorists are willing to grant it full scientific respectability only if it is reduced to performance terms. As a result, enactment paradigms are used which are rooted in the traditional assumption that responses must be performed before they can be learned. Instant reproduction of modeled responses is favored, thereby minimizing dependence upon cognitive functions which play an especially influential role when retention over time is required. The issue of whether reinforcement enhances modeling is pursued to the neglect of the more interesting question of whether one can keep people from learning what they have seen.

When learning is investigated through observational paradigms, a broader range of determinants and intervening mechanisms gains prominence. Learning by observation is governed by four component processes: (*a*) attentional functions regulate sensory input and perception of modeled actions; (*b*) through coding and symbolic rehearsal, transitory experiences are transformed for memory representation into enduring performance guides; (*c*) motor reproduction processes govern the integration of constituent acts into new response patterns; and (*d*) incentive or motivational processes determine whether observationally acquired responses will be performed. Studied from this perspective, observational learning emerges as an actively judgmental and constructive, rather than a mechanical copying, process.

Because observational learning entails several subfunctions that evolve with maturation and experience, it obviously depends upon prior development. Differences in theoretical perspectives prescribe different methodologies for studying how the capacity for observational learning itself is acquired. When modeling is conceptualized in terms of formation of stimulus–response linkages, efforts are aimed at increasing the probability of imitative responses through reinforcement. Modeling can be increased by rewarding matching behavior, but such demonstrations are not of much help in identifying what exactly is being acquired during the process, or in explaining imitation failures under favorable conditions of reinforcement. From a social learning view, the capability for observational learning is developed by acquiring skill in discriminative observation, in memory encoding, in coordinating ideomotor and sensorimotor systems, and in judging probable consequences for matching behavior. Understanding how people learn to imitate becomes a matter of understanding how the requisite subfunctions develop and operate. Capacity for observational learning is restricted by

deficits, and expanded by improvements, in its component functions.

Over the years, proponents of the more radical forms of behaviorism not only disclaimed interest in mentation but also marshaled numerous reasons why cognitive events are inadmissible in causal analyses. It was, and still is, argued that cognitions are inaccessible except through untrustworthy self-reports, they are inferences from effects, they are epiphenomenal, or they are simply fictional. Advances in experimental analysis of behavior, it was claimed, would eventually show them to be unnecessary. Empirical evidence, however, has shown the opposite to be true. A large body of research now exists in which cognition is activated instructionally with impressive results. People learn and retain much better by using cognitive aids that they generate than by repetitive reinforced performance (Anderson & Bower, 1973; Bandura, 1971a). With growing evidence that cognition has causal influence in behavior, the arguments against cognitive determinants are losing their force.

These recent developments have shifted emphasis from the study of response learning to analyses of memory and cognition. From this effort we have gained a better understanding of the mechanisms whereby information is acquired, stored, and retrieved. There is more to learning, however, than the acquisition and retention of information. Behavioristic theories addressed themselves to performance but deemphasized internal determinants, whereas the cognitive approaches remain immersed in thought but divorced from conduct. In a complete account of human behavior, internal processes must eventually be tied to action. Hence, explanations of how information eventuates in skilled performance must additionally be concerned with the organization and regulation of behavior. Social learning includes within its framework both the processes internal to the organism as well as performance-related determinants.

Speculations about man's nature inevitably raise the fundamental issues of determinism and human freedom. In examining these questions it is essential to distinguish between the metaphysical and the social aspects of freedom. Many of the heated disputes on this topic arise as much, if not more, from confusion over the dimensions of freedom being discussed as from disagreements over the doctrine of determinism.

Let us first consider freedom in the social sense. Whether freedom is an illusion, as some writers maintain, or a social reality of considerable importance depends upon the meaning given to it. Within the social learning framework, freedom is defined in terms of the number of options available to people and the right to exercise them. The more behavioral alternatives and social prerogatives people have, the greater is their freedom of action.

Personal freedom can be limited in many different ways. Behavioral deficits restrict possible choices and otherwise curtail opportunities to realize one's preferences. Freedom can therefore be expanded by cultivating competencies. Self-restraints arising from unwarranted fears and stringent self-censure restrict the effective range of activities that individuals can engage in or even contemplate. Here freedom is restored by eliminating dysfunctional self-restraints.

In maximizing freedom a society must place some limits on conduct because complete license for any individual is likely to encroach on the freedom of others. Societal prohibitions against behavior that is socially injurious create additional curbs on conduct. Conflicts often arise over behavioral restrictions when many members of society question conventional customs and when legal sanctions are used more to enforce a particular brand of morality than to prohibit socially detrimental conduct.

The issue of whether individuals should be allowed to engage in activities that are self-injurious but not detrimental to society has been debated vigorously over the years. Prohibitionists argue that it is difficult for a person, other than a recluse, to impair himself without inflicting secondary harm on others. Should self-injury produce incapacities, society usually ends up bearing the treatment and subsistence costs. Libertarians do not find such arguments sufficiently convincing to justify a specific prohibition because some of the self-injurious activities that society approves may be as bad or worse than those it outlaws. Normative changes over time regarding private conduct tend to favor an individualistic ethic. Consequently, many activities that were formerly prohibited by law have been exempted from legal sanctions.

Some groups have their freedom curtailed by socially condoned discrimination. Here, the alternatives available to a person are limited by skin color, sex, religion, ethnic background, or social class, regardless of capabilities. When self-determination is prejudicially restricted, those who are subordinated remove inequities by altering

practices that compromise or temporize the professed values of society.

Freedom deals with rights as well as options and behavioral restraints. Man's struggle for freedom is principally aimed at structuring societal contingencies so that certain forms of behavior are exempted from aversive control. After protective laws are built into the system, there are certain things that a society may not do to an individual, however much it might like to. Legal prohibitions on societal control create freedoms that are realities, not simply feelings or states of mind. Societies differ in their institutions of freedom and in the number and types of behaviors that are officially exempted from punitive control. Social systems that protect journalists from punitive control, for example, are freer than those that allow authoritative power to be used to silence critics or their vehicles of expression. Societies that possess an independent judiciary ensure greater social freedom than those that do not.

In philosophical discourses, freedom is often considered antithetical to determinism. When defined in terms of options and rights, there is no incompatibility of freedom and determinism. From this perspective, freedom is not conceived negatively as the absence of influences or simply the lack of external constraints. Rather, it is defined positively in terms of the skills at one's command and the exercise of self-influence upon which choice of action depends.

Psychological analyses of freedom eventually lead to discourses on the metaphysics of determinism. Are people partial determiners of their own behavior, or are they ruled exclusively by forces beyond their control? The long-standing debate over this issue has been enlivened by Skinner's (1971) contention that, apart from genetic contributions, human behavior is controlled solely by environmental contingencies, for example, "A person does not act upon the world, the world acts upon him" (p. 211). A major problem with this type of analysis is that it depicts the environment as an autonomous force that automatically shapes and controls behavior. Environments have causes as do behaviors. For the most part, the environment is only a potentiality until actualized and fashioned by appropriate actions. Books do not influence people unless someone writes them and others select and read them. Rewards and punishments remain in abeyance until prompted by appropriate performances.

It is true that behavior is regulated by its contingencies, but the contingencies are partly of a person's own making. By their actions, people play an active role in producing the reinforcing contingencies that impinge upon them. Thus, behavior partly creates the environment, and the environment influences the behavior in a reciprocal fashion. To the oft-repeated dictum, change contingencies and you change behavior, should be added the reciprocal side, change behavior and you change the contingencies.

The image of man's efficacy that emerges from psychological research depends upon which aspect of the reciprocal control system one selects for analysis. In the paradigm favoring environmental control, investigators analyze how environmental contingencies change behavior [$B = f(E)$]. The personal control paradigm, on the other hand, examines how behavior determines the environment [$E = f(B)$]. Behavior is the effect in the former case, and the cause in the latter. Although the reciprocal sources of influence are separable for experimental purposes, in everyday life two-way control operates concurrently. In ongoing interchanges, one and the same event can thus be a stimulus, a response, or an environmental reinforcer depending upon the place in the sequence at which the analysis arbitrarily begins.

A survey of the literature on reinforcement confirms the extent to which we have become captives of a one-sided paradigm to map a bidirectional process. Environmental control is overstudied, whereas personal control has been relatively neglected. To cite but one example, there exist countless demonstrations of how behavior varies under different schedules of reinforcement, but one looks in vain for studies of how people, either individually or by collective action, succeed in fashioning reinforcement schedules to their own liking. The dearth of research on personal control is not because people exert no influence on their environment or because such efforts are without effect. Quite the contrary. Behavior is one of the more influential determinants of future contingencies. As analyses of sequential interchanges reveal, aggressive individuals actualize through their conduct a hostile environment, whereas those who display friendly responsiveness produce an amicable social milieu within the same setting (Rausch, 1965). We are all acquainted with problem-prone individuals who, through their aversive conduct, predictably breed negative social climates wherever they go.

It should be noted that some of the doctrines

ascribing preeminent control to the environment are ultimately qualified by acknowledgment that man can exercise some measure of countercontrol (Skinner, 1971). The notion of reciprocal interaction, however, goes considerably beyond the concept of countercontrol. Countercontrol portrays the environment as an instigating force to which individuals react. As we have already seen, people activate and create environments as well as rebut them.

People may be considered partially free insofar as they can influence future conditions by managing their own behavior. Granted that selection of particular courses of action from available alternatives is itself determined, individuals can nevertheless exert some control over the factors that govern their choices. In philosophical analyses all events can be submitted to an infinite regression of causes. Such discussions usually emphasize how man's actions are determined by prior conditions but neglect the reciprocal part of the process showing that the conditions themselves are partly determined by man's prior actions. Applications of self-control practices demonstrate that people are able to regulate their own behavior in preferred directions by arranging environmental conditions most likely to elicit it and administering self-reinforcing consequences to sustain it. They may be told how to do it and initially be given some external support for their efforts, but self-produced influences contribute significantly to future goal attainment.

To contend, as environmental determinists often do, that people are controlled by external forces and then to advocate that they redesign their society by applying behavioral technology undermines the basic premise of the argument. If humans were in fact incapable of influencing their own actions, they could describe and predict environmental events but hardly exercise any intentional control over them. When it comes to advocacy of social change, however, thoroughgoing environmental determinists become ardent exponents of man's power to transform environments in pursuit of a better life.

In backward causal analyses, conditions are usually portrayed as ruling man, whereas forward deterministic analyses of goal setting and attainment reveal how people can shape conditions for their purposes. Some are better at it than others. The greater their foresight, proficiency, and self-influence, all of which are acquirable skills, the greater the progress toward their goals. Because of the capacity for reciprocal influence, people are at least partial architects of their own destinies. It is not determinism that is in dispute, but whether it is treated as a one-way or a two-way control process. Considering the interdependence of behavior and environmental conditions, determinism does not imply the fatalistic view that man is but a pawn of external influences.

Psychological perspectives on determinism, like other aspects of theorizing, influence the nature and scope of social practice. Environmental determinists are apt to use their methods primarily in the service of institutionally prescribed patterns of behavior. Personal determinists are more inclined to cultivate self-directing potentialities in man. The latter behavioral approach and humanism have much in common. Behavioral theorists, however, recognize that "self-actualization" is by no means confined to human virtues. People have numerous potentialities that can be actualized for good or ill. Over the years, man has suffered considerably at the hands of self-actualized tyrants. A self-centered ethic of self-realization must therefore be tempered by concern for the social consequences of one's conduct. Behaviorists generally emphasize environmental sources of control, whereas humanists tend to restrict their interest to personal control. Social learning encompasses both aspects of the bidirectional influence process.

When the environment is regarded as an autonomous rather than as an influenceable determinant of behavior, valuation of dignifying human qualities and accomplishments is diminished. If inventiveness emanates from external circumstances, it is environments that should be credited for people's achievements and chastised for their failings or inhumanities. Contrary to the unilateral view, human accomplishments result from reciprocal interaction of external circumstances with a host of personal determinants including endowed potentialities, acquired competencies, reflective thought, and a high level of self-initiative.

Musical composers, for example, help to shape tastes by their creative efforts, and the public in turn supports their performances until advocates of new styles generate new public preferences. Each succeeding form of artistry results from a similar two-way influence process for which neither artisans nor circumstances deserve sole credit.

Superior accomplishments, whatever the field, require considerable self-disciplined application. After individuals adopt evaluative standards, they expend large amounts of time, on their own,

improving their performances to the point of self-satisfaction. At this level of functioning, persistence in an endeavor is extensively under self-reinforcement control. Skills are perfected as much, or more, to please oneself as to please the public.

Without self-generated influences most innovative efforts would be difficult to sustain. This is because the unconventional is initially resisted and gradually accepted only as it proves functionally valuable or wins prestigious advocates. As a result, the early efforts of innovators bring rebuffs rather than rewards or recognition. In the history of creative endeavors, it is not uncommon for artists or composers to be scorned when they depart markedly from convention. Some gain recognition later in their careers. Others are sufficiently convinced of the worth of their work that they labor indefatigably even though their productions are negatively received during their lifetimes. Ideological and, to a lesser extent, technological advances follow similar courses. Most innovative endeavors receive occasional social support in early phases, but environmental conditions alone are not especially conducive to unconventional developments.

The operation of reciprocal influence also has bearing on the public concern that advances in psychological knowledge will produce an increase in human manipulation and control. A common response to such apprehensions is that all behavior is inevitably controlled. Social influence, therefore, is not a question of imposing controls where none existed before. This type of argument is valid in the sense that every act has a cause. But it is not the principle of causality that worries people. At the societal level, their misgivings center on the distribution of controlling power, the means and purposes for which it is used, and the availability of mechanisms for exercising reciprocal control over institutional practices. At the individual level, they are uneasy about the implications of psychotechnology in programming human relations.

Possible remedies for exploitative use of psychological techniques are usually discussed in terms of individual safeguards. Increased knowledge about modes of influence is prescribed as the best defense against manipulation. When people are informed about how behavior can be controlled, they tend to resist evident attempts at influence, thus making manipulation more difficult. Awareness alone, however, is a weak countervalence.

Exploitation was successfully thwarted long before there existed a discipline of psychology to formulate principles and practices of behavior change. The most reliable source of opposition to manipulative control resides in the reciprocal consequences of human interactions. People resist being taken advantage of, and will continue to do so in the future, because compliant behavior produces unfavorable consequences for them. Sophisticated efforts at influence in no way reduce the aversiveness of yielding that is personally disadvantageous. Because of reciprocal consequences, no one is able to manipulate others at will, and everyone experiences some feeling of powerlessness in getting what they want. This is true at all levels of functioning, individual and collective. Parents cannot get their children to follow all their wishes, while children feel constrained by their parents from doing what they desire. At universities, the administrators, faculty, students, and alumni all feel that the other constituencies are unduly influential in promoting their self-interests but that one's own group is granted insufficient power to alter the institutional practices. In the political arena, Congress feels that the executive branch possesses excessive power, and conversely the executive branch feels thwarted in implementing its policies by congressional counteraction.

If protection against exploitation relied solely upon individual safeguards, people would be continually subjected to coercive pressures. Accordingly, they create institutional sanctions which set limits on the control of human behavior. The integrity of individuals is largely secured by societal safeguards that place constraints on improper means and foster reciprocity through balancing of interests.

Because individuals are conversant with psychological techniques does not grant them license to impose them on others. Industrialists, for example, know full well that productivity is higher when payment is made for amount of work completed rather than for length of time at work. Nevertheless, they cannot use the reinforcement system most advantageous to them. When industrialists commanded exclusive power, they paid workers at a piece-rate basis and hired and fired them at will. Reductions in power disparity between employers and employees resulted in a gradual weakening of performance requirements. As labor gained economic coercive strength through collective action, it was able to negotiate guaranteed wages on a daily, weekly, monthly, and eventually on an annual basis. At periodic intervals new contractual contingencies are adopted that are mutually ac-

ceptable. In the course of time, as better means of joint action are developed, other constituents will use their influence to modify arrangements that benefit certain segments of labor and industry but adversely affect the quality of life for other sectors of society.

As the previous example illustrates, improved knowledge of how to influence behavior does not necessarily raise the level of social control. If anything, the recent years have witnessed a diffusion of power, creating increased opportunities for reciprocal influence. This has enabled people to challenge social inequities, to effect changes in institutional practices, to counteract infringements on their rights, and to extend grievance procedures and due process of law to activities in social contexts that hitherto operated under unilateral control. The fact that more people wield power does not in and of itself ensure a humane society. In the final analysis, the important consideration is the purposes that power serves, however it might be distributed. Nor does knowledgeability about means of influence necessarily produce mechanical responsiveness in personal relations. Whatever their orientations, people model, expound, and reinforce what they value. Behavior arising out of purpose and commitment is no less genuine than improvised action.

The cliché of *1984*, and its more recent kin, diverts public attention from regulative influences that pose continual threats to human welfare. Most societies have instituted reciprocal systems that are protected by legal and social codes to prevent imperious control of human behavior. Although abuses of institutional power arise from time to time, it is not totalitarian rule that constitutes the impending peril. The hazards lie more in the intentional pursuit of personal gain, whether material or otherwise, than in control by coercion. Detrimental social practices arise and resist change, even within an open society, when many people benefit from them. To take a prevalent example, inequitable treatment of disadvantaged groups for private gain enjoys public support without requiring despotic rule.

Man, of course, has more to contend with than inhumanities toward one another. When the aversive consequences of otherwise rewarding life-styles are delayed and imperceptibly cumulative, people become willful agents of their own self-destruction. Thus, if enough people benefit from activities that progressively degrade their environment, then, barring contravening influences, they will eventually destroy their environment. Although individuals contribute differentially to the problem, the harmful consequences are borne by all. With growing populations and spread of lavish life-styles taxing finite resources, people will have to learn to cope with new realities of human existence.

Psychology cannot tell people how they ought to live their lives. It can, however, provide them with the means for effecting personal and social change. And it can aid them in making value choices by assessing the consequences of alternative life-styles and institutional arrangements. As a science concerned about the social consequences of its applications, psychology must also fulfill a broader obligation to society by bringing influence to bear on public policies to ensure that its findings are used in the service of human betterment.

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The Substance and the Shadow

BERNARD M. BASS *University of Rochester* [1]

In industrial/organizational psychology (as in social science, generally) we have been grasping at the shadow—but losing sight of the substance. We should attend more fully to both. Each supports the other in advancing our discipline. While nothing may be as practical for advancing technology (the substance) as a good scientific theory (the shadow), it is probable that nothing may be as impractical as a bad theory. The reverse is also generally true. Good technology may be a prerequisite for the advancement of a science. Bad practice can ruin potentially good science; it can send science on false leads and wasteful detours into fads and fancies.

Bad theories in social science often are bad in form and bad in effect. Theories may be bad in form because they are no more than speculation, propaganda, or wishful thinking. They may be bad in effect because they are premature in the time line of investigation of a phenomenon. Frequently, they are bad in both form and effect because they lack connectivity with substance—the real world of the social practitioner, artist, technician, craftsman, and engineer. Viteles (1972) pinpointed this gap between the science of psychology and its applications to social engineering. I will try to suggest some bridging processes [2] which should not only strengthen real-world applications but also strengthen the science.[3]

Industrial/organizational psychology has arrived late at the scene for the "theory-hypothesis testing" bout with nature. The pleas in the 1950s were filled with it; the 1960s gave it to us; and now in the 1970s, we are up to our ears in it. After wallowing through "shot-gun" data gathering in earlier decades, industrial and organizational psychology was smitten by the bug for building theory and quasi theory in the past decade. Thus, motivation-hygiene theory led ultimately into an excess of job enrichment efforts, often with marginal payoffs and relatively little technical advancement in sociotechnical aspects of job design. Although some attention has been paid to the effects of individual differences in preferences, conflicts with the economic and technical constraints which impose limitations on what changes are desirable and feasible have mostly been ignored (Gomberg, 1973).

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[1] An earlier version of this article was presented as the Edwin A. Henry Memorial Address at the meeting of the American Psychological Association in Montreal, Canada, August 28, 1973. I am indebted to a number of my colleagues, including Gerald V. Barrett, Enzo R. Valenzi, Ralph A. Alexander, Zur Shapira, and John A. Miller for helpful suggestions. Also helpful were Gerald Emsch of the University of Rochester and Carl E. Bennett of the Battelle Institute.

Requests for reprints should be sent to Bernard M. Bass, Graduate School of Management, University of Rochester, Rochester, New York 14627.

[2] Ed Henry, professor, consultant, and employee relations research director, could operate comfortably on both sides of the gap. It is fitting that this article deals with bridging the gap.

[3] By science, I mean the accumulations of understandings and formulated knowledge about universals using abstract and logically compelling means. By art or technology, I mean the accumulation of workable practices and, particularly, predicted and controlled applications. As W. J. Jevons noted: "Science teaches us to know; and art, to do." Science emphasizes doctrine, theory, and controlled experimentation; art and technology stress experience and operationality. Science accentuates rationality, consistency and full systemization; technology emphasizes sensibility, reasonableness, and heuristics.

In actuality, there is much overlap. Nevertheless, it seems reasonable to agree that the organizational/industrial psychologist is acting more like a scientist than a practitioner if his activities can be labeled as basic, abstract, and theoretical. Similarly, he is more readily recognized as practicing his profession if he is applied, concrete, and empirical. There is no merit in starving one sector of research or practice for the sake of the other. Nevertheless, there often are imbalances in support for one or the other and lack of connection between them detrimental to the advancement of both (Archibald, 1970; Bakan, 1966; Bandura, 1969; Brann, 1972; Brooks, 1965; Bunge, 1966; Fishbein & Ajzen, 1972; Forbes, 1956; Fried, Gumpper, & Allen, 1973; Margenau, 1961; Moser, 1971).

Why the Gap?

There are at least three epistemological reasons for the continuing rift between social science and social technology and the failure of social science to build from a base in social technology: (*a*) Social science and social technology differ in norms and goals; (*b*) in social science, theory is treated as an end rather than a means; and (*c*) social scientists ignore the differences between social and natural science. Four additional reasons are methodological: (*d*) We fix problem delineation and conceptualization prematurely; (*e*) we push for complete theorization prematurely; (*f*) we push for experimental rigor prematurely; and (*g*) we move to mathematical modeling prematurely.

Identifying the reasons for the gap will help to point out ways of bridging it.

NORMATIVE DIFFERENCES BETWEEN SCIENCE AND APPLICATION

The differential concerns of science for truth and technology for workability make it difficult for an industrial/organizational psychologist to feel comfortable, able, and willing to operate on both sides of the gap between science and application. When he does, little real bridging is attempted.

Differential values and rewards on either side of the chasm maintain it. Self-selection and selective attention to what is required for career advancement contribute as well. The science-oriented academics ignore or reject the practice-directed organizational development consultants, and vice versa.

Each side builds up self-satisfying bodies of knowledge and methods that do not take into account the results of the other's accumulations. As each body becomes more developed without interconnections to the other, it becomes increasingly difficult to open and maintain links between them. The scientist builds esoteric theories, which are mainly of interest to his scientific peers, with little attention to what the technologist is doing. The technologist, ignoring scientific endeavors and evaluations, builds esoteric skills mainly of interest to his disciples. Such vested interests increase the impermeability to contributions from across the gap. The scientist has to learn an entire new set of skills if he wants to get some understanding of the practice that has developed. He may have to reject what seemed to be some fundamentals of his science. The social technologist has to learn more mathematics or has to deal with a set of abstractions that fail to mirror his reality.

Coleman (1972) noted a number of critical differences in the criterion requirements for applied policy-oriented research in the world of action and basic discipline-oriented research. The world of action operates in real time; the disciplinary world is timeless. Language and concepts of the world of action differ from those of the disciplines; the translation from one to the other is a key problem. The world of action involves vested interests; the disciplinary world strives for neutrality. Redundancy is useful to the world of action; parsimony is the rule in the disciplines.

Translating results of scientific endeavor into practical application often requires that the rigorous measures and constraints of the scientific effort have to be relaxed for the application. For expediency, the longer reliable test form in an experiment may become the five-minute short-form edition in practice, where it may fail to work. At the same time, social science finds it difficult to incorporate relatively weakly controlled, less reliable applications in broader real-life complex operations. For, as current social science, it is locked into a theory-hypothesis testing quest for understanding which cannot make much use of advances in social technology. A more dynamic conceptualization of scientific theory, models, data gathering, and applications is required.

SCIENCE VERSUS SCIENTISM: THEORY FOR THEORY'S SAKE

In pushing for more theory in the absence of strong technology, we have failed to distinguish between science and scientism. Theory for theory's sake is scientism, not science. Sometimes we are at a stage when more theory is needed before further understanding is possible. Other times, however, more theory may hinder rather than help. To solve social problems, social scientists conduct research to generate and test fundamental broad generalizations and to achieve general methodological solutions rather than conduct research which is less ambitious in scope and is directed at solving particular problems. Once the generalizations have been proved, it is believed that solutions to the particular problems will follow automatically. But, as Glaser (1973) has noted:

> The sequence from basic research, to applied research, to development, to practice and application on which most

of us were weaned is no longer applicable if, in fact, it ever was [p. 557].

SOCIAL SCIENCE AND NATURAL SCIENCE ARE DIFFERENT

On the day of his death, Socrates criticized the natural philosophers such as Anaxagoras for trying to use the same explanations for human as for natural phenomena. It is still germane to criticize the failure to note the differences between natural and social science. While rationality is intended by both, the relative roles of theory, models, and data gathering may need to be quite different. Ordinarily, in natural science, identifiable physical mechanisms with known relations can be sought to eventually connect observed behavior with antecedent conditions. However, in social science, observed behavior usually can only be connected via inferred states to antecedent conditions. Thus, theory is in a very different role in the search for understanding the natural compared to the social sciences.

As social scientists we face much more difficulty in our search for understanding than natural scientists face. We deal with much more complex, shifting, and fluid phenomena, differing widely from place to place and group to group and often lacking stability over time (Beck, 1949; Myrdal, 1973, p. 32). It seems logically compelling that the more a country invests in growth, the more it will grow. And so economists used a capital/output ratio like a natural science constant. But now it is known that capital investment is not even a major antecedent of economic growth (Myrdal, 1973).

Differences in the utility of precision. Meehl (1967) pointed to a paradox. The very thing that may be desired and required of measurement in the physical sciences may be undesirable in the social sciences. He suggested that whenever we can and do improve the power of statistical designs, we may increase the prior probabilities to 50% of finding statistically significant differences in the theoretically predicted directions even if the theory is without merit!

The very precision that would lend confidence to the generalizability of results to support a theory from a tightly controlled, precisely measured physics experiment literally reduces confidence in the generalizability of results from a similarly tight psychological experiment to support a psychological theory. Meehl reasoned that in social science any dependent variable of interest is a consequence of several strong effects such as genetic makeup, learning history, immediate motivation, and stimulus conditions. Superimposed on this finite list of important determiners are an indefinite, large number of random effects. For two groups not to differ, all these random effects have to balance out. The probability of such balancing is quite low. It becomes almost a sure thing that we can reject the null hypothesis by increasing the power of our design. For instance, by making samples large enough, most results will be significant. Meehl reported finding statistically significant relationships in 91% of miscellaneous, accidental pairwise associations for a sample of 55,000 Minnesota high school students.

Other differences. In the natural sciences, ideological revolutions take place in the acceptability of particular conceptual paradigms (Kuhn, 1962) but not so in the social sciences (Bronfenbrenner, 1971).

Complicating social science is the need to establish and maintain necessary expressions which "in compressed form . . . stand for complex, vaguely perceived ideas . . . about social reality [Myrdal, 1972, p. 32]." In social science, we formulate paradigms, complex sets of assumptions and beliefs that are adopted implicitly when researching a topic (Cartwright, 1973). Furthermore, although in the natural sciences value premises are simple and mostly a priori evident, in the social sciences "valuations are diversified and anything but self-evident [Myrdal, 1973, p. 34]." Nevertheless, we have tried to emulate the natural and physical scientist by constructing "utterly simplified models often given mathematical dressing [Myrdal, 1973, p. 33]."

However, the fault may not lie in the mathematical modeling and related abstract theorizing but in their premature application in much of social science before less demanding structures have been built, tested, and modified according to how well they mirror reality.

PREMATURITY OF DEFINITION AND CONCEPTUALIZATION

Premature freezing begins with the rush to define the problem to be investigated. The definition then determines not only what we will do, but what we will not do with the problem (Caplan & Nelson, 1973). Often the definition is selected because the

social scientist has the method available to study the problem provided he defines it so that the method will be applicable. Later on, he seeks problems so that he can use his method. Finally, he finds problems everywhere for which he can use his method. As Kaplan's (1964) law puts it: Give a small boy a hammer, and suddenly he discovers everything needs hammering. If you invite a people-oriented consultant into the factory, he will find people problems and needed people changes. If you invite a communications expert, he will see needed communications changes everywhere, not people changes (Leavitt, 1965). We engage in the drunkard's search. We hunt for our keys under the lamppost, because it is lighter there, not because we have any more reason to believe we lost the keys near the lamppost rather than elsewhere in the dark (Kaplan, 1964). We become "prisoners of our own procedures [Gardner, 1965, p. 47]."

We confuse our valuations of how reality ought to be with our beliefs about what it is (Myrdal, 1973). Such propaganda then excludes from research and practice alternatives focusing on other aspects of empirical reality. It excludes divergence in opinion often important in leading to more creative understandings.

The freezing tends to be extremely resistant to thawing. Once these definitions are "legitimated and acted upon, they tend to define the problem indefinitely irrespective of their validity [Caplan & Nelson, 1973]." Risky shift research is illustrative (Cartwright, 1973). The freezing is further enhanced by our deference to the credibility of authority (Becker, 1970) and the rewards for pursuing theoretical rather than applied interests (Caplan & Nelson, 1973).

PREMATURE FREEZING OF THEORIZATION

"Mind-forged" manacles bind "many a gifted scholar [smothering] 'his creative talent' . . . by a growing commitment to his own previously stated doctrine [Gardner, 1965, p. 52]." Thus, if the turn-of-the-century world of physics had been more locked into theory as we may be now in social science, quantum mechanics would have been rejected by that world because it was not logical in the sense that it did not conform to Aristotelian logic nor did it do as well as Newton for macroscopic physics.

In applied psychology, too often, unchanging theories tend to be tested and retested and although results fail to support them or give marginal support (i.e., $p < .05$, $r = .20$), the theories remain and the results are used to maintain their status over long periods, often decades. Bad theories never die, they just slowly fade away in the social sciences:

> An old theory is not discarded solely because someone comes up with a better alternative: it is cherished and preserved by devout adherents until its day of resurrection and ultimate triumph, the meanwhile being made more irrefutable, generally applicable, and ambiguous with every passing year [Starbuck, 1972, p. 17].

We are not alone. "In economics, all doctrines live on [Myrdal, 1973]." But as Ernst Mach pointed out, theories ought to wither away leaving a body of empirical facts behind (Frank, 1949, p. 62).

If you read a typical study purporting to test valence-instrumentality-expectancy theory, the results do not lead to a revision of the theory. We play with a nomothetic valence-instrumentality-expectancy network. A few weak relations are overemphasized. Stronger results are discussed selectively to support a chimera followed by surprise at the empty endings. One more article is published and the next appears using somewhat different methods but trying to test the same theory.

If we do not develop a better technology and approach in this area, my guess is that after another 100 experiments to be reported in the *Journal of Applied Psychology* between now and 1980, we will actually know little more about designing specific programs to motivate workers than we do now, given the existence of Pascal's theorem published first in 1669 that expected utility is equal to the sum of the subjective probabilities of earning rewards from a designated act and alternative acts available (Pascal, 1964). Heneman and Schwab (1972) reviewed nine field investigations which attempted to test expectancy theories of motivation to work. While valence, instrumentality, and role perceptions were each significantly (but modestly) related to performance, little support was obtained for the hypothesized interactions. Poor methodology was blamed for the failures.

Yet nobody is going to question that performance depends on ability and motivation. Nobody is going to question that Pascal's expectancy theorem is plausible. The utility of research here will be in finding out how these variables combine in their effects under varying conditions. Before we can do much with these broad truisms, we need to

develop consensus among investigators and the technical facility to measure each of the variables in a variety of situations.

Theorization gets too far ahead of technology. Often in psychology, we have been trying to imitate primarily the theoretical propensities of physical science without the kind of technology that contributes so much to the development of theory after the fact or provides rapid tests of the adequacy of theoretical attempts before the facts have been established.

For instance, today, in their quest for judging the worth of academic personnel on the basis of their contributions to teaching, a substantial number of institutions of higher learning depend on student ratings to evaluate the effectiveness of their teachers. Nevertheless, Rodin and Rodin (1972) reported a correlation of −.75 between student ratings of the effectiveness of each of 12 calculus teachers and the mean amount learned in each of these teachers' calculus classes. What kind of science can be built with such an erroneous technology?

We put too much weight on the scientific search for universals without the equally important craft side of our enterprise. In particular, the academics among us often act like ancient Greek scientists disdaining psychological technology as opposed to psychological science. Nevertheless, technology and science are mutually dependent; each feeds on the other. The closer the coupling, the more rapid the negative feedback loops and the greater the likely progress and well-being of both. But current theories are seen as far more important than current problems—at least in the academic world. Learning about controlled experimentation and hypothesis testing is still emphasized compared to learning about how to search, select, discover, or invent technology to deal with real social problems. Yet, I argue that we will make significant contributions to the extent we can deal with real-world complexities rather than artificial laboratory problems; to the extent we can deal with ill-defined messes (Ackoff, 1972) rather than replications of prototype problems.

As John Gardner (1961) noted:

> The society which scorns excellence in plumbing as a humble activity and tolerates shoddiness in philosophy because it is an exalted activity will have neither good plumbing nor good philosophy. Neither its pipes nor its theories will hold water [p. 86].

Our theories often are quasi theories. I do not mean to imply that theorizing is necessarily bad or that theory is unessential. What I see as strangling is quasi theory couched in pseudoscientific jargon, which is usually a consequence of its being prematurely and/or poorly constructed. What is missing from quasi theories is formalization with explicit assumptions. Quasi theorists remain narrow minded and fail to see that once the logic is made explicit every piece of logic can have multiple empirical examples and every empirical example can be handled by many different logical structures. "The fallacy . . . is supposing that any single theory can capture the essence of concrete reality. . . . Logic is an infinitely rich and variable instrument [Diesing, 1971, p. 122]."

Krause (1972) applied symbolic logic to postulates that underlie cognitive dissonance to derive 25 predictions. He found that with all the empirical research that has been done in the area, only 2 predictions have ever been tested! That is, the informal logic that represented cognitive dissonance theory badly failed to convey the many alternative predictions that could be derived. What were thought to be derived were no more than value judgments. Nevertheless, such judgments constrained exploration and search in the false supposition that we were pursuing a highly logical, scientific approach to understanding.

Theoretical arguments in ordinary language move along so smoothly that there is a failure to see "the underlying logical machinery—the relational definitions, the exhaustive classifications, the tautologies [and] the hidden assumptions. . . . The theory [is treated] as a simple description of the facts . . . rather than an abstract bit of logic in disguise [Diesing, 1971, p. 121]" and seems very convincing to its proponents.

> Further, since all possible facts can fit into any complete set of logical categories or any set of tautologies, there is no way of falsifying the "descriptions." Consequently as the theorist looks around for evidence to support his theory, he finds to his delight that the evidence is overwhelming. There is no contrary evidence! Everything in the world falls into place, and he comes to see that the theory is really a deep, fundamental statement of the most essential principles of human behavior. As a result he becomes the prisoner of his own logical model and is rendered incapable of seeing reality from any other standpoint [Diesing, 1971, p. 121].

Thus, Skinner "finds schedules of reinforcement wherever he directs his attention." Homans comes to see all social behavior in terms of exchange (Diesing, 1971, p. 121). And the neoclassical economists (e.g., Frank Knight, Ludwig van Mises,

and Henry Simons) "seemed to think they had penetrated to the very essence of rational human action, and that they knew whatever was knowable about it [Diesing, 1971, p. 121]."

Diesing suggested that such theorizing makes for poor understanding but good propaganda by passing off an idealized picture as a true account of reality. Facts that do not fit are quietly ignored, but the propagandist is left convinced that he is telling the truth. Kline (1962) called this *factifuging* and has detailed more than 20 ways for avoiding facts that conflict with current understanding. It may be that a premature theory in social science is like a politician and can only satisfy everyone by saying nothing (Popper, 1959).

PREMATURE OVERCONTROLLING

Our urge to be scientists leads us to ignore E. H. Moore's precept "Sufficient unto the day is the rigor thereof [Bass, 1960, p. 31]." We rush to institute controls in our data gathering before we learn what to control. Thus we may control age of subjects when age is unrelated to either the mean differences between samples or within samples. At earlier stages in a line of investigation, finding out what to control is far more important than controls for controls' sake. Just because our samples differ in age or our subjects within samples differ in age may be utterly irrelevant.

We need to avoid premature overcontrolling of expensive and wasteful effort at earlier stages of understanding in a given field. At the same time, to advance our discipline we need to give more attention to the implausibility of rival alternative hypotheses which could account for our results in early stages of model development (Campbell & Stanley, 1969).

PREMATURE FREEZING INTO MATHEMATICAL MODELING AND COMPLETE RATIONALITY

The extremes of rigidity in approach, method, and outcomes reach a zenith in the effort to bodily transfer the mathematical modeling and theorizing derived from engineering systems to understanding and controlling social and behavioral problems ranging from beating the North Vietnamese to delivery of hospital services (Hoos, 1972).

This is not to say that the linear and nonlinear programming models and so on are in themselves inconsistent or invalid for understanding the physical systems for which they were developed. Rather, the experience, judgment, values, and accumulated understandings of the social scientist must be brought into play in the many approximations and estimations required for these models to begin to have any chance of replicating social and behavioral reality (Hoos, 1972). Otherwise, they become like the Walras model in economics, forms of analysis without much substance (Samuelson, 1952).

> Unfortunately, the lack of factual knowledge of conditions existing in the real world forces the model builder to base many if not all of his general conclusions on all kinds of *a priori* assumptions, chosen for their convenience rather than for their correspondence to observed facts [Hoos, 1972, p. 37].

Will applied psychology go the way of economics and become increasingly the province of the mathematically anointed dealing with shadows rather than substance? You may think I have created a straw man rather than a real possibility, but keep in mind that organizational/industrial psychologists are increasingly finding employment in business schools where economists often form the dominant faction. It may be difficult for many of us to avoid slipping into the same fallacious activities as a consequence of interdisciplinary pressures toward conformity which can exist in academia.

The costs to society as well as to social science of the premature modeling and theorizing were learned through the bitter experience of Southeast Asia where an enemy played by its own irrational (to us) rules rather than ours. The costs are continuing at home where we are seeing the economist who believes himself to be completely rational becoming "the expert in all aspects of public planning, whether the area be health, education, urban redevelopment, or construction of low-cost housing [Hoos, 1972]."

Apparent rigor and certitude of economic analysis, its models and its mathematics, have been allowed to outweigh other outlooks and methods more pertinent to reality. In the interests of simple solutions to complex problems, an economist can seriously argue that city centers ought to be allowed to deteriorate in value so that their price to developers becomes low enough to enable the latter to profitably redevelop the area. Ignored are all the social, cultural, and political costs of allowing such a process to occur.

Ways to Bridge the Gap

What I want to do is show how investigators ought to approach reality and how the mathematical

models ought to appear only after much earlier work has been accomplished.

I offer 10 interdependent guidelines to pass over the pitfalls dug by assuming too much similarity between science, social science, and social technology and to bridge the methodological gap caused by a too hasty rush to complete systematization: (*a*) hang loose; (*b*) strengthen informational links; (*c*) advance our technology as we advance our science; (*d*) be open to more issues at the interface of science and technology; (*e*) make more use of technical assignments; (*f*) consider the relative state of the science and the technology; (*g*) change our statistical usages; (*h*) increase the fidelity of the variables we investigate; (*i*) use multiple investigatory approaches; and (*j*) teach both science and technology.

HANG LOOSE

The path to useful definition, conceptualization, and theory construction calls for more work in wallowing around in reality and more openness to experience prior to being frozen into a narrow statement of the problem, rigid conceptualization, a parochial methodology, turgid theorization, and "mathematical dressing." Rogers (1973) may be saying much the same thing with more feeling when he calls for a more humanistic science of psychology.

From exploration to systematization. For a metaphorical prescription for what I mean by hanging loose, I am indebted to Gerard Emch (personal communication, April 1973):

1. An area of ignorance is to be conquered. Paratroopers are sent in who do not necessarily even know on what or where they will land. There already may be some rudimentary road maps and guidebooks available, but the need here is for openness to reality, for avoiding premature closing of leads and hunches.
2. The long-range artillery is brought up for global attack. They depend, of course, for information feedback from the paratroopers.
3. Ground forces fully occupy the territory.

Improvable definitions. Hanging loose implies looser definitions at earlier stages in the time line of investigation. Definitions are always improvable. In the light of new data we modify old definitions. When an accepted definition proves inadequate to handle the many exceptions that emerge both in the laboratory and in the field, we modify it. The exclusive categories of matter as solid, liquid, or gas give way to handling the discoveries of new forms such as plasma (Stevens, 1951).

More trial and error. Hanging loose implies encouraging trial-and-error problem solving by behavioral technologists to give chance more of a chance. We would like to believe that science advances mainly by constructing models that generate testable hypotheses. In fact, serendipity plays a much more important role in some fields, although not in all. Jacques Monod (1971) suggested that most of the discoveries in biogenetics have been serendipitous. However, Platt (1964) argued that the advances in molecular biology are a consequence of rigorous application of strong scientific methodology. I surmise serendipity is a particularly significant factor in organizational/industrial psychology.

Before we make large-scale investments in social programs, we need to invest a longer period of trial and error in engineering and re-engineering simulations or small field demonstrations of what we hope ultimately to introduce. Going from deductive models or from laboratory studies directly into the field with a large-scale innovation seldom works. We need to spend much more of our time, energy, and resources in the field, working as real systems tinkerers and cooks, trying to establish generalizations from our experiences in efforts to make a sequence of small changes in the system in which we are interested. Over 40 job-enlargement experiments are being carried out among more than 1,500 workers in one Volvo plant I recently visited. The new job designs are created out of management–worker discussions, with a few primitive models[4] in mind requiring simple changes in task

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[4] A *theory* is a systematic, generalized explanation of some part of reality that is scientifically acceptable; a *model* is a simplified representation of reality instead of a broad explanation. Theory generalizes to all cases; models cover a few specific situations (Starr, 1971). Models can be simple lists of possible factors of consequence or they can be complex arrangements specifying functional relationships between variables and constants.

Starr described three increasingly complex models:

1. A shopping list designed to increase a family's happiness is a primitive model. It may include soap of an unspecified brand, size, and price, as well as coffee of a particular brand and amount. Satisfaction can be expected to depend on price, taste, and shopping convenience. Factors can be combined to achieve specified objectives. Variables can be changed in amplitude and intensity.

configurations. Ultimately complex theory may emerge in this area, but for the present, trial-and-error advances in technology seem much more productive.

More problem focus and subjective orientation. Edwin Land ("Edwin Land," 1972) has noted that if you can completely identify the problem, it can be solved. That is, if you know the elements of consequence in the particular situation and how they affect each other, you are well on the way to solving the problem. In social science, you also may need to know how the elements of the problem affect you. Investigators need to give more credence to their own observations, sensations, feelings, as well as those of other astute observers who have tried to articulate the nature of the problem and have proposed models for representing the problem (Rogers, 1973). Out of this can emerge a primitive model listing the variables expected to be of importance as well as how to measure and dimension them.

Revisionistic thinking. By beginning with problems—of which there is no scarcity—and then concentrating on model building and model testing with the expectation that the model will be improved with each inductive test, we will approach better solutions of the real problem for which the model is in incomplete representation. Tests ought to be designed so that whatever their outcome, a revised model will emerge which in turn will contribute to changing the related broader theory.

If we want to use a priori thinking before data collection with some profit, what we need as applied psychologists is to think more in terms of transitional models, "each giving way to a succession of simpler and empirically more accurate models under the guidance of research findings [Heller, 1972, p. 19]." The transitional models can become increasingly complex as we progress down the time line of investigation. A deliberate policy of revision is entailed as well as location of the boundary conditions for the validity of the developing model.

Tukey and Wilk (1965) underscored the need to stay loose with one's models and theories when they speak about the common requirements for good experimentation and good data analysis:

> some ideas [in advance] of a model, are virtually essential, yet these must not be taken too seriously. Models must be used but must never be believed. [Furthermore] data analysis, like experimentation, must be considered as an open-ended, highly interactive process, whose actual steps are selected segments of a stubbily branching, tree-like pattern of possible actions [pp. 9–10].

Myrdal (1973) observed that "facts kick, even if somewhat belatedly, when data have been assembled under inadequate categories [p. 35]." We need to be ready to receive the kicks to revise our categories. We need to be able to revise our models to fit the facts rather than revising our methods to fit the models. This calls for solid fact finding, for becoming technologically proficient in establishing empirical facts before we try to move from models based on laboratory and survey fact finding to social engineering. At the very least, we need to build and revise transitional models as we move from survey and experiment into the field.

More consensus on measures. If different investigators are able to employ the same standardized but improvable measures (see Barrett, 1972) with reasonable confidence in their validity, the beginnings are available for constructing a useful theory, one which transcends a variety of situations, rooted in already successful problem solutions. As noted earlier, all too often we tend to start theorizing prematurely with little preceding modeling to provide some minimum confidence in the reliability or validity of what we purport to connect together with our theory. And failure to attend to the adequacies of one's measurements can lead one far astray following what he would like to find in reality, not what he really finds. The Tosi, Alda, and Storey (1973) reexamination of Lawrence and Lorsch's (1967) seminal study is illustrative. Expanding on Kelvin's dictum (Wheeler, 1956) that if you cannot measure it, you do not know what you are talking about: *If you cannot measure what you are talking about, no amount of theorizing will put you any closer to understanding the real world.*

Delay serious deductive efforts. It is only in the third stage of exploration to systematization that we should begin trying to be fully systematic and

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2. An intermediately complex model specifies relationships such as $y = f(x)$. It also posits a priori that given outcomes will depend on given antecedent conditions. For example, the model can specify that the amount of coffee purchased will be a function of the amount remaining on the home shelf, the amount of coffee drunk each day, and the frequency with which the shopper visits the grocery store.

3. The most complex model has feedback which alters values of the variables of the model. Relationships can be changed based on the data gathered. For example, price rises may force family discussions about continuing to purchase the particular brand rather than turning to a less expensive one. At this point we probably are ready to start formal theorizing with some purpose.

thoroughly deductive. Thus, Tukey and Wilk (1965) noted that:

> While detailed deduction of anticipated consequences is likely to be useful when two or more models are to be compared, it is often more productive to study the results before carrying out these detailed deductions [p. 10].

Furthermore, they argued that:

> In both good data analysis and good experimentation, the findings often appear to be obvious—but generally only after the facts [p. 9].

It is at this time in a line of investigation that we become capable of building a formal theory. As Kline (1953) noted:

> Logic . . . sanctions the conquests of intuition. It is the hygiene [which helps] to substitute intuitions more consistent with the mass of ideas than currently stated thoughts [p. 408].

It would be nice if we could base our models on theory but *it is more likely that useful theories will emerge if we first have been able to construct some reasonable models of the behavior we are interested in theorizing about*.

STRENGTHEN THE INFORMATIONAL LINKS BETWEEN SOCIAL SCIENCE AND SOCIAL TECHNOLOGY

Robert Glaser raised my consciousness about these matters at the 1972 APA meeting when discussing my revising *Training in Industry: The Management of Learning* (Bass & Vaughan, 1966). He said that the first half of the book dealing with a traditional overview of learning, motivation, and perception needed considerable augmentation. In its present form, it is mainly a list of variables which need to be considered by anyone interested in designing an applied training or development program. It needs to provide better bridges from learning to training, a straddling between laboratory and field. Glaser (1973) has elaborated on the need for this interactive mode of research in education and educational psychology.

Unfortunately, few of us are effective straddlers. More ought to be. But more important we need to develop mutually influencing linkages between social science and social technology.

Problem-centered social technologists have a lot to offer to those interested in fundamentals. In turn, the social scientist may offer much guidance to social technologists. For both, much creative work proceeding from exploration to systemization lies ahead with transitional models representing the areas in which they both are working. Note that I reach this without one word about relevance. From this point of view, the fact that the research approach I propose may be more relevant is a happy coincidence. It may also mean that what is relevant in a single instance has greater possibilities of contributing to generalities which will hold up across many instances. To say something is irrelevant may also mean it has limited potential for contributing to generalization across instances.

The contrast between this proposed relation between scientists and technologists and the arrangements more often thought to exist is schematized in Figure 1a. In Figure 1a, scientists spin around their own axis building models increasingly closer to the truth until one can be spun off to social technologists, who then develop successively more workable models. This is the science-to-application approach which I have already noted is more fiction than fact. In Figure 1b is an approach with more promise of connecting needed applications with basic science. Successively more complex models increasing in both truth and workability flow back and forth between scientists and technologists. The

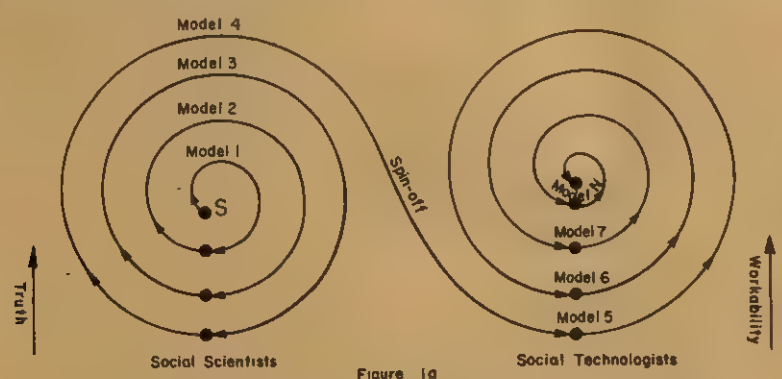

Figure 1a. Ineffective relations between social scientists and technologists.

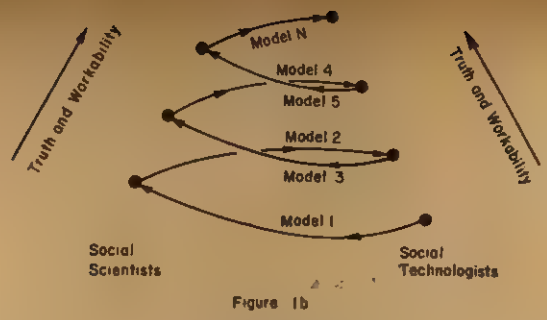

Figure 1b. Effective relations between social scientists and technologists.

gap between them tends to close as the models increasingly reflect the contributions of both.

What I propose for industrial/organizational psychology is consistent with what is now recognized as the appropriate relations between science and technology.[5] On occasion, in the physical sciences, theoretical insights have preceded technological advances. Einstein's mass-energy equation preceded the practical conversion of mass to energy. But just as often, contrary to popular belief, much of theory in the physical sciences created unifying but after-the-fact explanations of what physical technology had already reliably demonstrated. Thus, "flight by machines heavier than air was still being declared impossible by eminent scientists when in 1903, the first airplane was unexpectedly lifted into the air [Brinkman, 1971, p. 125]." Again, "[Gail Borden] did not know enough to be confused by the theories [of his day, the 1850s] which proved that [one could not condense milk]. . . . He simply went ahead and did it [Boorstin, 1973, pp. 27–28]."

Many times technological advancements were made based on false scientific assumptions. Thus, Watt built his steam engine as an adherent of the phlogiston theory (Brinkman, 1971) and Agassi (1966) claimed that even today "we do not believe but we still apply Newtonian mechanics [p. 364]" which does not work very well for micromechanics.

More than the practical application of science, technology is a "mode of disclosing" of a particular world and making it manifest according to Heidegger (Alderman, 1970). In advancing technology we advance science and in advancing science we advance technology (Skolimowski, 1966).

> What the executing hand taught the head was used again for the better guidance of the hand. Thinking followed the action and was at the same time a pre-thinking of the next action . . . knowing and making began to aid each other and it did not take very long before physical science and technology discovered that they essentially belonged together [Melsen, 1961, pp. 224; 241].

A good case in point in industrial/organizational psychology is change agentry. Much is being written about what change agents should do. The prescriptions mainly derive from the values of the prescribers and extension of surveys and experiments loosely connected to past practices and personal experiences. Change agents often claim they are being guided by theory, but as Tichy and Hornstein (1972) noted, most change agents are artisans rather than scientists. This does not mean they should be dismissed or ignored by those interested in scientific understanding of organizational change. Tichy (1972) identified four types of change agents with both similarities and differences in values, approaches, methods, and objectives: outside pressure agitators, top-down efficiency experts, organizational development craftsmen focused on improving the organization's problem-solving capabilities, and people-change artisans working to develop increased individual skills and satisfactions. Knowing this gives us the rudimentary beginnings for modeling alternative organizational change efforts and proceeding to appropriate next steps to understanding the change processes.

Figure 2 shows (with the heavy arrows) the informational links between social scientists and technologists that need particular strengthening. Tichy's work represents one attempt to start doing so. Some of us devote ourselves to trying to confirm the validity of a model. Others see a better technology as an end product. We should be clear that spending all our time and effort on one will

[5] Noting the differences between science and social science and then using the history of science to illustrate directions for social science may seem contradictory. This is not so. The differences call attention to the fact that the lessons of science may need qualification when applied to social science to the extent that both are quests for rational understanding but are about different subject matter.

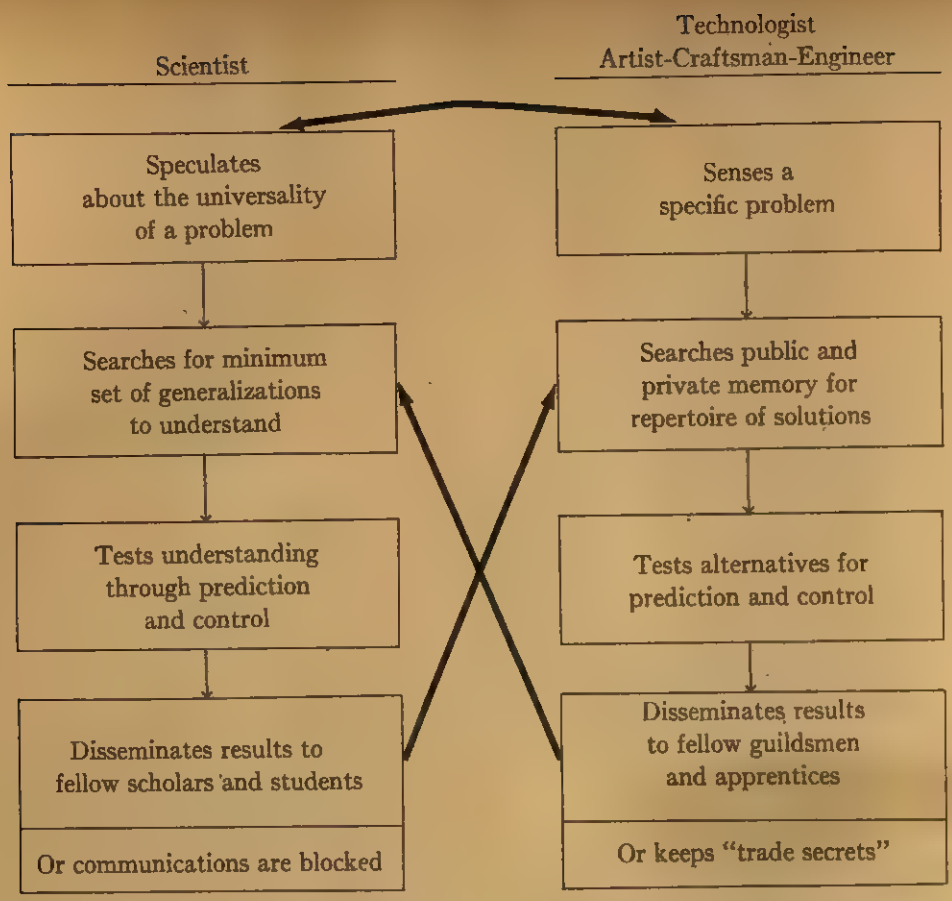

Figure 2. Informational links that need to be strengthened in the social sciences.

not necessarily directly advance the course of the other. The connection must be made. Subscribing to this position, Glaser (1973) sees that the times are ripe and society more open than ever for experimentation and innovation which will make it possible for educational psychology to move itself forward in this way.

ADVANCE OUR TECHNOLOGY AS WELL AS OUR SCIENCE

How much attention do we pay to our paratroopers in industrial/organizational psychology? How often do we even hear from them publicly? How do they make themselves heard? Our science tries to occupy the unknown territory with full systemization long before we have much sense and measure of what is really going on in the territory, long before we have tried a series of progressively better models to give us clues as to what direction a formal theory will have to take.

Industrial/organizational psychology needs to support its paratroopers, artillery, and occupying forces. All three can be linked via transitional models. The paratroopers discover the variables for the models; the artillery, the functional interrelations; and the occupying forces found theory on the complex models which, after many iterations, have become stabilized replicas of reality. This, I see, provides a considerably different approach to relating basic science to applied technology.

It is not that we do not have many social technologists. It is that we have been unwilling or unable to connect what they do and what they learn (or think they learn) with what we social scientists should be doing if we seriously want to ultimately understand the realities of social science. We particularly need to become aware of the different criteria of success that govern much of the social craftsman's efforts compared to those of the social scientist (Coleman, 1972).

We must recognize the need for some of us to pursue technological efforts from which learning can accrue and be made public. Mastery of the unknown concerning a behavioral phenomenon, particularly one likely to be sensitive to the con-

textual situation in which it was embedded, would grow from iterative trials leading to a better and better model for specifying how to predict and control the phenomenon. Thus we need a much strengthened technology for search and delineation of behavioral problems, for behavioral modification, and for evaluation of the effects of social engineering. Laboratory models can only provide suggestions to stimulate our search and inventive process.

It should be clear that at the core of behavior change lies some applicable principles like operant conditioning. But one might argue that Skinnerian science is good mainly because it has initiated the establishment of a reliable technology and bad to the degree it has diverted attention and effort away from developing alternative technologies which derive from other theoretical perspectives.

Instead of expecting and hoping to move from laboratory-based experimentation into social engineering, we see applied problem solving and basic and pure research moving along side by side. I argue for equal rights for problem-oriented paratroopers, not because what they do is more relevant but because what they must do is as important to the advancement of the science as the theory construction and theory testing engaged in by the occupying troops. It is interesting to note that the first time we made some lasting progress in the psychological study of supervisory leadership was when C. L. Shartle (1952) and his Personnel Research Board at Ohio State in 1945 began pushing the need to study systematically as a job analyst what real supervisors do. Many factor analytic studies by Hemphill, Stogdill, Halpin, and Fleishman lay between the 500 or more descriptive items originally collected and the two-factor 44-item Leader Behavior Description Questionnaire (Fleishman, 1953). (By the way, we now know it is time to go back and look at many of the areas and items which got lost in the distillation process [Bass & Valenzi, 1973; Miller, 1973].)

Instead of diverting the time, effort, and resources of applied social science with hasty systematization we must concentrate on fostering the discovery of the myriad of phenomena "out there" sensed "in here" that are of consequence to us. More of us need to concentrate on observing, thinking, and articulating what we see around us to contribute to the health and well-being of our discipline. Good models are still needed, but for applied psychology they must be couched in terms of real-world observables and must be subject to correction and modification as they are tested in the real world.

It is true that we need to develop umbrella-like connections between constructs of consequence as well as several different approaches to indirectly sensing or observing the construct. But why should these several approaches not be the subject of careful standardization subsequently to be used by all investigators interested in the particular area of study in which the construct is located, as Barrett (1972) has suggested. Current concerns for theory building and hypothesis testing might well await better measurements and agreement on the real-world facts about which we are trying to theorize. Thus, Glaser (1973) suggested that a good introduction for students interested in the psychology of training and development would be to identify programs of instruction that have been found to work well, i.e., that are technologically sound. Differentiating these from programs that have failed to work might provide a better basis for furthering study and application of principles which might be abstracted. Glaser did not deny the importance of undirected basic research but he also placed importance on the

> intuitive design of educational practices by outstanding teachers. Good practice has an artistry and intuition which must not be restricted and which may far outrun any momentary attempts at definitive understanding and analysis. Ideally, our job as educational psychologists is to work within these two extremes, contributing to knowledge and practice, and trying to understand both without inhibiting either [p. 557].

It is in this interactive mode that Glaser sees many individual researchers now working on such topics as subject-matter learning, the teaching of basic abilities and aptitudes, adaptation to individual differences, and testing and measurement.

Admittedly, there is a danger here of relying only on raw empiricism. We may not understand why a particular training program works. We then will be tempted to apply it across the board to many situations in which it will not work and then, in an attempt to explain the preordained failures which result, churn out complicated, after-the-fact "theories" which duplicate the superstition paradigm. Thus, in the application of behavior modification, the faithful technologist may feel it is necessary to follow the techniques devised with pigeons when he deals with humans who have a much richer stimulus–response repertoire. For example, he may:

spend countless hours patiently shaping behavior bit by bit when much of this tedious process can be drastically reduced . . . through graduated modeling combined with positive reinforcement for matching responses [Bandura, 1969, p. 233].

BE OPEN TO MORE ISSUES AT THE INTERFACE OF SCIENCE AND TECHNOLOGY

We ought to direct particular attention to problems that lie at the interfaces of what we have heretofore dealt with in more narrowly confining ways (Bass, 1968). For example, as behavioral scientists we may decide that instead of one more technological analysis of the reliability and validity of selection boards in comparison to the reliability and validity of bosses' appraisals in bureaucracies for assessing management potential, we would focus on the differential effects of selection boards and bosses' appraisals on managerial performance. Is less toadying done if one is beholden to a board of superiors for one's promotion, such as in the state department or the military, than if one is beholden only to one's own boss for promotion, such as in civil service? How does this, in turn, affect the reliability and validity of board decisions and bosses' appraisals?

Perhaps it is this greater openness to issues, as well as shorter circuitry requirements for information links between the real world of action and the disciplinary world, that accounted for Gordon and Marquis' (1972) finding that medical sociologists working in academically marginal settings (health agencies, hospitals, or medical schools) were three times as likely as their higher status university colleagues to produce extremely innovative studies (Starbuck, 1972).

MAKE MORE USE OF TECHNICAL ASSIGNMENTS

Technology can be better incorporated in our science if we do more to make use of engineering, consulting, or technical assignments as a location for learning that ultimately may lead indirectly to scientific advances.

Rather than seeing these technical assignments only as low-grade services unlikely to ever contribute to science, we should regard these as opportunities to tinker, to conduct quasi experiments, to experience, to wallow around in the muck of the real-world complexities to give us insights which we may later find useful to bring under suitable control.

CONSIDER THE RELATIVE STATE OF THE SCIENCE AND THE TECHNOLOGY

There is a time for scientific theorizing in an area of study. There is a time for technological advancement. For instance, London (1972) ably summed up the current need in behavioral modification study for concentration on technological rather than theoretical efforts. He saw as needed

the systematic exploration of all kinds of therapeutic things without inhibition or concern as to whether they fit ostensible principles of learning, or reinforcement or whatever, but with a single focus on whether they fit the facts of human experience. . . . We have gotten about as much mileage as we are going to get out of old principles, even correct ones, but we have hardly begun to work the new technology [p. 919].

The psychologist concerned about understanding real industrial/organizational problems should ask first if available models and concern for experimental verification of these help or hinder his efforts to search and discover optimum solutions to his problems. He then should proceed with a complete problem-solving strategy of search and invention within cost constraints to test the extent his solutions solve the problem. The "establishment" of journal editorial boards, graduate schools, and granting agencies should support these efforts as completely as they support model building and testing. There is some optimum mix depending on the state of the area of inquiry. We need to increase extensively our knowledge of what in fact are optimum shaping strategies for given problem situations and how to aid in designing optimum programs for doing so.

Again, for organizational/industrial psychology as a whole, at this time I think we need technological invention, study, and examination as well as theory construction and controlled experiments to verify or refute theoretical models.

CHANGE STATISTICAL USAGES

Transitional modeling suggests changing the character of our statistical testing. C. E. Bennett (personal communication, April 1, 1973) noted that we tend to use statistics like a drunk uses a lamppost: for support, not illumination. To sober up, we need to start thinking more in the Bayesian approach and less in the classical Neyman-Pearson approach to the use of statistics. That is, supposedly verifying the fleeting appearance of a phenomenon at the 5% level will not advance the

science much. But deciding whether to change an element in a model will. Statistical analysis ought to be seen as more of an aid in decision making rather than as a way of verifying some fundamental theoretical position.

The difference between the classical Pearson-Neyman use of statistics and the Bayesian approach lies in both the exploration and confirmation of hypotheses: In the classical approach, previous experimentation is used to design the new experiment but one's conclusions strictly depend on the extent the new experiment departs from a particular model of randomness. In the Bayesian approach, the data found in exploration are used along with subjective information to make explicit one's expectations. It is against this accumulated knowledge that confirmatory tests are run.

A paradox lies in hypothesis testing itself. According to Bakan (1966) our archives are being filled with false information. He reasoned as follows: Consider the case where the null hypothesis is actually true, that is, no difference really exists between two treatments. One hundred psychologists run experiments to reject the null hypothesis. Of these, 5% succeed and 95% fail. Who publishes? The 5% who rejected the null hypothesis submit manuscripts which are accepted by the journal editorial establishment. The journal duly publishes five independent studies rejecting the null hypothesis when truly there was no difference to report (Bakan, 1966).

Some serious rethinking is posed by Meehl's (1967) paradox noted earlier and related malpractices in behavioral science. In private communication (February 27, 1973), Meehl indicated that he is looking at how to deal positively with five malpractices he noted (Meehl, 1967) of behavioral scientists: (*a*) equating confirmation with refutation of theories (equating rejecting the null hypothesis at the 1% level of confidence with supporting the theory derived from the hypothesis at the 99% level of confidence); (*b*) ignoring the true weakness of statistical tests for the purpose; (*c*) exploiting a posteriori explanations of failures; (*d*) introducing auxiliary assumptions to account for failures; and (*e*) challenging of a succession of after-the-fact hypotheses, occupying a position of a "potent-but-sterile intellectual rake, who leaves in his merry path a long train of ravished maidens but no viable scientific offspring [Meehl, 1967, p. 33]."

To these malpractices could be added another: locking ourselves into the use of the wrong statistics to treat hypotheses that have emerged from our informal theorization. For instance, we propose multiplicative formulations and then use analyses of variance which are clearly additive in all their conceptualizations.

INCREASE THE SPECIFICITY AND FIDELITY (THE REAL-LIFE CHARACTER) OF VARIABLES

The three-stage pattern for the growth and interlacing of social science and social technology suggests that we begin with studying specific problems in specific locations before moving too quickly into abstractions. Models with general applicability are more likely to develop out of such beginnings.

Dissatisfied with inconsistent results of job enlargement programs based on theoretical considerations, one large firm essentially returned to the beginning of the three-stage pattern. Currently it encourages self-planning by teams (see Bass, 1970) and studies the effects on a case-by-case follow-up to see what self-planning works and what does not. In short, instead of a push toward job enlargement with implications that everyone wants a bigger, more self-actualizing job that can be automatically provided, the approach is to make reorganization self-guided—and to learn better ways of doing it.

We can also draw into our experiments more faithful replicas of reality to manipulate. Hawkes (1972, 1973) described the design of basic research with hypotheses and variables drawn from real-life operational observations:

> It is possible to design good basic research with selection of parameters based on analysis of operational situations ("real life"), hypotheses on the functions involved, and laboratory studies on these functions. . . . In designing your research you can ensure better generalization to applied areas as we build the data base of psychology, if the environment outside the laboratory is viewed as a source of appropriate values for your variables [Hawkes, 1973, p. 269].

A good illustration is provided of how to increase the linkage between the field and the laboratory and the generalization of results to the field. Instead of using white noise as a background distractor in discrimination learning, traffic recordings were employed. Hawkes (1973) went on to cite three examples where this was done with profit: to study time perception, background noise, and ego involvement.

Another implication is that we need to begin to act on what Cartwright (1973) has so ably pointed out in his case study of research on the risky shift:

> After 10 years of [laboratory] research [and almost 200 journal articles], Stoner's original problem remains unsolved. We still do not know how the risk-taking behavior of "real-life" groups compares with that of individuals. . . . The answers to such questions are not much clearer now than they were 10 years ago [p. 231].

Both field and laboratory research are needed. It is with this reasoning that my colleagues and I have begun two major research efforts. In one effort, we have been engaged in looking at what can be done to make work more attractive to the worker. Using Smith and Kendall's (1963) allocation procedures we have been able to identify 37 conceptually different variables in the way work is done which are likely to affect worker satisfaction. We are now trying to develop suitable scaling procedures to put into the hands of job and organizational designers. From this primitive modeling stage should grow more complex models ultimately leading to the furtherance of theory as well. In tandem, we have constructed a laboratory simulation in which we will be able to manipulate some of these variables which we have located in our field studies.

In the same way, we are looking at management styles. We began with a primitive model, a long list of variables concerning the manager himself, his subordinates, the task, and the external and internal environments of the organization and the work group, which are known from laboratory and survey evidence to affect whether a manager is participative or directive. We have developed reliable measures of five conceptually distinguishable styles: direction, manipulation, delegation, consultation, and participation. We have constructed comprehensive survey instruments about organization, task, and person of consequence to leader behavior. At the same time we have created a parallel laboratory simulation to permit manipulation of many of the variables of consequence. Our aim is to develop a more complex model for which we also have the technical capabilities of measuring each of the variables of the model with reliability and validity. We intend to move back and forth between laboratory and field using the same real managers as our subjects (Bass & Valenzi, 1973).

A last implication concerns our own pedagogy. Many obvious inferences can be drawn from the preceding discussions. But several extensions can be offered here. Ed Henry was successful because he could talk the language of the managers of his day. Increasingly the managers and decision makers of tomorrow will be steeped in the technology of operations research and in Bayesian analyses which will force us to be explicit about our assumptions and value judgments. Our failures will be more readily seen.

Ultimately, many managers will reach the level of sophistication to deal with the messes, the Jamesian blooming, buzzing confusion they face from total systems rather than the clearly defined problems of the system subparts which can be solved mechanically (Ackoff, 1972). Such managers will be aware of the chimera represented by optimization, single objectives, and single solutions to complex messes. They will be trying to handle the total system as an adaptable organic whole with interrelated parts. Our students will need to be able to communicate effectively with such managers. Our curricula and some courses, at least, ought to emphasize delineation of ill-defined problems and messes as well as dealing with them as craftsmen and scientists.

Ed Henry worked hard to try to create psychological internships in industry. Some success has been achieved; more is needed. At the same time, opportunities need to be developed in the school situation for students to practice delineating problems, trying out techniques, and engaging in creative opportunities.

To illustrate how I try to promote this, each student in a training and development seminar I run determines a training need in an adult sample, designs a program to meet the need, administers it, and evaluates it, presenting reports on why he used the methods he did and what alternatives based on literature review and experience he could have tried but did not. At the end of the course, students hopefully have increased their technical competence as training program designers, at the same time as they have become aware conceptually of the classes of variables to which they had to attend at each stage of the game. Stimulus conditions, behavioral repertoires, and feedback loops are concretized. Specific models are emphasized rather than global theories. The phenomena of

consequence are seen, probed, documented and manipulated.

Conclusion

What have I tried to say here? First, I think we need to delay locking ourselves prematurely into complex theorizing, defining, and hypothesis testing before we have adequately, fully, and openly looked at the problem to which we address ourselves. This does not mean raw empiricism and a rejection of theory. It means theory commensurate with where we are in the process of studying our problem.

Second, I think that as much as possible we need to look at real-world data for the source of our problems and to bring into the experiment data that we solidly connect to the real world. This will enable us to seek valid generalizations in real-world operations of what we can accomplish in controlled experimental settings.

Third, models and theories that do not yield tests of their revision hinder understanding of a problem more than they help.

Fourth, industrial/organizational psychologists should play a crucial role in advancing the science of psychology if they can straddle or work on teams that straddle from operations to laboratory in promoting the understanding of problems of human abilities, motivations, and interpersonal behavior.

Applied psychologists are men in the middle. Some of our colleagues in more general areas see us as soft, inexact, and muddleheaded; the laymen who "meet payrolls" with whom we must deal see us as hard but impractical, academic theoreticians grasping for the shadows rather than the substance. We need to learn how to do both.

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Culture and Achievement Motivation

MARTIN L. MAEHR *University of Illinois, Urbana–Champaign* [1]

The study of culture and behavior has regularly prompted a concern with variation in achievement motivation. Whether the focus is on growth and change in national groups, the characteristics of minority subcultures, or the behavior of individuals, it is widely assumed that achievement is not just a function of capacity, opportunity, or good fortune. Rather, it is believed that "where there is a will there is a way," and therewith achievement motivation has emerged as a popular explanatory concept.

This concern has been epitomized in the work of David McClelland (1961, 1965a, 1965b, 1971a, 1971b, 1972; McClelland, Atkinson, Clark, & Lowell, 1953; McClelland & Winter, 1969); indeed, no one is more closely identified with the study of culture and achievement than he. But McClelland's work typifies what may well be a critical shortcoming in the research thus far. Based on a conceptualization of achievement motivation in which stress is placed on learned inner drives and culturally derived personality patterns, his work has given limited attention to the situational contexts that affect achievement. The possibility that achievement may take a variety of forms and be pursued to differing ends has really not been fully explored. Further, it has perhaps been too readily concluded that certain cultural groups are lacking in motivation as far as achievement is concerned. Just as recent work in linguistics and cognition (e.g., see Baratz & Baratz, 1970; Bernstein, 1970; Cole, 1972; Cole & Bruner, 1971; Ginsberg, 1972; John, 1972; Maehr, 1974; Shuy, 1969) has raised serious questions about the cultural deprivation assumptions implicit in the study of intellectual development among some cultural groups, so a similar line of questioning may be desirable with respect to motivation.

In any case, this article suggests a framework for the cross-cultural study of motivation that stresses the importance of contextual conditions in eliciting achievement motivation and emphasizes cultural relativity in the definition of the concept.

The Nature of Achievement Motivation

In general, the term *motivation* has referred, in the first instance, to inner states or processes of the organism—needs, drives, etc.—which prompt and guide behavior. Concepts of achievement motivation have likewise laid considerable stress on the workings of inner dynamics. However, it may be profitable, initially at least, to ignore these inner workings and to concentrate specifically and directly on the behavior that is taken as indicative of motivation. At the least, such an approach should probably yield less disagreement as to the object of consideration. More importantly, this approach will suggest what amounts to a different framework for the study of motivation.

The study of motivation, then, might well begin with a taxonomy of *behavioral patterns* that are typically placed under the rubric "motivation." Such a taxonomy of behavioral patterns which characteristically prompt motivational inferences would most certainly include three readily identifiable routines: change in direction (choice), persistence, and variation in performance when capacity is in some sense controlled. The study of motivation could profitably begin with these more or less readily identifiable behaviors rather than with postulates regarding inner states, psychic energies, predispositions, predilections, etc. These are the basic data, and quite possibly the study of motivation ought to be candidly defined as the study of certain specifiable *behavioral* routines, such as persistence, change in direction, and selected cases of variation in performance. But regardless

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Requests for reprints should be sent to Martin L. Maehr, College of Education, University of Illinois at Urbana–Champaign, Urbana, Illinois 61801.

of how motivation is generally defined, this is the way it will be defined here.

Given this definition of motivation, the business of motivational theory is basically one of developing effective schemes for determining when and under what conditions persistence, change in direction (choice), or variation in performance will occur. Three strategies can be followed. In an important sense they are overlapping strategies, and one strategy cannot be considered exclusive of the other two. In the present instance, however, it is helpful to define these strategies separately.

1. A first strategy may be expressed diagrammatically as follows:

$$C \longrightarrow P \longrightarrow M,$$

where C = "culture," or, more specifically, the social learning experiences provided by the milieu in which the person develops; P = "personality," or some assumed predispositions to respond in a given manner; M = observed tendency to exhibit in a variety of situations that behavior termed *motivation*.

2. A second strategy may be diagrammed as follows:

$$S \longrightarrow (P) \longrightarrow M,$$

where S = a situation or context that has demonstrable effects on motivation; (P) = "personality," but placed in parentheses ( ) it refers to the relative unimportance of this variable in the present scheme. That is, presumably the interest is in this aspect of S irrespective of personalities; M = an observed behavioral pattern.

3. A third strategy in some sense represents a combination of the two foregoing strategies. It may be diagrammed as follows:

$$C \longrightarrow P \longleftrightarrow S = M$$

Briefly, it is assumed here that social learning (C) does eventuate in certain personality predispositions (P) and that these patterns eventuate in different motivational behavior (M) depending on the situation or context (S).

When the term *achievement* motivation is employed, study is then restricted to certain conditions under which these behavioral patterns occur. More specifically, achievement motivation refers first of all to behavior that occurs in reference to a *standard of excellence* and thus can be evaluated in terms of success and failure. A second defining condition is that the individual must in some sense be *responsible for the outcome.* Third, there is some level of *challenge* and, therewith, some sense of uncertainty involved.

Since this approach to defining achievement motivation is so self-evidently in debt to the McClelland tradition, it may be well to take cognizance of some discrepancies from this tradition and to show the importance of these discrepancies insofar as the cross-cultural study of achievement is concerned.

First, it may be noted that by reducing achievement motivation to more or less simple behavioral terms, some of the more attractive, imaginative, and exciting facets of achievement theory have been lost. Operationally defined and situationally limited, persistence, choice, and performance patterns are hardly as fascinating as McClelland's (1961) ubiquitous "Spirit of Hermes." But there are some distinct advantages in backing away from a more wholistic, comprehensive, and also imaginative approach to certain more limited but manageable specifications. Such specification more readily allows for the close situational analysis of the contexts that can and do elicit achievement behavior. It is, further, of special importance to the practitioner who is in the business of creating achievement on the spot. Perhaps of greater importance and interest is the fact that in defining motivation in this manner it has been defined as a phenomenon extant among all peoples and groups. Persistence, choice, and variation in performance are certainly not the sole property of persons in any given culture; they are universal behavioral patterns. Similarly, the achievement-defining conditions stated above can likewise be found in all cultures. Achievement motivation, as here defined, is assumed to be universal. Moreover, there is no necessary reason to believe that it exists to a greater or lesser degree in one or another culture. There is reason to suspect that it may well be elicited by different contexts and directed to different ends. In any case, this definition of achievement motivation seems to do more than shuffle concepts around. It establishes an entirely different orientation toward cross-cultural research.

Strategies in Predicting Achievement Motivation

STRATEGY 1: $C \longrightarrow P \longrightarrow M$

The first strategy relates to the role of "personality" in motivation. Since different cultures do provide different contexts for social learning, it is likely that members of these cultures will hold

varying orientations toward achievement and that such orientations will determine when and how achievement motivation will be exhibited. However, several questions must necessarily be raised regarding the use of this strategy.

First, it may be noted that the achievement of cross-cultural comparability in personality assessment is no mean problem. This is no less true in the case of the fantasy-based measures that have become the mainstay of achievement motivation methodology. Aside from reservations that one might have about such measures generally (see Entwisle, 1972; Klinger, 1966, 1967; Weinstein, 1969), cross-cultural research compounds the problem. It is difficult, if not impossible, to make the cues used to elicit achievement imagery comparable across cultures. For example, are measures of achievement motivation based on fantasy responses to pictures designed for blacks (see Mingione, 1965, 1968) equivalent to those normally used with whites? Are pictorial cues suggesting males in an achievement context fair to females, or vice versa? (See, e.g., French & Lesser 1964; Lesser, Krawitz, & Packard, 1963.) And will such differences between sexes in any given culture exist in a comparable fashion between cultures? (See Heckhausen, 1967, p. 17 ff.) Pictures are a part of a culture as much as words (see Cole, 1972; Maehr, 1974; Segall, Campbell, & Herskovits, 1960). Recognizing such difficulties, some researchers (e.g., Horner, 1968) have employed verbal cues in eliciting achievement imagery. However this may reduce the comparability problem in cross-cultural research, it hardly disposes of it altogether.

Related to such assessment problems, there is a more basic conceptual problem. McClelland, like others concerned with personality and achievement, has given minimal attention to distinctions between the motive to achieve and the ways in which this motive might be actualized—dependent on culturally approved means and ends. Rather, the achievement motivation literature emphasizes that achievement depends on a generalized desire to achieve and, at least by implication, minimizes the importance of also knowing and valuing the appropriate instrumental behaviors. Culturally derived beliefs about ends (terminal values) and means (instrumental values) typically play little or no significant role in conceptualizing achievement motivation. Moreover, the personal desire or inclination to achieve is usually confounded with the goal to achieve in such a way that little consideration is given to the possibility that one might exhibit all or most of the basic behavior supposedly characteristic of highly motivated persons, only directed to different ends—ends that are appropriate in one, but not another, culture.

The problem is not unimportant as far as a cross-cultural study of achievement is concerned, and it is critical, for example, to an educationist concerned with so-called "culturally deprived" children. According to the usual way of conceptualizing and assessing achievement motivation (n Ach), the lower-class black child comes out on the short end. Those who have read about and conducted game-like experiments purporting to provide some validation for the construct n Ach and have also observed so-called "disadvantaged" children at play or at work in their own settings may be a bit suspicious here. There seems to be a lot of n Ach behavior on basketball courts in black ghettos, even though the participants are presumably low in this particular personality trait. The counterargument, of course, is that such instances are probably not sufficiently general across a wide variety of situations and the high n Ach person is one who exhibits such behavior typically and characteristically, not just in a few specific and identifiable instances (see McClelland et al., 1953). But the problem may actually be that current assessment procedures are so culture bound that they only sample instances of achievement motivation associated with a given culture. Granted that such assessment may provide the best basis for predicting achievement in this culture, is it a fitting basis for assuming that the individual from an alternate culture has a limited will or desire to achieve?

Additionally, Strategy 1 poses problems insofar as intervention is concerned. Essentially, this strategy implies that achievement motivation can be increased only as the personal history of the individual is in some sense reversed: Changes in achievement motivation involve personality change. In order to increase the achievement motivation of the ghetto black, the Mexican-American, or the Iranian peasant, a kind of psychotherapy is necessitated in which a basic personality transformation can occur. While it has been demonstrated that this type of intervention is possible (Alschuler, Tabor, & McIntyre, 1970; McClelland & Winter, 1969), it is also problematic. One might, then, question just how practicable this intervention strategy actually is. Certainly, there have been and there will continue to be instances when se-

lected individuals will submit themselves to the rigors of such personality change, but what suggestions does it hold for the more typical situation? The teacher in an inner-city high school or the employer of so-called disadvantaged persons will likely derive little but a fatalistic outlook from such suggestions, for after all, what can they do about lack of motivation when it is simply in the nature of the person?

Finally, then, this strategy must be considered in developing a complete understanding of achievement motivation. It can hardly be denied that social learning plays some role in creating personal dispositions. However, just as clearly, the time is ripe for a certain redirecting of activity even when operating in a culture and personality mode. In addition to taking value assessment more seriously, traditional measures of achievement motivation must be viewed more critically. Conceivably, it would be profitable to define personality more explicitly in terms of the culture in which the person exists. Possibly also, the most cross-culturally valid definition of high- and low-achievement-motivated persons must be based on the structure of social meanings an individual holds—the subjective side of culture (cf. Saral & Crotts, 1971).

STRATEGY 2: S ——→ (P) ——→ M

There is an extensive literature that deals with the role of situations in affecting achievement motivation. By and large, this literature argues strongly for the conclusion that situational variables play a major, and in many cases *the* major, role in determining behavioral patterns that are labeled achievement motivation. Since different cultures characteristically create different situations as well as adapt to general situations in idiosyncratic ways, arguments underscoring the importance of situational variables increase in importance as one attempts cross-cultural study. This literature is worthy of closer scrutiny than it has sometimes had by those concerned with the nature and sociocultural origins of achievement motivation. Although it is impossible at this point to incorporate the many and diverse findings into a single, nicely integrated, theoretical statement, it is possible to identify dimensions of situations that seem to be critical in determining achievement motivation.

The normative dimension. At the outset it should be noted that an individual does not achieve in a social vacuum. He achieves as a member of a social group, and in such behavior as choosing and persisting he is influenced by the guidelines, expectations, and values of the groups that are significant to him. There is much that has been said on this point, but less has been done in systematically analyzing such normative influences on motivational behavior. Recently, however, several lines of research have proved particularly interesting. The first line of research has involved a role analysis of achievement behavior. Klinger and McNelley (1969) reviewed considerable literature that supports the hypothesis that role changes and movement in the status system affect achievement motivation. A notable study here is one conducted by Zander and Forward (1968), in which subjects identified as high and low in n Ach following usual procedures assumed leader and follower roles in a three-person group performance experiment. While in the follower position, high and low n Ach subjects did differ in the behavior they exhibited—much along lines that one would predict on the basis of the person-based achievement motivation theory associated with McClelland's group. However, when placed in a leadership role, both high and low n Ach subjects exhibited high n Ach behavior. What is particularly intriguing about this is that such a momentary switch in social roles can have such immediate and direct effects on motivational behavior—motivational behavior that is typically attributed to enduring personal disposition.

Recently, Stuempfig (1973) established a research paradigm for systematically analyzing the effects of status changes on achievement motivation. The point in question here is hardly trivial insofar as the practitioner-teacher, employer, or recreational leader is concerned. At the least it may suggest how variable and flexible these presumed personality characteristics may be (see Hunt, 1969). It may also suggest that, in fact, these basic achievement behaviors are routines or behavioral patterns which are normally developed in most persons through the course of socialization, ready to be elicited under the "right conditions." Conceivably, then, achievement motivation is universal, perhaps also evenly distributed, and it is only a question of establishing the appropriate cues or context for eliciting these behaviors.

For the educator, a second line of research is of special interest—that concerned with so-called "teacher expectancy effects" (Rosenthal & Jacobson, 1968). Briefly reviewed, there is a growing amount of evidence that teachers "slot" children into certain categories, much as people filling differ-

ent social roles are slotted. Once slotted, a child is expected to perform in one or another way and does—fulfilling, as it were, a kind of prophecy. Recent work by Rubovits and Maehr (1971, 1973) has demonstrated how teachers behave toward differentially slotted students. While the amount of feedback distributed among children labeled "gifted" or "average" did not vary, the quality of feedback did. "Gifted" children were given the approval and praise which in other contexts had been shown to increase self-regard (Haas & Maehr, 1965; Ludwig & Maehr, 1967; Maehr, Mensing, & Nafzger, 1962), perhaps by changing attributional patterns (Dweck & Reppucci, 1973; Weiner & Kukla, 1970). In any case, the expectations that elicit conformity to roles and norms indirectly affect achievement by eventuating in the approval/disapproval patterns that create the persistence, choice, and performance patterns that give rise to accomplishment.

A third line of research focuses more directly on the cognitive aspects of roles and norms. One not only follows a given role pattern because of the prospect of certain rewards or sanctions but also follows those patterns of which he has some knowledge and which have salience within the cultural context he knows. The analysis of "subjective culture" (Triandis et al., 1972) provides a possible entrée in identifying which behavioral patterns make sense to a member of a given culture. Persistence on the basketball court may be the perceived antecedent to success and achievement in one culture, but not in another. Whether or not an individual has knowledge of "what really leads to what" in an objective sense, he will probably only receive encouragement from significant others if and when the appropriate antecedent–consequent patterns are held. Knowledge and valuing of appropriate patterns are critical in establishing achievement patterns.

Other lines of research could be cited in illustrating the importance of culturally established roles and norms in affecting what is ultimately labeled as achievement motivation, but perhaps the point has already been made. In one sense, it is an obvious point. But obvious as it is, it is surprising that it is not taken more seriously and employed more pervasively in the systematic analysis of achievement motivation.

Locus of control dimension. A second dimension that emerges in the analysis of achievement situations might be termed the *locus of control dimension.* The previously stated definition of achievement clearly assumes that the achievement context must permit the achiever to be, in some sense, in control of the situation and responsible for the outcome. However, the fact remains that situations can and do vary in the degree to which such control is transferred to the achiever. Achievers may, in varying degrees, be engaging in a behavior of their own choosing, pursued for ends they themselves designate.

Currently, there is considerable interest in the role of locus of control and attributional processes in achievement motivation (Crandall & Battle, 1970; Crandall, Katkovsky, & Preston, 1960; Gurin, Gurin, Lao, & Beattie, 1969; Weiner, 1972a, 1972b, 1974; Weiner & Kukla, 1970; Weiner, Heckhausen, Meyer, & Cook, 1972; Weiner et al., 1971). This work has progressed beyond personality analysis to show how certain contextual conditions create the "illusion" of control (Lefcourt, 1973) and thereby drastically affect achievement motivation. DeCharms (1968, 1972) has shown that those contextual conditions that communicate to persons that they are "origins" rather than "pawns" will increase their achievement motivation. Similarly, Maehr (1967, 1969; Maehr & Stallings, 1972) has pointed out that freedom from external control in learning may encourage a continuing interest and a greater willingness to engage in challenging tasks.

Quite obviously, locus of control is a critical contextual variable. It seems to have some definite and specific effects on how persons view their capacity to achieve as well as having observable effects on achievement behavior per se.

Interpersonal dimension. A third dimension to be considered in the analysis of achievement contexts deals with interpersonal variables. In this regard, one should first of all take cognizance of the considerable literature on the "quality" of feedback on performance. For example, it seems that whether an individual is simply provided information on his performance or given personal feedback from a significant other may make a difference. Moreover, the effects of personal feedback are further affected by the relationship that the performer has with the significant other (Katz, Henchy, & Allen, 1968; Stuempfig & Maehr, 1970). Persistence, choice patterns, and performance seem to be significantly influenced by the way evaluative information is communicated to the achiever. Such information can be made to be or in fact is a social

event having varying influence, dependent on its meaning to the performer.

There is, however, another consideration to be noted: the organization of achievement in an interpersonal context. Achievement situations can be defined as "self-competitive" or "other competitive" (Maehr & Sjogren, 1971). On the one hand, the standard of excellence imposed on or associated with the situation may be socially normative, involving comparison or competition with others. On the other hand, it may be structured so as to involve comparison or competition with idiosyncratically defined standards. And, of course, there are a variety of ways to subdivide further, but the point to be stressed is that social competition may be more or less involved in any given achievement situation. This possibility has been insufficiently considered in the achievement literature. Atkinson's (Atkinson & Feather, 1966) model of achievement motivation seems to be primarily, if not exclusively, valid given a socially competitive situation. It was structured in terms of socially competitive games and may be restricted to such contexts insofar as its explanatory and predictive value is concerned (Maehr & Sjogren, 1971). Although Veroff (1969) made an important attempt to consider the role of social competition developmentally, little attempt has been made to understand self- and socially competitive patterns within a broad array of cultural contexts. While the Navajo child may not exhibit motivation to achieve in a socially competitive context, will he perhaps demonstrate such motivation when interpersonal conditions are so designed to fit more appropriately with patterns acceptable within his culture?

Task dimensions. Finally, one must take into account various task factors. Not only does this involve a consideration of levels of difficulty, already thoroughly dealt with in the achievement literature (e.g., Atkinson & Feather, 1966) but also the identification of "intrinsic" task interest (Day & Berlyne, 1971). Above and beyond that, however, it is clear that there is a necessity to attempt to scale tasks in terms of some sort of cultural relevance dimension. Such scaling would certainly involve a consideration of facets of achievement associated with several of the previously designated dimensions. Thus, it would seem most logical to consider tasks in terms of the normative factors previously mentioned. To what extent is a given task perceived as an appropriate antecedent to a desired consequent? As a parent, I am readily aware of the fact that my sons exhibit considerable motivation to read above and beyond the requirements of the context in which reading is formally an achievement task. I am also aware that they may not be typical of all children. Reading has a special place in a professor's family; some other behaviors do not. It is, therefore, surprising only when my children refuse to read, not when they choose to do it. This is a homey and oversimplified example of cultural relevance of a task, but perhaps it makes the point.

Summary. Situations do determine that behavior which is presumed to be indicative of achievement motivation. Moreover, a situational and contextual analysis of achievement motivation is likely to provide a fuller understanding of achievement-related behavior as it varies from culture to culture. Without such situational analysis it is all too easy to assume "motivational inferiority" in the case of those who achieve under different times and circumstances than we.

STRATEGY 3: C $\longrightarrow$ P $\longleftrightarrow$ S = M

The third strategy in some sense represents the most sophisticated analysis of achievement motivation. This strategy specifically identifies both personality and situation as simultaneously critical variables. Taking their cue from Lewin, a number of P × S models of achievement motivation have emerged. Perhaps the most prominent and highly developed of these is the one proposed by Atkinson (Atkinson & Feather, 1966). Although the model initially seemed limited in terms of its applicability to a variety of situations and cultural contexts, recent adjustments (e.g., Atkinson & Raynor, 1974) have improved matters. Without delving into the specific merits or problems of the Atkinson model per se, consider the workability of the general P × S strategy implicit in this approach as it relates to the analysis of behavior in different cultural contexts.[2]

Basically, this strategy puts considerable emphasis on how certain situational conditions will elicit responses dependent on previously acquired predispositions. Anyone who has conducted research on the influence of situational factors on motivation has probably noted the existence of individual

[2] Critical analysis and reviews are sufficiently available on this point. See, for example, Maehr and Sjogren (1971) and Weiner (1972).

differences in response to the treatments (e.g., Maehr, 1974; Maehr & Stallings, 1972). This in itself argues for a consideration of a P × S interaction within any complete statement of motivational theory. But there is a special reason for giving consideration to this strategy in varying cultural contexts, especially if one is ultimately interested in practical questions. Taking this strategy into the practical realm suggests a matching models approach (e.g., see Hunt, 1971; Stuempfig, in press) in eliciting desired behavior. If, for example, one wishes to design a recreational program for children, this strategy suggests that one must know something about the predispositions of these children. Simultaneously, something must be known about the effects of certain situations on these children in order that situation and person may be matched to achieve desired ends. As an example, some children may be motivated by highly structured play, by challenging tasks, or by competition with others, and others may not; the trick is to find out who is motivated by what. Moving from one culture to another, this or that situation may elicit the desired behavior depending on the culturally defined predispositions of the children in question.

In an important sense, this third strategy actually represents the ultimate goal of the search. Even in stressing the importance of Strategy 2 it has been implied that the ultimate fruition would be the identification of situations that work for different persons, whether persons are identified on the basis of personality tests or ethnography. However, some of the available P × S models are manifestly ineffective for these purposes, precisely because concepts of both situation and person have developed and evolved in very limited cultural contexts. The P variable in Atkinson's model, for example, is subject to most of the criticisms made in focusing on McClelland in discussing Strategy 1. The two components of P, "tendency to approach success" and "tendency to avoid failure," are both rooted in and directed to achievement as it occurs in a specific culture. *Tendency to approach success* is assumed to be adequately indicated by the fantasy-based measures of achievement motivation discussed previously; *tendency to avoid failure* is presumably indicated by the Test Anxiety Questionnaire, an instrument manifestly focused on a very specific achievement behavior as it occurs in certain cultural contexts. Similarly, the situational variables employed within the model represent a limited sampling of the dimensions of situations. Probability of success and directly related incentive values are given primary, if not exclusive, consideration. Although some attempt has been made to incorporate other variables such as locus of control (e.g., see Feather, 1967) and instrumental value of the task (Raynor, 1969, 1970), the burden of a more expanded situational analysis of achievement has certainly not been relieved. It is questionable, then, whether it is indeed profitable to operate within the context of this model in the case of different cultural groups, perhaps tacking on a new variable (locus of control, fear of success, etc.) here or there as the results seem to suggest. Even with the ultimate goal of achieving a working P × S model for research, the present stage of knowledge regarding culture and person demands a fresh look at person and situation at those times and in those places in which achievement motivation, as a more cross-culturally general phenomenon, occurs.

Toward the Naturalistic Study of Achievement Motivation

The thrust of the arguments thus far culminates in a plea for a "culturally based" (Brown et al., 1973) conception of achievement motivation derived from a contextual and situational analysis of achievement behavior. Quite possibly, they further suggest that we complement our experimentation with the naturalistic study of achievement motivation.

The argument for a situational analysis of achievement motivation has thus far been primarily buttressed by references to research conducted within the realm of experimental social psychology. A more telling argument could perhaps be made on the basis of naturalistic studies. As Labov (1970) pointed out, it is easy enough to assume that the black, the Chicano, or some other minority group member is deficient in language development if one only considers classroom conditions or observes these individuals in the context of a standard psychological experiment. But the highly developed language behavior that is not exhibited in the classroom may be elicited in the street, in the living room, or in the pool hall. Similarly, the uneducated African cowherd may exhibit learning and memory deficits on the standard tasks of Euro-American psychologists. However, in some contexts he possesses a remarkable memory for certain facts and feats, recalling readily the intricate details of cattle

transactions over many years. And if stimuli are presented in ways and contexts that are compatible with his culture, one finds little evidence of impoverished learning capacity (Cole, 1972). Quite possibly, similar principles are also operative in the case of motivational behavior. The achievement motivation that is not found among the so-called culturally disadvantaged (Rosen, 1959) may not have been found because it was not sought in the right places.

In the first instance this conclusion is a logical development from the previous arguments in relation to the importance of situations in determining achievement motivation. It may be more than that. Putting the issue this way may suggest that we employ a research methodology different than we have typically employed in the study of motivation. In outline form, that methodology implies that we, first of all, identify settings in which achievement motivation, as defined previously, occurs or does not occur in various cultures and then proceed to characterize the nature of these settings. Additionally, it is assumed that certain choice, persistence, and performance patterns, which may be labeled achievement motivation, will occur in all cultures. Furthermore, it is assumed that naturalistic observation will readily reveal instances when these patterns are maximally and minimally exhibited in any given culture.

Much of the research that attempts to understand the motivational patterns of ethnic and cultural groups involves placing children in a "middle-class-biased" performance setting and then observing behavior. If this strategy is undertaken with the conscious goal of determining how one gets children to adapt to middle-class-biased performance settings, it makes some sense. If, however, one is willing to accept the notion that performance settings must in some sense adapt to children, it makes less sense. Indeed, given this latter inclination, it may make more sense to find out when children "turn on" and replicate those situations within the curriculum or program. At the very least, this procedure might provide a more culturally based conception of motivation. It would probably also yield the conclusion that achievement, as here defined, is not absent in certain cultural groups. It might even force the conclusion that it is equally present in all groups, although it is elicited in different ways and to different ends. But of greatest importance, it might provide fresh insight into the design of intervention strategies to the effect that in creating motivation sometimes it is better and more convenient to change the situation rather than the person.

Perhaps, then, we would do well to pursue an ethnographic approach to the study of motivation, particularly in work with varied cultural groups. Not that this approach should be pursued exclusively, nor should it be pursued in an atheoretical manner. As a matter of fact, it could quite appropriately be pursued in terms of the social-psychological dimensions of achievement behavior that were suggested earlier. The important principle is that achievement and achievement motivation must be understood in terms of the sociocultural context in which they are found, as well as in terms of generalized descriptions of achieving norms or abstract constructions of psychological processes.

Conclusion

Serious problems exist with what might be called a "dynamic analysis" of achievement motivation. A focus on the inner state of the person—his achievement *need*—has typically eventuated in an ethnocentric construction of the nature of things. While for certain purposes such ethnocentrism may have its place, it also has its distinct limitations. What is needed is a framework within which culturally based conceptions of achievement motivation can arise. This need not represent a total rejection of theory for simplistic empiricism. It does involve a clearer specification of the object of study and more serious devotion to the analysis of situations, contexts, places, and events in which this object of study is to be found. In this we may do well to follow the lead of Michael Cole and others (Cole, Gay, Glick, & Sharp, 1971; Cole & Scribner, 1974) who have created an experimental anthropology of cognition and create an experimental anthropology of motivation. At the very least, it is clear that if work is to be carried out effectively with persons of varied cultural backgrounds, it is well to know more than how they relate to the "Protestant ethic."

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Scientific Awards Announcement

The Committee on Scientific Awards is accepting nominations for its award program. The Committee selects up to three persons as recipients of the **Distinguished Scientific Contribution Award** who, in its opinion, have made the most distinguished theoretical or empirical contributions to scientific psychology in recent years.

A new award, the **Distinguished Contribution for Applications in Psychology**, has been authorized by the Board of Directors and will be given for the third time this year. This is presented to an individual who, in the Committee's opinion, has engaged in a program of research which is systematic and applied in character.

The awards are subject to the following limitations: (*a*) members of the Committee, former recipients of the awards, the President and the President-elect of the APA shall be ineligible. (*b*) The Committee shall seek diversity in selecting recipients, avoiding as far as possible the selection of more than one person representing a specialized topic, a specific material, a given method, or a particular application.

Names and appropriate information which will guide the Committee on Scientific Awards in conducting an intensive career review and evaluation should be forwarded to Office of Scientific Affairs, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. *Deadline for nominations is January 15, 1975.*

Progress in Psychology: 1903–1907

WILSE B. WEBB *University of Florida*[1]

Beginning with the February 1904 issue of the *Psychological Bulletin*, the journal's editor, Edward Franklin Buchner, of the University of Alabama, established a "department" in that journal called "Psychological Progress." For the next five years he published a review of the preceding year's psychological panorama (Buchner, 1904, 1905, 1906, 1907, 1908).

Fired with enthusiam for his task, he declared in his first article that

> particularly in the domain of science, readjustment, as demanded by any given situation, is the *sine qua non* of progress. . . . Effective readjustment in the interest of what ought to be can only be made in the light of what has been. Prevision requires revision; and revision requires retrospection [p. 57].

This article on Buchner's five reviews of psychology from 1903 through 1907 has far more modest goals. Its primary purpose is, to modify a contemporary phrase, "to tell it like it was"; to convey some sense of psychology 70 years ago. In doing this I hope that a retrospective view may demonstrate that our "progress," from then to now, may well fit another phrase from Buchner's introduction: "Progress may thus come to be actually a direct advance, a tangential detour, or even a definite retreat [p. 57]." I would add, "or perhaps only an appearance of progress."

I can find no simple way to summarize Buchner's nearly 50 pages of tightly packed summaries. I will first present an introductory quantitative analysis, based on data drawn from the articles, about the number of psychologists and their written record in order to set the stage for the subsequent statements about prominent people and issues of those times.

The Data

In his 1907 article, Buchner compared the production of psychological doctorates in American universities over the preceding nine years with other scientific disciplines: chemistry, 282 doctorates; physics, 133; zoology, 128; psychology, 124 (an average of about 12 per year). In the following year he reported a slight decrease in psychological doctorates to 10. For comparative purposes, according to 1973 National Research Council data, the universities of the United States produced the following number of doctorates in 1972 in the categories cited: chemistry, 2,204 doctorates; physics, 1,627; zoology, 354; psychology, 2,116. Hence, in relation to these fields, the number of psychology PhDs rose from 19% to 34% of the total. Buchner (1907) also reported that 4.85% of the men listed in American Men of Science for 1906 were identified as psychologists.

A second set of data reflect the interests of psychologists of that time through their publications. The *Psychological Index*, a predecessor of *Psychological Abstracts*, listed and categorized yearly the psychological literature being published worldwide. Buchner reported on these psychological publications for the years 1903 through 1906. The findings are shown in Table 1.

There is an uneven upward trend in the number of publications, with about one-third more articles in 1906 (3,145) than in 1903 (2,122). For comparative purposes we should note that two issues of *Psychological Abstracts* (1971 and 1972) contained 22,982 articles and 24,326 articles, respectively.

The last five categories of publication varied less than 1% across the four years. There are, however, informative shifts in the first five categories. I would roughly translate these categories as follows: "Higher manifestations of the mind [sic]" refers primarily to philosophical considerations. In fact, in the 1906 rubric, this category was specifically redesignated as "philosophical implications of psychology." The category "genetics, individual and social psychology" combines interests in developmental, individual differences, and social psychology and substantively reflects the interests of "functional," "dynamic," and applied psychology.

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[1] Requests for reprints should be sent to Wilse B. Webb, Department of Psychology, University of Florida, Gainesville, Florida, 32611.

TABLE 1

Percentages of the Distribution of Literature in the Psychological Index

Year							
1903		1904		1905		1906	
Topic	%	Topic	%	Topic	%	Topic	%
I	18	V	21	I	18	V	19
II	18	II	18	V	17	III	18
III	17	III	17	III	17	I	17
IV	13	I	13	II	16	II	12
V	10	IV	10	IV	10	IV	11
VI	8	VI	8	VI	8	VI	9
VII	6	VIII	6	VIII	5	VIII	5
VIII	5	VII	5	VII	5	VII	5
IX	3	IX	3	IX	3	IX	2
X	2	X	2	X	1	X	2
Total *N*	2,122		3,445		2,727		3,145

Note. I = higher manifestation of mind; II = genetics, individual and social psychology; III = sensation; IV = anatomy and physiology of the nervous system; V = sleep, trance, and pathology; VI = general; VII = cognition, VIII = conation and movement; IX = characters of consciousness; X = affection.

The categories "sensation," "anatomy and physiology of the nervous system," and "sleep, trance, and pathology," respectively, indicate primary interests in psychophysics, physiological psychology, and abnormal or "clinical" psychology.

Accepting these interpretations, the figures suggest a steady interest in psychophysics (a range from 17% to 18%), a relatively steady interest in physiological problems (a range from 13% to 10%), and a declining interest in genetics, individual differences, and social psychology (a range from 18% to 12%). The major fluctuations occur in the philosophical and clinical categories and reflect the ferment of the times (see below). Philosophical articles varied in rank from 1 to 4 and in percentage from 18% to 14% of the writings of that period. The clinical category rose from 10% and fifth rank in 1903 to 21% and first rank in 1904 and maintained its first or second rank in the following two years.

People, Issues, and Events

The following section includes a very select series of extracts from Buchner's five articles on certain key persons, issues, and events. The basis and bias of the selections have been an emphasis on individuals and trends that evoked, in this reviewer, a resonant contemporary response. There has been an almost complete omission of the reports of substantive experimental contributions. As a result, there have been omissions that different reviewers of different backgrounds and interests would have certainly included.

The extent of this bias and selectivity can be best shown by noting, for example, that the review of 1905 (Buchner, 1906) contained some 50 individual names. Of these 50 persons, only 7 are specifically referred to in the body of this article. Two paragraphs are devoted to aesthetics (Volkelt, Lipps, and Witasek) and two to the psychology of religion (Vorbrodt and King). Almost a page is given to the psychology of affection or feelings (Hall, Johnson, Ribot, Geiger, Lipps, Wundt, and Baldwin). One and one-half pages report on the specific experiments on vision and visual movements by Wallin, Baird, Judd, McAllister, Steele, Stratton, Dodge, Smith, and Moore. None of this rich trove is reflected below. Thus particularized interests of many readers are omitted. I can commend the basic articles to almost any special interests of today. I suspect that most "modern" concerns can find nascent or developed reflections of those interests in these five reviews.

ON BINET AND INTELLIGENCE TESTS

That the psychological world is not at peace with itself on this question of method also appears from a contribution of Binet to individual and child psychology [*L'étude expérimentale de l'intelligence*, 1903]. His study of young members of his family by a combination of "tests" and introspection was primarily designed as a means of heralding "a new movement" in method. This is to supersede the "Wundtian epoch," which has been physiological and statistical. In his reaction against persistent quantitative ideals, Binet demands that we pay attention to the individual traits of experimental subjects, and that study be made of the higher mental processes and not of the lower elements. Binet's position seems to be national. Shall France give us a new method of psychologizing? Toulouse, Vaschide and Pieron, in presenting the result of several years' work, from their own point of view, consider only the technique of "tests" by which the mental qualities of an individual are measured. Independently of practically all previous work done on "tests," they attempt to formulate a system of psychological tests which are to be standardized by pointing out the exactly reproducible conditions in measuring subjects [Buchner, 1906, p. 128].

ON TITCHENER

Neither of the above [referring to Binet] is to be compared with the permanent addition Titchener continues to make to the finished literature of experimentation as a definite and well-established mode of approach to psychological problems. . . . The best symptom of progress in the field open to experimentation is to be found in Professor Titchener's timely St. Louis address [*American*

Journal of Psychology, 1905] which became available near the beginning of the year. Looking both backward and forward, this unusually excellent review of progress, trenchantly stated in terms of present experimental ignorance, will remain for some time to come an open guide to our laboratories. His earnest plea for more thorough testing of available methods and renewed research over much of the old ground is already being realized [Buchner, 1906, pp. 128–129].

The contributive systematic work [of] Professor Titchener continues in the better pedagogy of experimentation [Buchner, 1905, p. 95].

Surprisingly, these appear to be the only references to Titchener during the five years.

ON WILLIAM JAMES

The strong influence of William James is seen in his appearance in all four years of substantive review (Buchner, 1904, 1905, 1906, 1907). He is described by Buchner as "our psychological Moses." In "Psychological Progress in 1904," Buchner (1905) reported James confessing that

he has been uneasy, as early "as twenty years ago," about consciousness as a thing, a biographical item which must hereafter give permanent and perhaps reversed coloring to the two magnificent volumes which gave us our great English classic in 1890 [p. 91].

In "Psychological Progress in 1905," Buchner (1906) continued to worry about James, who "advised us that consciousness does not exist [p. 127]" and also reported his inaugurating the Department of Philosophy at Leland Stanford Junior University. "Psychological Progress in 1906" reported three experiments testing the James-Lange theory of emotions (Buchner, 1907). In "Psychological Progress in 1907," reporting on the influence of pragmatism in psychology, Buchner (1908) said, "One's disappointment with this assiduously prosecuted new tendency is keen when James . . . carries the systematization no farther than to leave it 'a mere mode of approach' [p. 7]."

Of particular interest is Buchner's (1908) comment on James's Presidential Address to the American Philosophical Association in which he contended that

there is more of man than ordinary experimental psychology reveals. In view of the fact that men constantly live inside their limits of power, it is proposed that new measurements of energy or will power be made in order to build up the practical pathway whereby men may live on higher levels [p. 9].

ON WUNDT

Clearly the thoughts and research of Wundt represented a prominent aspect of psychology during this period. "Psychological Progress in 1904" (Buchner, 1905) reported a questioning of Wundt's tridimensional analysis of the psychology of feeling into pleasure–displeasure, excitement–depression, and tension–relief. "Psychological Progress in 1905" (Buchner, 1906) reported on Wundt's account of the development of modern psychology in which he "finds the unity of Hegelian philosophy to be the inspiring seed of the last century's developments [p. 125]" and refers to Wundt's well-known positions on the psychology of feelings and his demands that "feelings be regarded both subjectively and objectively, in order that they may receive a treatment into elements and relations, much the same as sensations receive [p. 130]." "Psychological Progress in 1906" (Buchner, 1907) referred to Wundt's second volume of *Volker Psychologie* which

well indicates how the fields of anthropology and sociology may be reconstructed in the hands of a master in psychology. [In this book] primitive tendencies [in racial beginnings] . . . are viewed as on a level with the processes of perception in individual psychology. . . . Myth-making is identical with the process of sense-percention. . . . To the skeleton of this bold theory, flesh and blood are given by his adaptation of *Einfühlung*—the current esthetic theory [pp. 5–6].

ON HERBERT SPENCER

About two thirds of Buchner's (1904) first article is concerned with the contribution of Herbert Spencer to psychology's role as an independent science. This emphasis was perhaps accentuated by Spencer's death the year before:

The death of Herbert Spencer . . . brought to a close one of the most important movements in modern psychology [p. 59]. . . . [The *Principles of Psychology* (1855)] became and remained one of the dominant forces in shaping psychological theory, often negatively . . . [p. 60].

Psychology was developed by Spencer, not for its own sake, but as a means of exploiting the evolutionary hypothesis . . . [p. 61].

Psychology was defined by Spencer as follows:

Under its subjective aspect, psychology is a totally unique science, independent of, and antithetically opposed to, all other sciences whatever. The thoughts and feelings which constitute a consciousness, and are absolutely inaccessible to any but the possessor of that consciousness, form an existence that has no place among the existences with which the rest of the sciences deal [p. 62].

[For Spencer], "the data" of psychology . . . are supplied by biology through its knowledge of nerve structures and function. . . . The prime duty of psychology is to determine the definite relations between the two series of physical and mental phenomena. . . . The evolution of the mind [feelings and relation between feelings] is the progressive "adjustments of internal relations to external

relations"—the quality which distinguishes the truths of psychology from those of physiology [p. 62].

Buchner concluded by saying,

It is impossible at this time to make a tabular exhibit of specific influences emanating from this strained and partial theory of mind. The movement is closed; but the spirit of its motif lives stronger than ever in the genetic method and conception to which it gave such a wide preparation [p. 64].

Other Prominent Names and Ideas

Clearly, Mark Baldwin and James Cattell were important influences of the time. Both men appeared in all of the reports from 1905 to 1908. Each year there was reference to Baldwin's "consciously genetic method through the entire structure of cognition from the simplest to the most developed mode." Cattell's role appears to be that of spreading the dimensional directions of psychology:

He contended that psychology is not definable. The logical lack of determinability of the science, however, does not prevent there being 'psychologists.' Whatever these students study, *qua* psychologists, constitutes the field belonging to the science! . . . Professor Cattell thus holds forth the promise of larger things for the future, and in doing so he but reaffirms the recurrent attitudes which during two decades have been clearing more ground for the advent of the psychologists [Buchner, 1906, p. 127].

There are two interesting references to Thorndike. One (Buchner, 1906) refers to his text *Elements of Psychology,* which emphasizes both the "descriptive" and the "dynamic" sides of psychological problems. At another point (Buchner, 1905) Thorndike is cited as calling for the

eschewing [of] speculative opinion . . . for the employment of the methods of "exact" science, a call which, as followed by himself, amounts to doing all the work over again. In fact this countertendency is quite as revolutionary as that noted above [the distinction between introspection and observation as modes of approach] [p. 93].

Other individuals mentioned several times include Dewey, Calkins, Judd, Stratton, and Angell. There are no references to Freud.

ON COMPARATIVE PSYCHOLOGY

There are two reviews of comparative psychology (Buchner, 1906, 1908):

Comparative psychology is showing signs of increasing healthfulness. . . . It has already made a wide departure from the old-time observation of the naturalist, and is growing more and more insistent upon facts secured from controlled situations. While passing through the elementary stage of determining the sensory and motor elements in the mental life of animals, this branch of psychology is happily young and plastic enough to reap all benefit from the development of functionalism. The publication of the first volume in the "Animal Behavior Series" by Yerkes, and Watson's call for a new journal to be devoted to comparative psychology are unmistakable evidences of the rapid development in this field [Buchner, 1908, p. 9].

This is followed by a reference to Watson's doctoral dissertation, published in the *Psychological Review* of 1907.

ON SOCIAL PSYCHOLOGY

The status and thrust of social psychology during this period seem far from clear. Buchner (1905) said that

among those psychologists who are more inclined to social phenomena, there is a marked tendency to agree that a so-called "social mind" does not exist, and that social phenomena do not exist apart from the individual. Social psychology now tends to devote itself to a study of the individual mind in so far as it presents what is known as "group consciousness." When this theory is carried over to the child or to the race, social psychology readily falls into the keeping of a genetic psychology. . . . There is a danger in a tendency of the systematic psychologists to play too freely into the hands of the social relationship for explanatory factors, as, for example, Royce . . . who interprets the development of reasoning as due solely to the socius. On the other hand, that there continues a steady widening of the psychological point of view, hesitating not before the needs of a cool dissection of our practical living, is evidenced by Veblen [The Theory of Business Enterprises] and Scott [The Theory of Advertising] [p. 96].

Buchner (1906) next noted:

That social psychology may come to include sociology in so far as the careful analyses of individuals in social relations is concerned, is a position which seems to be gaining ground. . . . Draghiscesco [contends] individual consciousness originates in social consciousness, and the psychology of the individual must turn away from a biological explanation of his experience. . . . Ross [presents a] balanced scheme for tracing out the ramifications of psychological interests in the different fields of human life. . . . A good illustration of basing social analyses on the import of psychological values is presented by Ghent, who interestingly traces the dominant economic character of society to mental development as an explanation of social changes [p. 131].

Again Buchner (1907) noted that

genetic and social psychology seem to be setting up a new house together. And, if one mistake not the signs of current discussions, the progeny will stand forth accredited to a new moral psychology, or the mental evolution of the moral attitude. There is already a large amount of raw material scattered in these provinces and in the adjacent region of religious psychology [p. 7].

ON CLINICAL PSYCHOLOGY

As noted in the discussion of Table 1, interest in "abnormal" psychology was exhibited by these

topics, proportionally occupying the first or second rank in publication rate for the years 1904, 1905, and 1906. There are other evidences of the strength of these interests. The *Journal of Abnormal Psychology* was founded in 1906 by Morton Prince and the Psychological Clinic in 1907 by Witmer. Buchner further noted (1908) "the application of 'the clinical' method . . . at the University of Pennsylvania and Harvard . . . [p. 11]." To this, Buchner added an interesting comment, "[This] may encourage the hope of seeing greater socio-economic values placed upon the science in American community life [p. 11]." In this same year Buchner cited the English translation of *The Psychic Treatment of Nervous Disorders* by Dubois. He said, "This disciple of the school of Nancy supports the thesis that 'nervousness is a disease preëminently psychic, and psychic disease requires psychic treatment' [p. 9]."

There are two pertinent statements regarding psychology's relationship to psychiatry at this time. In 1905 Buchner said, "[There is] the steady reconstruction of psychiatry under the influence of a sound and generous psychology . . . [p. 97]." And in 1906 he stated that "psychiatrists are tending to show a greater reliance than formerly upon the value of exhaustive description and analysis of abnormal mental states for presenting the real problems of abnormal psychology . . . [p. 132]."

ON "FUNCTIONAL" PSYCHOLOGY

From Buchner's point of view, "functionalism" had won the day as a "philosophy" of psychology. Buchner (1905) first stated

> that psychology is slowly but surely gaining a restoration of her once constructive influence upon a total view of experience. . . . This comes out unequivocally in the recent widespread acceptance and defense of the "functional" as over against the earlier "structural" view of the science [p. 92].

In "Psychological Progress in 1905," Buchner (1906), in speaking of Angells' book *Psychology: An Introductory Study of the Structure and Function of Human Consciousness*, stated that the structural and functional viewpoint which maintains "a strongly flavored biological point of view . . . tends to regard consciousness as merely one among many manifestations of organic life [p. 128]." In the next year Buchner (1907) noted:

> The attitude of the psychologist towards his subject-matter seems to be less a question for debate or a theory of elaboration than formerly. In spite of the fact that the functional point of view seems to have almost completely won out, there continue efforts to be even more precise in the application of criteria to consciousness [p. 3].

In "Psychological Progress in 1907," the influence of functionalism on increased attention given to the experimental investigation of the motor half of conscious processes was noted, and Buchner (1908) said,

> This naturally brings one nearer the "practical" phases of daily experiences, and may result in removing the semi-popular objection to psychological experimentation that in view of the ingenuity and assiduity displayed the worth of the results thus far obtained has not been commensurate to the great labors [p. 5].

A Scattering of Pertinent Comments

> How patiently experimental methods must proceed, and how meagre their results, in an analysis of the conditions of "learning," may be seen, for example, in Brown's study of the processes of addition, subtraction, multiplication and division. One of our great desiderata is an efficient psychology of learning . . . [Buchner, 1907, p. 5].

> The attitude to Jennings is fruitfully different from that maintained by Pavlov in his recent Huxley Lecture, his contention being that physiology has been advancing by borrowed light, the explanatory ideas of psychology,—a source of "evil influences" [Buchner, 1907, p. 7].

In commenting on the *Dictionary of Philosophy in Psychology* by Rand, Buchner (1907) stated:

> The literatures of these cognate fields are now within the grasp of the reader as never before, and the current additions to bibliography will tend to favor a better grade of scholarship in these departments of knowledge, and possibly check some of the rush "to print." . . . [however] The creation of new journals does not give much encouragement to such a hope [p. 8].

In speaking about terminology, Buchner (1905) stated:

> We should be carried too far afield to specify . . . the renewed bearings of psychology upon those bantering philosophical novelties which are current under such terms as "radical empiricism," "humanism," "pragmatism," "instrumentalism" [p. 97]. [Or] It is a bit surprising to find a rising tide of dissatisfaction threatening to engulf some of the "older" (and almost consecrated) terms in psychology. The recent dispositions made of "consciousness" are closely followed by confessed willingness to see such terms as "feeling," "sensation," "perception," and others disappear entirely from the language of the student of human experience. Oddly enough, too, new terms are not at hand nor suggested to replace those now worn thread-bare by long usage. Might they have fallen into contempt by reason of too great familiarity? This tendency may be presaging the advent of the framer of the vocabulary of psychology for the new twentieth century [Buchner, 1907, pp. 1–2]!

Apologia

As I review my review of these years I become aware of conveying poorly a most pervasive tone

and concern of the times: the unsettled and uncertain nature of the enterprise itself and the plaintive, persistent, and personal assaults on the associated issues of subject matter, method, and psychology's place in the systematic disciplines. Such issues as consciousness, mind, introspection, biologism, and application are all threaded throughout these summaries. In describing the 1905 International Congress of Psychology, Buchner (1906) said somewhat resignedly:

> The geographical unity of time and space . . . must not be mistaken for the inner and progressive unity of a science. . . . There was no marked agreement; each continued to speak then just as he had hitherto thought in his individual capacity [p. 133].

Let me try to convey this tone by an extracted summary of Buchner's first two paragraphs from his final article in 1908:

> Judged within the close range of its dying hours, the year 1907 does not seem to be conspicuous in the annual achievements of psychology. Sketching its story is a task not unlike the ancient one of making bricks without straw . . . . The angle of individual straws has shown the winds to have been blowing in all directions [p. 1].

> That modern psychology is still an unsettled science is to be seen in the continued debate as to its real nature and the assumptions upon which its constructive activities are to be based. In his last work . . . Möbius reveals, as he thinks, the utter hopelessness of psychology in its struggle to become a science. . . . As much as to say that psychology is not "hopeless," Miss Calkins raises the query: Psychology: "What is It about?" In her partially published answer, the double demand is made that metaphysics be eliminated from psychology, and that the psychologist shall admit into his thinking the every-day distinction between the psychical and the physical. . . . Stumpf has newly declared that the science is dealing with both mental contents and with mental functions. In a still more hopeful manner, Kirkpatrick would enrich the science's vocabulary while suggesting its need of a broader base. . . . The most systematic exposition, if not defense, of the rapidly spreading functional point of view that has yet been made appeared in Angell's presidential address. One would almost be led to believe that the long observed structural point of view was an abstraction and produced erroneous results . . . [pp. 1–2].

Envoi

There can be no summary of the summaries. I refrain from drawing the obvious contemporary parallels, detours, and retreats and leave these for each reader's amusement and bemusement. I end with a quotation from Witt Auden's *To Clio*, the Muse of History:

> Madonna of silences, to whom we turn
> When we have lost control
> . . . forgive us our noises
> And teach us our recollections. . . .

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Reports of the Regional Associations for 1974

Beginning with the 1974 annual meetings, proceedings of the American Psychological Association and of the regional psychological associations will be published by and made available through the JSAS *Catalog of Selected Documents in Psychology*. The following proceedings are now available and can be ordered directly from the JSAS Office: Ms. No. 739, American Psychological Association, Proceedings of the Eighty-Second Annual Convention, 196 pages, $8; Ms. No. 740, Southern Society for Philosophy and Psychology, Proceedings of the Sixty-Sixth Annual Meeting, 14 pages, $2; Ms. No. 741, Eastern Psychological Association, Proceedings of the Forty-Fifth Annual Meeting, 88 pages, $3; Ms. No. 742, Western Psychological Association, Proceedings of the Fifty-Fourth Annual Meeting, 60 pages, $3; Ms. No. 743, Midwestern Psychological Association, Proceedings of the Forty-Sixth Annual Meeting, 29 pages, $2; Ms. No. 744, Southeastern Psychological Association, Proceedings of the Twentieth Annual Meeting, 83 pages, $3; Ms. No. 745, Southwestern Psychological Association, Proceedings of the Twenty-First Annual Meeting, 36 pages, $2; Ms. No. 746, Rocky Mountain Psychological Association, Proceedings of the Forty-Fourth Annual Meeting, 33 pages, $2.

Payment must accompany orders amounting to $15 or less. Send orders to JSAS, American Psychological Association, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

Below are short reports of the regional meetings. The report of the forty-fifth annual meeting of the Eastern Psychological Association appeared in the September issue (pages 711–713). The New England Psychological Association held its 1974 meeting in November—too late to be included in this issue.

Western Psychological Association

Report of the Fifty-Fourth Annual Meeting

JOSEPHINE FIEBIGER

Secretary-Treasurer
California State University, Long Beach

The Fifty-Fourth Annual Meeting of the Western Psychological Association was held April 25–28 at the Sheraton-Palace Hotel, San Francisco. There were 3,673 registrants at the Meeting. The number of association members is 2,755, a total which represents a 23% increase over last year's membership.

The host institutions for the Convention were University of California, Berkeley, and California State University, Hayward. Georgia Babladelis and Milton Blood were the Convention Managers. Other Convention personnel were: Nancy Anderson, Advertising; Sandra Hart, Audio-Visual; David Tieman, Exhibits; Christina Maslach, Film Program; Alan Monat, Housing; Judi Komaki, Placement; Robert Olton and Sheldon Zedeck, Program; Mike Patch, Public Information; Patricia Renwick, Registration; Nicholas Imparato, Social; and Robert Vallon, Volunteers.

This year the Program Committee processed about 1,000 program proposals of which approximately 400 papers, 55 symposia, and 20 workshops were accepted. In addition, there were 9 conversation hours, 23 films, and 4 invited addresses. Over 1,100 individuals were listed as formal participants in the program.

At the Board of Directors' meeting, the Status of Women Committee, chaired by Nora Weckler, presented an extensive report on their activities. The

Board of Directors voted to continue its support of the Committee's activities.

The 1974 Business Meeting lacked a quorum and no resolutions were passed this year.

Officers for 1974–1975 are Eleanor E. Maccoby, President; Richard C. Atkinson, President-elect; Paul H. Mussen, Past President; Southern Representative, Neil R. Bartlett; Northern Representative, Irwin G. Sarason; and Secretary-Treasurer, Josephine Fiebiger.

The 1975 Meeting of the Association will be held April 24–26 in Sacramento, California, with California State University, Sacramento, as host institution. George Parrott will serve as Convention Manager.

The 1976 Meeting will be held in Los Angeles, with California State University, Northridge, as host. Nora Weckler and Patricia Keith-Spiegel will serve as Convention Managers.

Midwestern Psychological Association

Report of the Forty-Sixth Annual Meeting

RUDOLPH W. SCHULZ
Secretary-Treasurer
University of Iowa

The Midwestern Psychological Association held its Forty-Sixth Annual Meeting in the Conrad Hilton Hotel, Chicago, Illinois, May 2–4, 1974. The program included 408 contributed papers, 7 symposia, 11 invited papers, 2 film programs, and 1 open meeting. Psi Chi presented a session of 8 papers and 2 open meetings. Four invited addresses were given: William K. Estes, Rockefeller University, "A New Look at Probability Learning"; Charles A. Kiesler, University of Kansas, "A Motivational Theory of Stimulus Incongruity with Applications for Such Phenomena as Dissonance and Self-Attribution"; John H. Flavell, University of Minnesota, "Some Perspectives on Human Memory Development"; and Elliot S. Valenstein, University of Michigan, "Evolution of Physical Interventions in the Human Brain." The Presidential Address, "Recent Explorations with a T-Maze: Women's Lib, Long Delays, and All That," was delivered by M. Ray Denny, Michigan State University.

The newly elected officers of MPA are William N. Dember, University of Cincinnati, President-elect (1974–1975), and Leonard D. Eron, University of Illinois at Chicago Circle, Council Member (1974–1977).

Members of the Program Committee for 1974 were James G. McHose, Southern Illinois University, Moderator; Donald V. DeRosa, Bowling Green State University; Steven J. Sherman, Indiana University; June P. Chance, University of Missouri; Arnold A. Gerall, Tulane University; Martin F. Kaplan, Northern Illinois University; Donald J. Levis, State University of New York at Binghamton; Margaret J. Peterson, Indiana University; and Rudolph W. Schulz, University of Iowa, ex-officio. George S. Speer, Illinois Institute of Technology, served as Convention and Local Arrangements Manager. Local Arrangements Coordinators were Ralph K. Meister, Chicago Jewish Vocational Service, Registration; David W. Bortree, Thornton High School, Placement; Fredric L. Ware, American Personnel Consultants, Mail and Information; Tom Edwards, University of Illinois at Chicago Circle, Public Relations; Eugene Zechmeister, Loyola University of Chicago, Volunteers; and Steven Langer, Abbott, Langer & Associates, Audio-Visual.

The 1975 Annual Meeting will be held at the Conrad Hilton Hotel in Chicago, May 1–3. Donald V. DeRosa, Department of Psychology, Bowling Green State University, Bowling Green, Ohio 43402, will serve as Program Committee Moderator for this year's meeting. Inquiries concerning membership should be directed to Rudolph W. Schulz, Secretary-Treasurer, Department of Psychology, University of Iowa, Iowa City, Iowa 52242.

Rocky Mountain Psychological Association

Report of the Forty-Fourth Annual Meeting

URSULA DELWORTH
Secretary
Western Interstate Commission for Higher Education

The Forty-Fourth Annual Meeting of the Rocky Mountain Psychological Association was held May 8–11, 1974, at the Cosmopolitan Hotel, Denver, Colorado. Registration totaled 675. Membership in the Association totaled 480 as of May 1, 1974.

Irwin W. Cohen served as Convention Manager. Chairpersons of convention committees were: Donald L. Tasto and Thomas L. Bennett, Program; John C. Hocking, Placement; Alice R. Fehrenbach, Publicity; Robert Trustman, Volunteers.

The program included original papers, symposia, workshops, tutorials, movies, and invited addresses. Program cochairperson Donald Tasto reported the papers submitted and accepted to be of higher quality than in previous years.

Richard Weigel, as President of the Association, conducted the annual business meeting. Major announcements and items of business transacted during the meeting were as follows:

1. The next annual meeting is scheduled for May 7–10, 1975, in Salt Lake City, Utah. Meetings will be held in the city Convention Center.

2. Harl Young was named President-elect of the Association by a mail ballot of the members.

3. W. C. Leiding was reelected as Treasurer of the Association for a three-year term.

4. The membership voted to support the following action of the Executive Committee regarding the APA decision to no longer publish proceedings of the regional association meetings: That RMPA communicate objections to this decision to APA, that RMPA write to the presidents of the other six regional associations stating our objections and asking their opinions and what actions they might have taken, and that RMPA write to the chairperson of the APA Publications and Communications Committee questioning whether this is a change in editorial or publication policy.

5. The membership voted to buy institutional membership in both AAP and CAPPS.

6. The membership voted to go on record against cosponsoring a regional meeting of APA.

7. The membership authorized the Executive Committee to pursue incorporation concerns and nonexempt status for the Association, and to take such actions as it can in this regard.

8. Reports were made by all officers and the following committees: Convention—Manager, Program, and Placement; Membership; Committee on Groups Underrepresented in Psychology.

9. No action was taken on adding the province of Alberta to RMPA because the wishes of psychologists in Alberta had not been explored fully.

10. Gregory Kimble was installed as President of the Association.

Full-length proceedings of the regional associations are available through the JSAS *Catalog of Selected Documents in Psychology*. To order the complete proceedings, see details on page 903.

Southwestern Psychological Association

Report of the Twenty-First Annual Meeting

C. EUGENE WALKER

Secretary-Treasurer
University of Oklahoma Medical School

The Twenty-First Annual Meeting of the Southwestern Psychological Association was held at the Paso Del Norte Hotel in El Paso, Texas, on May 2–4, 1974. A total of 420 persons registered for the Meeting. The current membership is 1,594.

Randolph H. Whitworth, University of Texas at El Paso, served as Program Chair; Philip Himelstein, University of Texas at El Paso, was in charge of Local Arrangements; and Paul Whitmore, Human Resources Research Organization, Ft. Bliss, El Paso, was Placement Service Chair.

Vladimir Pishkin, Veterans Administration, University of Oklahoma Health Sciences Center, gave the Presidential Address on Thursday. Joseph D. Matarazzo, University of Oregon Medical School, was the Distinguished Invited Speaker on Thursday. Rogelio Diaz-Guerrero, National Autonomous University of Mexico and Visiting Professor, University of South Florida, was the Distinguished Invited Speaker on Friday.

Winners of the Publisher's Prize were Charles J. Holahan, University of Texas at Austin, and Robert E. Prytula, Middle Tennessee State University. Winners in the Graduate Student Competition cosponsored by Psi Chi were Michael R. Berren, Texas Tech University, first; Robert Breaux, Texas Tech University, runner-up; and William J. Broussard, Texas Research Institute of Mental Sciences, runner-up. Winner in the Undergraduate Student Competition was James M. Williams, Texas Arts and Industries University.

New officers for the Association for 1974–1975 are Logan Wright, President; Vladimir Pishkin, Past-President; Harry J. Parker, President-elect; C. Eugene Walker, Secretary-Treasurer; and the following Council Representatives for the states in the SWPA region: Robert Haygood, Arizona; Philip Trapp, Arkansas; Leon Rappoport, Kansas; Laurence Siegel, Louisiana; Frank A. Logan, New Mexico; Oscar A. Parsons, Oklahoma; and Janet T. Spence, Texas.

The 1975 meeting of the Association will be held in Houston, Texas, April 17–19. William G. Braud will serve as Program Chair; and J. K. Weatherly will

serve as Local Arrangements Chair. Inquiries regarding the annual meetings should be addressed to the Association's central office at P.O. Box 7156, University Station, Austin, Texas 78712.

Southeastern Psychological Association

Report of the Twentieth Annual Meeting

EDWARD H. LOVELAND

Secretary-Treasurer
Georgia Institute of Technology

The Twentieth Annual Meeting of the Southeastern Psychological Association was held May 2–4, 1974, in the Diplomat Hotel in Hollywood, Florida. There were 1,255 registrants for the Convention, which met jointly with the Florida Psychological Association and the Florida Association of School Psychologists. The total membership of the Association at the time of the Meeting was 1,862. Joseph C. Hammock, University of Georgia, was Program Chair. Wallace Wilkins, University of Miami, chaired the Local Arrangements Committee. Michael Epstein, University of Miami, was Director of Placement. Under the auspices of its Continuing Education Committee, the Association sponsored six preconvention professional workshops, which were well attended.

Special programs which were held in conjunction with the Meeting were the annual all-day meeting of the Southeastern Society for Multivariate Experimental Psychology, with Harry E. Anderson, University of Georgia, presiding; the Southeastern Regional Psi Chi meeting and invited address with William Grossnickle, East Carolina University, presiding, and Sidney Jourard, University of Florida, speaking on "Within Freedom and Dignity: The Task of Humanistic Psychology."

Other groups which met during the Convention included the Southeastern Workers in Memory, the Southeastern Region State Psychology Boards, and the Southeastern Association of Heads of Departments.

William D. Spears delivered the Presidential Address, "On the Operant Behavior of Unicorns," immediately preceding the business meeting of the Association.

During 1973–1974, the Executive Committee of the Association was composed of William D. Spears, President; Joseph C. Hammock, President-elect; Charles D. Spielberger, Past-President; Edward H. Loveland, Secretary-Treasurer; and Members-at-Large Raymond Shrader and Laurence Siegel.

Officers for 1974–1975 are Joseph C. Hammock, President; Marshall Jones, President-elect; and Edward H. Loveland, Secretary-Treasurer. Past-President is William D. Spears, and Members-at-Large of the Executive Committee are Laurence Siegel and Ellen Kimmel.

Southern Society for Philosophy and Psychology

Report of the Sixty-Sixth Annual Meeting

MICHEL LOEB

Secretary
University of Louisville

The Sixty-Sixth Annual Meeting of the Southern Society for Philosophy and Psychology was held on April 11–13, 1974, at the International Inn in Tampa, Florida. Hosts were the Departments of Philosophy and Psychology at the University of South Florida. Approximately 200 members, friends, and guests were in attendance.

The Program consisted of 36 papers in philosophy and 47 papers in psychology, in addition to two symposia in psychology and three joint sessions. One of the philosophy sessions was a colloquy in honor of Erwin W. Straus. As usual, a social hour and business session were included in the program.

The first joint session was a symposium regarding women, philosophy, and psychology. Participants included Mary Anne Baker, William T. Blackstone, Clyde E. Noble, and Dwight Van de Vate. At the second, Douglas Browning, University of Texas, introduced Lelon J. Peacock, University of Georgia, who delivered the Presidential Address, "Ionizing Radiation and Behavior." At the third joint session, Duane Rumbaugh, Georgia State University, delivered an invited address, "Automated Language Training of the Chimpanzee, Lana." Discussants for Dr. Rumbaugh's address were Donald E. Nute, University of Georgia (Philosophy), and Herbert D. Kimmel, University of South Florida (Psychology).

Two Richard M. Griffith Memorial Awards were announced. The recipient in Philosophy was Dr. Roger J. Sullivan of the University of South Carolina for his paper, "The Kantian Critique of Aristotle's Philosophy: An Appraisal." The recipient in Psychology was Dr. Mary Ellen O'C. Chernovetz of the University of Tulsa for her paper, "Repeated Exposure to X-Radiation and Radiation-Induced Taste Aversion." These awards are given annually for the papers of highest merit by members who have not yet received the doctoral degree or who have received it within the last five years.

Cochairs of the Committee on Local Arrangements were James Gould (Philosophy), University of South Florida, and Herbert D. Kimmel (Psychology), University of South Florida. Cochairs of the Program Committee were Frank R. Harrison III (Philosophy), University of Georgia, and Norman R. Remley (Psychology), Texas Christian University. Cochairs of the Richard M. Griffith Memorial Awards were Douglas Browning (Philosophy), University of Texas, and Stanley B. Williams (Psychology), College of William and Mary. Cochairs of the Membership Committee were Clyde E. Noble (Psychology), University of Georgia, and Donald Sherburne (Philosophy), Vanderbilt University.

At the Annual Business Meeting, reports of the officers and of several standing ad hoc committees were received and accepted. It was announced that the 1975 Annual Meeting would be held in the Marriott Inn in New Orleans, Louisiana, under the joint sponsorship of the Departments of Philosophy and Psychology of Tulane University on March 27–29.

The deaths of members Robert C. Wingfield and Alvin Nelson were noted with sorrow.

Thirty-three philosophers and thirty-four psychologists were elected to full or associate membership in the Society. Frank A. Geldard and Erwin W. Straus were elected Honorary Members for Life. Full and associate members elected were:

Full Members, Philosophy: Robert Almeder, Lynne R. Baker, Margaret A. Balinsky, Ronald L. Barnette, Peter A. Bowman, George Brenkert, L. Hughes Cox, Richard E. Creel, Daniel T. Devereux, Ronald H. Epp, Ash Gobar, Alfred H. Guy, Jr., H. Phillips Hamlin, James R. Harris, Donald S. Klinefelter, Betsy P. Latta, Donald W. Livingston, Ruth Manor, Stephen A. McKnight, Delos B. McKown, Eben S. Moulton, Kai Nielsen, M. A. C. Richter, E. W. Van Steenburgh, James L. Stiver, Roger J. Sullivan, Paul E. Tibbetts, Jeffrey Tlumak, Frank C. Williams, and John J. Young.

Full Members, Psychology: James A. Beshai, Sue Bogner, Robert L. Breckenridge, Allen Blomquist, Darryl Bruce, Barbara L. Canter, Kyle R. Carter, Mary Ellen O'C. Chernovetz, Catherine S. Davison, William B. Dragoin, Katherine Ernst, Rose Frank, Jack N. Haney, Robert E. Hicks, Charles S. Noble, Shirley W. Osgood, Milton S. Palanker, Andrew J. Pavlos, Florrie M. Pennington, John W. Porter, Jack B. Rollins, Dewey Rundus, Kendon Smith, John R. Stabler, David J. Stang, Paschal N. Strong, Jr., and George J. Vesprani.

Associate Members, Philosophy: John N. Hooker, Jr., Stephen Skousgaard, and Michael E. Zimmerman.

Associate Members, Psychology: Brenda J. Clower, Franklin J. Euse, Sam J. Hagan, Richard P. Honeck, Keith W. Jacobs, Gary L. Snethen, and H. Roger Wilkerson.

The following officers were elected to serve during 1974–1975: William T. Blackstone, University of Georgia, President; Clyde E. Noble, University of Georgia, President-elect; Donald Sherburne, Vanderbilt University, Treasurer; Nancy Simco, Memphis State University, to a three-year term on Council for Philosophy; Joel S. Warm, University of Cincinnati, to a three-year term on Council for Psychology; and Frank R. Harrison III, University of Georgia, to a one-year term on Council for Philosophy. Continuing members of the Council include Mary Ellen Curtin, Veterans Administration Hospital, Lexington, Kentucky; Thomas K. Hearn, College of William and Mary; and Norman R. Remley, Texas Christian University. Michel Loeb, University of Louisville, continues to serve as Secretary.

APA Visiting Scientist Program: 1973–1974

EDUCATIONAL AFFAIRS OFFICE

The American Psychological Association's Visiting Scientist Program has now completed its fourteenth year. Support for the program was provided by a grant from the American Psychological Foundation, contributions from the institutions visited, and budgeted funds from the American Psychological Association.

As in the past, the Visiting Scientist Program provided colleges and small universities with visits from eminent psychologists who offered their services as either scientific lecturers or educational consultants. Over 1,000 such visits have been made to educational institutions across the United States, 70 of which were made during 1973–1974.

Throughout its existence the program has maintained the following objectives: (a) to stimulate interest in the scientific aspects of psychology at the collegiate level; (b) to present some of the new developments in psychological research and application; (c) to provide psychology departments with expert consultation regarding curricula, laboratories, staffing, programs, and procedures; (d) to acquaint students with the opportunities for advanced study and scientific and professional careers in psychology, including the teaching of psychology. For the last several years a special effort has been made to include psychologists and institutions which would serve as representatives of minority groups.

The reports of the visits were very favorable and the Educational Affairs Office would like to express its appreciation to all of those who contributed to the success of this year's program.

TABLE 1

Institutions Visited in the 1973–1974 Visiting Scientist Program

Institution	Visitor
Adrian College Adrian, Michigan	Donald R. Brown University of Michigan
Albany State College Albany, Georgia	Lawrence W. Littig Howard University
Albion College Albion, Michigan	Lauren Jay Harris Michigan State University
Alma College Alma, Michigan	Glayde Whitney Florida State University
Bates College Lewiston, Maine	Colin Martindale University of Maine
Beaver College Glenside, Pennsylvania	Alan I. Leshner Bucknell University
Bishop College Dallas, Texas	Charles Hill Louisiana State University at New Orleans
Bradley University Peoria, Illinois	Russell G. Green University of Missouri
Capital University Columbus, Ohio	Timothy C. Brock Psychology Licensing Commission–Ohio
Carthage College Kenosha, Wisconsin	Marion H. Groves Illinois Institute of Technology
Cheyney State College Cheyney, Pennsylvania	Freda Rebelsky Boston University

TABLE 1—(*Continued*)

Institution	Visitor
Clemson University Clemson, South Carolina	Robert H. Pollack University of Georgia
Colby College Waterville, Maine	Robert Chin Boston University
Dillard University New Orleans, Louisiana	Marshall R. Jones University of Miami
Drury College Springfield, Missouri	Richard G. Weigel Colorado State University
Edinboro State College Edinboro, Pennsylvania	James T. Tedeschi State University of New York at Albany
Elmhurst College Elmhurst, Illinois	Martin F. Kaplan Northern Illinois University
Franklin and Marshall College Lancaster, Pennsylvania	Nancy S. Anderson University of Maryland
George Mason University Fairfax, Virginia	James B. Appel University of South Carolina
Guilford College Greensboro, North Carolina	Ronald L. Webster Hollins College
Hollins College Hollins College, Virginia	Arthur I. Schulman University of Virginia
Hope College Holland, Michigan	Wilbert J. McKeachie University of Michigan
Illinois College Jacksonville, Illinois	Melvin H. Marx University of Missouri
Indiana Institute of Technology Fort Wayne, Indiana	Norman Kagan Michigan State University
Indiana University at South Bend South Bend, Indiana	Wilse B. Webb University of Florida
Jackson Community College Jackson, Michigan	Norman Abeles Michigan State University
Kalamazoo College Kalamazoo, Michigan	Joseph B. Sidowski University of South Florida
Lincoln University of Missouri Jefferson City, Missouri	Alan E. Gross University of Missouri at St. Louis
Manchester College North Manchester, Indiana	N. John Castellan, Jr. Indiana University
Manhattan College Bronx, New York	Donald E. Mintz City College of New York
Mankato State College Mankato, Minnesota	Thomas J. Bouchard, Jr. University of Minnesota
Middle Tennessee State University Murfreesboro, Tenessee	Josef Brözek Lehigh University
Missouri Western State College St. Joseph, Missouri	William E. Jaynes Oklahoma State University
Moravian College Bethlehem, Pennsylvania	Dale B. Harris Pennsylvania State University

TABLE 1—(*Continued*)

Institution	Visitor
Mount Union College Alliance, Ohio	Paul A. Games Pennsylvania State University
Nebraska Wesleyan University Lincoln, Nebraska	Donald L. Tasto Colorado State University
North Dakota State University Fargo, North Dakota	Gregory A. Kimble University of Colorado
Northeast Missouri State University Kirksville, Missouri	Lawrence Simkins University of Missouri at Kansas City
Ohio Northern University Ada, Ohio	Michael Chandler University of Rochester
Olivet Nazarene College Kankakee, Illinois	R. Stewart Jones University of Illinois
Ouachita Baptist University Arkadelphia, Arkansas	William C. Howell Rice University
Pembroke State University Pembroke, North Carolina	Lyle F. Schoenfeldt University of Georgia
Pitzer College Claremont, California	Dwight L. Goodwin California State University, San Jose
University of Puget Sound Tacoma, Washington	Robert E. Knox University of British Columbia
Randolph-Macon Woman's College Lynchburg, Virginia	Gregory R. Lockhead Duke University
Ripon College Ripon, Wisconsin	Ross Stagner Wayne State University
Russell Sage College Troy, New York	David J. King State University of New York College at Oswego
Sacred Heart University Bridgeport, Connecticut	Otello Desiderato Connecticut College
St. Joseph's College Rensselaer, Indiana	Myron M. Arons West Georgia College
St. Mary College Winona, Minnesota	William W. Beatty North Dakota State University
Santa Clara University Santa Clara, California	Paul Ekman University of California, San Francisco
St. Mary-of-the-Woods College St. Mary-of-the-Woods, Indiana	John F. Santos University of Notre Dame
College of Saint Teresa Winona, Minnesota	Willard W. Hartup University of Minnesota
Seton Hill College Greensburg, Pennsylvania	Marvin W. Daehler University of Massachusetts
Slippery Rock State College Slippery Rock, Pennsylvania	Stanley Pliskoff University of Maine
Southern State College Magnolia, Arkansas	Luciano L'Abaté Georgia State University
Southwest Minnesota State College Marshall, Minnesota	Gerald Rosenbaum Wayne State University

TABLE 1—(*Continued*)

Institution	Visitor
University of Southwestern Louisiana Lafayette, Louisiana	Edward J. Murray University of Miami
Southwestern University Georgetown, Texas	Ralph Mason Dreger Louisiana State University
State University of New York College at Oneonta Oneonta, New York	Harold Babb State University of New York at Binghamton
State University of New York College at Plattsburgh Plattsburgh, New York	John A. Nevin University of New Hampshire
Suffolk University Boston, Massachusetts	Richard T. Louttit University of Massachusetts
Tougaloo College Tougaloo, Mississippi	William F. White University of Georgia
Ursinus College Collegeville, Pennsylvania	Wallace A. Kennedy Florida State University
Washington College Chestertown, Maryland	Carl D. Swanson Madison College
Washington and Jefferson College Washington, Pennsylvania	Willem A. Wagenaar Pennsylvania State University
Western Connecticut State College Danbury, Connecticut	Dorothy Tennov University of Bridgeport
Westmar College LeMars, Iowa	C. F. Wrigley Michigan State University
Willamette University Salem, Oregon	Joseph D. Matarazzo University of Oregon Medical School
College of Wooster Wooster, Ohio	Patrick J. Capretta Miami University

APA Visiting Psychologist Program: 1973–1974

EDUCATIONAL AFFAIRS OFFICE

The NIMH-sponsored Visiting Psychologist Program has completed its seventh year of operation. This program is designed to facilitate the dissemination of current knowledge generated in recent research findings and to provide for the demonstration of innovations in psychological research to mental health professionals. The program attempts to reach practitioners in service settings by offering consultation, lectures, and demonstrations by psychologists who are themselves significantly involved in research, training, and delivery of services. During the 1973–1974 program year, 49 agencies were visited, of which 38 were newly selected agencies and 11 were return visits to agencies visited the previous year. Forty-eight psychologists participated as visitors in the program. Agencies that received initial visits are listed, with their Visiting Psychologists, in Table 1. Agencies and Visiting Psychologists involved in return visits are listed in Table 2.

The program is administered by the professional staff of the Educational Affairs Office and is under the general supervision of the Education and Training Board. An Advisory Committee, the current members of which are Stanley Brodsky, Mildred Buck, Ruth Lesser, William Pierce, and Arthur Wiens (Chair), monitors the program. The Advisory Committee would like to thank all those who contributed to the success of this year's activities.

The selection of agencies for the program has been guided by the same broad criteria since the inception of the program. The target group continues to be those agencies wherein psychologists are employed in a professional role but where there is little in the way

TABLE 1

Agencies Receiving Initial Visits in the 1973–1974 Visiting Psychologist Program

Agency	Visitor
Alaska Board of Psychologist Examiners Fairbanks, Alaska	Sumner Morris University of California
Arizona State Prison Florence, Arizona	Paul Bramwell Atascadero State Hospital
Baker County Mental Health Clinic Baker, Oregon	Arthur Wiens University of Oregon Medical School
Child Guidance Center Dayton, Ohio	Max Magnussen Pittsburgh Child Guidance Center
Community Mental Health Center of Fulton McDonough Counties Macomb, Illinois	William Hunter Range Mental Health Center
Community Mental Health Mental Retardation Center St. Francis General Hospital Pittsburgh, Pennsylvania	R. Constance Fischer Duquesne University
Comprehensive Mental Health Center Savannah, Georgia	John Barry University of Georgia
Concord Mental Health Center Concord, New Hampshire	Margalith Holzinger Hofstra University
Cumberland County Guidance Center Millville, New Jersey	Dorothy Evans Alexandria Community Mental Health Center
Dixmont State Hospital Sewickley, Pennsylvania	Gottlieb Simon American Psychological Association
Family Mental Health Services Hickory, North Carolina	Edward Kelty NIMH

TABLE 1—(*Continued*)

Agency	Visitor
Gila County Guidance Clinic Globe, Arizona	Norris Vestre Arizona State University
Indian River Community Mental Health Center Fort Pierce, Florida	Patrick Cook Florida State University
London Correctional Institution London, Ohio	Robert Mills Cincinnati, Ohio
Marathon County Health Care Center Wausau, Wisconsin	Roberta Ray University of Wisconsin
Marquette Branch Prison Marquette, Michigan	Newton L. P. Jackson University of Michigan
Massillon State Hospital Massillon, Ohio	William Seeman University of Cincinnati
Maui Mental Health Service Wailulu, Hawaii	Ronald Tharp University of Hawaii
Mid-Columbia Center for Living The Dalles, Oregon	James Lindemann University of Oregon Medical School
Midland-Gladwin Community Mental Health Center Midland, Michigan	Norman Abeles Michigan State University
Navajo County Guidance Clinic, Inc. Winslow, Arizona	Marvin Kahn University of Arizona
O'Berry Center Goldsboro, North Carolina	Al Baumeister University of Alabama
Pajaro Children's Services Watsonville, California	Amado Padilla University of California
People's Place II Milford, Delaware	Nathan Stockhamer New York, New York
Pineland Center Pownal, Maine	William Fraenkel Massachusetts Department of Mental Health
Pine Rest Christian Hospital Grand Rapids, Michigan	Floyd Wylie Highland Park Mental Health Center
Quinco Consulting Center Columbus, Indiana	Sol Garfield Washington University
St. Ann's Home, Inc. Methuen, Massachusetts	John Burchard University of Vermont
St. Clare Hospital Monroe, Wisconsin	Eugene Gauron University of Iowa
St. Louis Association of Black Psychologists St. Louis, Missouri	William Pierce Westside Mental Health Center
South Central Mental Health and Retardation Center Jamestown, North Dakota	Carl Sipprelle University of South Dakota
Southwestern State Hospital Marion, Virginia	James Anker Knoxville, Tennessee
State Department of Welfare and Institutions, Mobile Psychiatric Clinic Richmond, Virginia	Herbert Quay Temple University
Tri-County Mental Health Center Bennettsville, South Carolina	Robert Heckel University of South Carolina

TABLE 1 (*Continued*)

Agency	Visitor
Tuscarawas Valley Comprehensive Mental Health Center Dover, Ohio	Donald Freedheim Case Western Reserve University
University Hospital of Jacksonville Community Mental Health Center and Mental Health Clinic of Jacksonville Jacksonville, Florida	Charles Spielberger University of South Florida
Wernersville State Hospital Wernersville, Pennsylvania	Richard Lundy Pennsylvania State University
Western State Psychiatric Hospital Bolivar, Tennessee	David Terrell Meharry Medical College

TABLE 2

Agencies Revisited in the 1973–1974 APA Visiting Psychologist Program

Agency	Visitor
East Mississippi State Hospital Meridian, Mississippi	C. J. Rosecrans University of Alabama
Family Counseling Center Kenosha, Wisconsin	Margaret Clay University of Michigan
Michigan Reformatory Ionia, Michigan	Asher Pacht Department of Health and Social Services
Mon Valley United Health Services Monessen, Pennsylvania	Max Magnussen Pittsburgh Child Guidance Center
Okmulgee County Guidance Center Okmulgee, Oklahoma	Mildred Buck St. Louis, Missouri
Pennyroyal Regional Mental Health–Mental Retardation Center Hopkinsville, Kentucky	William Bricker George Peabody College
Plateau Mental Health Center Cookeville, Tennessee	Robert Sprague University of Illinois at Urbana-Champaign
Polk General Hospital (Peace River Center) Bartow, Florida	Theodore Blau Tampa, Florida
Regional Mental Health Center Kokomo, Indiana	Eugene Levitt Indiana University School of Medicine
Rutherford County Guidance Center Murfreesboro, Tennessee	J. L. Khanna University of Tennessee
Tidewater Mental Health Clinic Williamsburg, Virginia	John McMillan George Washington University

of resources to provide outside stimulation and exposure to new developments in the field. The program tries to provide consultation and continuing education service to agencies that might go unnoticed in most efforts directed at broad dissemination of knowledge.

The program has endeavored to recruit visitors with special qualifications to meet the special needs of the agencies that have applied. Wherever possible, visitors are paired with agencies on a regional basis in order to keep travel expenses at a minimum. An additional benefit of this regionalized pairing has been the development of more lasting relationships between visitors and agencies.

Last year it was reported that the Visiting Psychologist Program was in its final year of support by NIMH (Educational Affairs Office, 1973). We are pleased to report now that the grant funds have been restored by NIMH for the 1974–1975 year and that a full program of visits will take place. During the coming year it is expected that the Visiting Psychologist Program will continue to follow a trend toward greater flexibility and broadened scope. This trend includes attempts to reach larger numbers of agencies which serve primarily disadvantaged or minority clientele to make increased use of minority psychologists as visitors, to send more than one visitor when the problems presented by the agencies are unusually complex, and to reach more local professional groups of psychologists.

The use of graduate students as visitors who accompany the regular Visiting Psychologists has become an important and highly successful feature of the program. Last year 16 graduate students participated. They provided significant consultation service to the agencies and also had a unique training experience. Furthermore, the opportunity to take students on visits has made the Visiting Psychologist Program more attractive to the visitors.

Agencies wishing to apply or anyone wishing to nominate agencies for participation in the Visiting Psychologist Program should write to Ronald B. Kurz at the Educational Affairs Office, APA Central Office, 1200 Seventeenth Street, N.W., Washington, D.C. 20036.

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Gold Medal Award

The Trustees of the American Psychological Foundation invite the members of the American Psychological Association to nominate candidates for the 1975 Gold Medal Award. The Award is given to an American psychologist in recognition of a distinguished and long-continued record of scientific and scholarly accomplishments. The Award is limited to psychologists 65 years of age or older and is also limited to those residing in North America. Nominations should be accompanied by a statement highlighting the accomplishments of the nominee. Nominations should be addressed to: Secretary, American Psychological Foundation, 1200 Seventeenth Street, N.W., Washington, D.C. 20036, *to be received no later than March 15, 1975*. Decisions will be made by the Trustees of the Foundation.

Comment

How High Are the Odds Against Women?

Bernstein and Russo (February 1974) point with pride to the achievements of earlier women psychologists and conclude that these pioneers worked against adverse circumstances and incredible odds. Think, they say, what we could do if we had an equal chance. Fortunately these earlier women did not seem to be aware of adverse circumstances and incredible odds and, in the usual way, they saw things not as things are but as they were. Historically, all significant contributors, male or female, have faced the odds and won.

In a preliminary quiz, Bernstein and Russo tested the reader's knowledge of some productive women. This reader, a "foremother" in time if not in eminence, made a high score. It can be taken for granted that a worker in a field will know by work and by sex who has accomplished what. To the authors' list, I would add Bluma Zeigarnik (of the Zeigarnik Effect) and Tamara Dembo who, with Maria Ovsiankina, worked in the 1920s with Kurt Lewin in Germany; also Myrtle McGraw, Leta Hollingworth, and Harriet Babcock in New York. Among women who worked with Terman, add Melita Oden. Then, if Anna Freud, Melanie Klein, and Karen Horney, why not Susan Isaacs, Helene Deutsch, and Marie Bonaparte?

Like men, women of talent and determination make their way, regardless of climate. If there is discrimination, they find a way around it. When publishers would accept only male authors, Jane Austen, the Brontë sisters, George Eliot, and George Sand, among others, wrote under male pseudonyms; time has taken care of the rest.

Going back to the Greek dramatists, one finds a gallery of women bewailing their powerlessness, but as dauntingly formidable as any member of their sex ever seen on or off stage. And, perceptively, the Greeks embodied wisdom in a feminine figure. Bewailing their grievances while wisely exercising real power has proved to be an effective feminine pattern.

Shakespeare's women were certainly no repressed house drudges. Portia is the epitome of the fully developed personality, a good friend for a man to have, if not easy to live with. No one exposed to England's Elizabeth I could conclude that women were inferior in any way, or that a women judge was unusual.

Of more recent foremothers, the list of outstanding women reaches into space. Even in the less feminine fields there are Lise Meitner and Marie Curie in physics; Helen Wills, May Bundy, and Suzanne Lenglen in tennis; Gertrude Ederle and Annette Kellerman in swimming; Aline Saarinen and Ada Louise Huxtable in architectural criticism; Simone Weil, Hannah Arendt, and Susanne Langer in philosophy. For years, Sylvia Porter has written a financial column; women specialists on Wall Street write and appear on television; Claudia Cassidy, a longtime music critic in Chicago, made and unmade reputations; in the political arena, Rosa Luxemburg, Emma Goldman, La Pasionaria, Indira Ghandi, and Golda Meir; in foreign correspondence, Dorothy Thompson, Dorothy Kilgallen, and Margaret Bourke-White; in creative choreography, at one time a male field, Martha Graham, Doris Humphrey, Ruth St. Denis, and Anna Solow.

In painting, we find Vigee Le Brun, Rosa Bonheur, Marie Laurencin, Berthe Morisot, and Mary Cassatt, whose work was hung in Paris when Cezanne was rejected. Kathe Kollwitz, a wife and mother, pursued her art in early twentieth-century Germany with the full cooperation of her husband and of society. Louise Nevelson is considered by some to be America's foremost sculptor.

Among Nobel Prize winners are Sigrid Undset, Selma Lagerlof, Nellie Sachs, Grazia Deledda, Gabriela Mistral, and Pearl Buck in literature; Marie Curie and Maria Goeppert-Mayer in physics; Irene Joliot-Curie and Dorothy Crowfoot in chemistry; Gerty Cori in medicine; Baroness von Suttner and Jane Addams for peace. Among Pulitzer Prize winners for fiction, poetry, drama, historical writing, and journalism, the feminine names are too numerous to list.

Women have always played a large part in music. If one eminent pianist finds that her career excludes motherhood, another, equally eminent, is a mother and grandmother, and tours and records in her late seventies. How great the talent and the will to utilize it depends on the individual. Women have been musically trained in large numbers, and no prejudice exists. Women composers (and there are several hundred of them), contrary to public opinion, were neither stifled nor denied an audience. If there are no Bachs and Beethovens among them, only one Bach and one Beethoven appear among thousands of male composers (Rubin-Rabson, 1973).

In early nineteenth-century Germany, hardly known for its liberal attitude toward women, Clara Schumann, wife of Robert, was a touring virtuoso, a sought-after teacher, a gifted composer; she also bore eight children and supported the household on her earnings. In the same period, Louise Bertin, a gifted French writer and composer, wrote an opera to a libretto by Victor Hugo; the opera

was performed. If it has disappeared, so have most other operas.

French composer Germaine Tailleferre heard her own works performed in public by famous artists and orchestras. Ethel Smythe, a suffragette and composer, was made Dame of the British Empire for her compositions. Nadia Boulanger, herself a winner of the Prix de Rome, has been a teacher of nearly every composer since 1920, but modestly admits to little merit in her own compositions. Marion Bauer, a professor of music at New York University, was recognized by critics as competent, and her work was widely performed. She professed to be pleased with her success and her treatment in the critical musical world but "had no specific goal nor definite plan to do anything important [Rubin-Rabson, 1973]." Talent alone is not enough; the imperious urge to accomplishment must be present as well.

Professional women, psychologists as well as others, show characteristics somewhat aberrant from the norm (Bachtold, 1970; Ross, 1973). They are independent, self-sufficient, competitive. A large number are single, allowing them to work more single-mindedly than their family-responsible male colleagues. Among the married, the divorce rate is high, children absent or few. There is, then, no lack of working time. However, with or without home distractions, the professional woman has only so much personal substance to expend. The more involved she is with activities outside of her prepared field, the more she must leave to others those productions that result in recognition in the field.

As women move rapidly into all areas, the competition increases, but unfortunately they still seem to move into the same old areas. Matina Horner (1974), associate professor of psychology at Harvard, mother of three and at 32 already president of Radcliffe College, said recently, "Radcliffe's entering freshmen often express strong desires to major in esoteric [!] areas such as physics and chemistry, but along the way . . . change their majors and settle for easier subjects." The behavioral sciences?

This situation is not unique to American women. In Switzerland ("Congestion in Psychology Courses," 1972), the number of students in psychology has doubled in four years, compared to a 5% increase in the top technical schools. A large number of women are seeking marriage preparation, not professional skills, and are using the university as a "waiting room peopled by many men." Another group is looking for "snap courses," or a solution to personal problems, or has some philanthropic urge to aid others. The dropout rate, however, is a university lifesaver. Of all 1961 matriculates, only 33% finished the course, compared to 86% of law students, and 82% of medical students. In 1970, among those choosing psychology as a major subject, 50% had dropped out by the end of the fourth semester.

In psychology, or in any other pursuit, no one, male or female, is "given an equal chance." With talent and will, one "makes an equal chance." The foremothers, hard-working and undeterred, would say: "My daughters, discrimination, like fear, knocks on the door; one rises to open; there is no one there. Forget your grievances, dry your tears, and get to work."

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GRACE RUBIN-RABSON
Los Angeles, California

On the Role of Mental Health Workers in the Criminal Justice System

Silber (April 1974) attempted to describe and justify guidelines for the development of a role for mental health workers within the criminal justice system and existing correctional settings. His defense was disturbing in that he made a number of contradictory statements concerning the issues of mandatory treatment, the receptivity of the correctional system and prisoners to mental health workers, and the manner in which one should fulfill one's responsibilities as the therapist to the prisoner-client. Silber's difficulties in the handling of these issues are worth noting because they point out some of the basic conflicts which must be overcome if mental health workers are ever to play a more meaningful role in the treatment of offenders than the role prescribed by Silber.

1. Silber seems ambivalent and frequently inconsistent in his statements about mandatory treatment for prisoners. He claims to eschew mandatory treatment in principle (p. 243) and urges that the prisoner must make the decision as to whether he will receive treatment. On the other hand, Silber believes that:

> Diagnosis and classification should occur as soon as the offender is incarcerated. . . . the results should be discussed at a case conference. Major decisions concerning diagnosis, therapy, and placement with the system would be made there [p. 242].

These results are to be used to "motivate the prisoner to get involved in his own rehabilitation [p. 242]." The major issue is whether such procedures in practice, occurring as they would in a controlling and somewhat threatening environment, might constitute direct or indirect coercion of the prisoner (which Silber advises against on p. 243) to participate in treatment. To refuse treatment in such a system would be to risk being labeled uncooperative or negativistic (Brodsky, 1973), to which extent coercion would be inherent in Silber's procedure. Furthermore, the inten-

tion to "motivate" prisoners to become involved in treatment is in itself a treatment goal, suggesting that in fact some sort of psychological intervention designed to change attitude or behavior would be mandatory and part of the admission process for all prisoners. Silber avoids discussing this issue in the article.

2. Silber recognizes that many prisoners might not accept offers of treatment readily, but he feels that many could be brought to do so if they were "treated as . . . dignified and reasonable human being[s] [p. 242]." While this view might have merit, it is doubtful that many prisoners would be responded to in this manner if they were viewed by the majority of mental health workers as they are described by Silber, that is, as "some variation of the character disorder/psychopathic personality label. Such prisoners are hostile, suspicious, and immature [p. 242]." The latter characteristics do not necessarily follow from the diagnostic categories mentioned, and the diagnostic statement itself represents an overgeneralization concerning prison populations. In six of the nine studies of psychiatric evaluations of offenders reviewed by Brodsky (1973), from 41% to 85% of the offenders were evaluated as "normal." Brodsky concluded that our present knowledge of offenders does not allow us to presume homogeneity (see also Dean, 1973). It is precisely the stereotyping of prisoners which Silber demonstrated that would make it difficult for many mental health workers to approach a prisoner as "dignified and reasonable."

3. Silber feels that treatment efforts with offenders should occur within existing correctional programs because they "have the practical advantages of being already in existence" and have "avowedly rehabilitative goals [p. 241]." On the other hand, Silber recognizes that the staff members of prisons "usually view the professional staff with ambivalence or outright hostility [p. 242]." Such hostility is in part a result of the infusion of those with *rehabilitative* goals into a system which has a long tradition of *retributive* goals. That such institutions are "already in existence," then, is at least as much a liability as an asset with regard to the establishment of effective treatment programs within these institutions. In addition, whether or not such institutions "avow" rehabilitative goals is irrelevant; given the long-standing fear, suspicion, and commitment to retribution which Silber admits are the hard realities frequently confronting the mental health worker in the correctional system. Finally, one can argue that the routine of prison life necessary to maintain security (Sutherland & Cressey, 1970, chap. 23) is antithetical to the implementation of a treatment " 'package' tailored to the individual [p. 242]" which Silber recommends.

4. Silber argues that in the correctional system, as in other treatment settings, "the first concern of the therapist is the client [p. 242]." In attempting to practice this maxim in my own work with institutionalized delinquent boys, I have sometimes felt that I have failed before I have begun because I have had no power to avert a boy from commitment to the institution itself. The institutional experience does not have marked negative effects on all prisoners, but it does on some: In the case of delinquent adolescents, the issue of frequently needless or ill-advised institutionalization has been recognized (e.g., Rose, 1972; Sutherland & Cressey, 1970). Decisions occur in the courts concerning whether an offender will be institutionalized, yet Silber counsels mental health workers to "stay out of the adjudication process and in the correctional system [p. 241]." Thus he disallows any mental health workers as advisers to the court concerning probationary or dispositional decisions which might be in the best interests of the offender. Since the offender potentially is destined to be a "client," who is the "first concern of the mental health worker," in the correctional system, it seems inconsistent that there should be no input by mental health workers at the point when the offender's "client" status is determined, especially in the case of juvenile offenders.

It would seem, then, that Silber's suggestions for mental health workers in correctional settings are not those which could be executed without exhibiting in one's thoughts and actions the same sort of inconsistencies which appear in Silber's argument. This does not mean that one could not develop a more meaningful role for professionals in correctional settings.

In searching for a more viable role for mental health workers, however, we should probably: (*a*) focus our attention *first* on modification of correctional staff attitudes and system variables, and *then* on providing treatment to individual offenders themselves; (*b*) decide whether to accept or reject, for all, some, or no offenders, the idea of mandatory treatment, and then defend and practice our decision without pretense; (*c*) begin to refine sufficiently our views of offenders and treatment methods appropriate for them so that we do not fall prey to the stereotyping and generalization that have plagued this area; and (*d*) not feel bound to confine ourselves to roles within correctional institutions—that is, explore ways in which the mental health professional may assist the offender at the postinstitutional (release) phase (see Erickson, Crow, Zurcher, & Connett, 1973) and may act as consultant to the court during the preinstitutional (probation and disposition) process.

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J. Thomas Grisso
St. Louis University

Reflections on "Reflections on Introspection"

Radford's (April 1974) reflections on introspection make a number of substantive statements about introspection as event and process. However, his reflections reflect prevalent misconceptions and distortions of the behaviorist view of the status of introspection in psychological inquiry. First, he pursues an argument which is a misstatement of the behaviorist position; and he then avoids reckoning with empirical as opposed to doctrinaire grounds underlying the behaviorist emphasis on behavior. He thus annihilates a straw man. The following is an attempt to clarify the behaviorist position.

First, those who pursue knowledge within a scientific framework of inquiry know that all data are admissible insofar as they are replicable and communicable. The relevance of any reliable data to theory building, prediction, and control is an empirical question and as such is not to be decided by definitional fiat or on a priori grounds. Further, the inclusion of particular events as data within an area of general inquiry is necessarily a function of available and *publicly* verifiable conditions for their observation, as well as of the theoretical and empirical utility of such data. Again, definitional fiat provides no scientific ground for the exclusion of data. The proper scope of a subject matter is, like that of all sciences, loosely defined, continuously evolving, and therefore open to both empirical possibility and theoretical advantage. So, for example, chemistry as science, once defined as "the study of matter," properly and necessarily incorporated what was seen as nonmaterial (or nonmatter) concepts, such as valence, later developed into electrical charge, into its domain, once the technology was developed whereby such theoretical constructs and hypothesized laws involving them could be verified empirically. As a result, electromagnetic theory and action are currently central to the understanding of the actions and reactions of matter. If we may speculate by analogy, it is reasonable to similarly avoid a priori limitations on the exploration of potentially "interesting" data such as introspection. There is no behaviorist argument with Radford here.

Radford errs when he cites the doctrine of objective experiment and confounds this doctrine, which is obviously a methodological given for behaviorists, with an alleged doctrine of behaviorist psychology, namely, that the proper and presumably only acceptable subject matter of psychology is behavior and that data regarding conscious experience obtained by the process of introspection and made public through verbal report are off limits. Now it just is not the case that behaviorism rejects introspection as process with or without the data of verbal report. The behavioristic view is to treat verbal report as data (verbal behavior), and of course such data are obtained through the process of introspection. However, it is also true that data obtained through the process of introspection (one may seriously question whether introspection is a single process or reflects different processes depending on the conditions of inquiry) are unreliable and poor predictors, and that such data, in the traditional and currently available methods of ordering and conceptualizing them, have not been demonstrated to have predictive and therefore empirical power.

Now data that do not appear to be significant under some conditions may be so under others. It remains for whomsoever of the behavioristic, humanistic, psychoanalytic, etc., schools interested in public inquiry to conceptualize introspectively obtained findings in a way that advances theory and applications. This is what science is and no more, and arguments over whether introspection exists and whether the results of introspection constitute the subject matter of psychology are spurious. The issue is whether the procedure and the data can be used in lawful ways. It is one thing to stand with both feet in the camp of science and to look and work for lawfulness using introspection. It is something else to stand with one foot in science and the other in a body of statements (verbal reports?) which, after 100 years of trying, have not been conceptualized in a form that provides scientific meaning in more than the immediate self-observational sense and to loudly proclaim the import of these data. Let those who are dedicated to the import of particular data study them and articulate their meaningfulness.

Behaviorist psychology is a field of intervening variables to which introspectively obtained data might be fitted (Edwards, 1954; Goldiamond, 1958; Mowrer, 1960; Solomon & Wynne, 1953; Swets & Birdsall, 1955; Tanner & Swets, 1954). I suggest that those interested in developing the relation between introspection and behaviorally derived findings and theory continue to work at it as an alternative to making pseudoarguments against a pseudoposition.

A final analogy with another domain of inquiry with similar problems may be instructive. The status of pain in medical-physiological inquiry is in certain ways comparable to the status of conscious experience in behavioral-psychological inquiry. The major access to the observation of pain is through the subject's introspectively obtained verbal report. The verbal report which makes public the pain event has been shown to be more reliable, more precise, and more sensitive than any other measure (such as GSR) or combination

of measures (such as GSR or respiratory rate), and has therefore useful and respected status as an indicator of independently observable events, for example, pinpricks. This reliability, accuracy, and precision force the scientific observer to accord the verbal report of pain special status as an indicator of physiological malfunction which stands even where other observational procedures fail (as in headache or gastrointestinal disorders), although the verbal report of pain is neither necessary nor sufficient to define a physical trauma. Thus, verbal report of pain is of great utility although there may be physical trauma without pain, or reported pain without apparent physical defect but with defect presumed (as in headache), or reported pain with insignificant likelihood of associated physical defect (as in psychogenic pain). Reported pain is taken as a real event, primarily because all observers experience it. But more important scientifically, correlates such as tissue trauma are discovered; so are other indicators such as heart rate, behavioral reactions, and so on. There is no doubt of the existence of pain nor of its significant relationship to other observable events. Reported pain thus has a diagnostic value, has a predictive value, can be reduced to the physiological terms of neural receptors and networks, and thus has powerful theoretical import.

Similar characteristics hold for some introspective data in psychology, as in the experimental study of dream reporting and its correlates (Dement & Wolpert, 1958), and such work is likely to be expanded into other areas of study. Only as wishing won't make it so, neither will misdiagnosis of the commitment of behaviorism to empiricism as a thesis denying the existence or potential utility of introspectively gained data make it so.

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HELEN M. BRODY
Adelphi University

Open Minds and Ideology

In response to my comment (Dworkin, June 1974) on the shift in his views between 1967 and 1969, Jensen (June 1974b) says he has changed his mind, and that is the mark of a scientist as opposed to an idealogue. He also deplores any contamination of psychology with political ideology. He has changed his mind because he has tested 15,000 more children, read the Coleman report, and learned some quantitative genetics.

On the politically and educationally most crucial issue—the presence of racial genetic differences in IQ—Jensen has continued to change his mind. It is instructive to follow his development and bring it up to date.

In 1967, Jensen's position was:

> since we know that the Negro population for the most part has suffered socioeconomic and cultural disadvantages for generations past, it seems a reasonable hypothesis that their low average IQ is due to environmental rather than genetic factors [p. 10].

In 1969, Jensen stated:

> So all we are left with are various lines of evidence, no one of which is definitive alone, but which, viewed all together, make it a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-white intelligence difference [p. 82].

Note by the way the crucial ambiguity of the phrase "strongly implicated." On the one hand it could mean that it is strongly (highly probable) implied that genetic factors play some role; on the other, it could mean that it is implied that genetic factors play a strong (major) role. For someone writing on an issue of such moment to phrase his conclusions in such an ambiguous way is, at the least, negligent.

In 1973, Jensen claimed:

> All the major facts would seem to be comprehended quite well by the hypothesis that something between one-half and three-fourths of the average IQ difference between American Negroes and whites is attributable to genetic factors, and the remainder to environmental factors and their interaction with the genetic differences [p. 363].

I fail to understand how testing 15,000 children (or 15,000,000), reading any number of reports on compensatory education, and least of all learning some behavior genetics could lead anyone to make the last claim. Thoday (1969), Gibson, Mascie-Taylor, and Thompson (1972), Lewontin (1970), Cavalli-Sforza and Bodmer (1971), Layzer (1974), and others have pointed out the insuperable obstacles, both theoretical and empirical, to making any quantitative claims of that nature about subpopulation differences, either between social classes or between races.

As to the role of ideology, it seems to me that Jensen's (1973) following attitude toward the promulgation of hypotheses must be influenced by a specific weighting of the values of scientific knowledge versus the possible social consequences of one's work:

> There are always differences among investigators working on the frontiers of a field. They differ in their weight-

ing of items of evidence, in the range of facts in which an underlying consistency is perceived, in the degree of caution with which they will try to avoid possible criticisms of their opinions, and in the thinness of the ice upon which they are willing to skate in hopes of glimpsing seemingly remote phenomena and relationships among lines of evidence which might otherwise go unnoticed as grist for new hypotheses and further investigations. On all these points we [Jensen and his critics] differ in varying degrees, and my own inclination is perhaps to be somewhat less conservative than would be some other students in dealing with the central topics of this book. My own reading of the history of science, however, leads me to believe that conservatism in generating hypotheses and in seeking means for testing them has not made for progress as often as a more adventurous approach [pp. 3–4].

Being adventurous may be admirable when the risks of such adventures are born by the adventurer or by others who consent to such risks. In this matter Jensen faces only the risk of being wrong in his speculations; others face the risk of increased political, economic, and educational oppression.[1]

With respect to pessimism about the prospects for improving educability by environmental means, again it seems to me that one's degree of optimism or pessimism is necessarily, in part, a function of one's ideology. By ideology I mean here the most general, fundamental, and usually unexamined beliefs about human nature and its limits, the kind of knowledge we can have on such matters, the relationship of human nature to normative issues, the limits set by biology to political possibility, and the use of science to rationalize (or change) existing social and economic relationships.

Jensen (1969) said: "Compensatory education has been tried and it apparently has failed [p. 2]." Compare, for example, Bereiter's (1971) conclusions from the same set of facts:

> To treat the eventual vanishing of preschool effects as failure is to imply either that preschool compensatory education is futile or that the effective method has yet to be discovered. Either of these conclusions *could* be true, but those who think they follow from current evidence are applying criteria of success to preschool education that are not applied in any other realm of human effort. They are asking the doctor for a pill they can take when they are ten that will prevent them from getting fat when they are fifty.
>
> Reason would have it that if we have designed a preschool program that produces benefits lasting for three years, then instead of agonizing that they didn't last for five or ten, we should be concerned with what can be done in the years after preschool to produce further benefits [p. 13].

One's hopes also ought to be, of course, a function of what has been tried and of what our best available theories tell us.

In fairness to Professor Jensen, he has recently indicated a change of mind in the opposite direction. In 1972, Jensen wrote of Burt's work on the genetics of mental abilities:

> This is the work that will most probably secure Burt's place in the history of science. [Burt's studies consisted of] larger, more representative samples than any other investigator in this field has ever assembled. . . . These studies, since corroborated by more recent investigations, scientifically established that genetic factors are the predominant cause of individual differences in mental ability [p. 117].

In 1974, Jensen concluded after a thorough study of Burt's data on kinship correlations with their "unknown, ambiguous, or inconsistent sample sizes":

> the correlations are useless for hypothesis testing. Unless new evidence rectifying the inconsistencies in Burt's data is turned up, which seems doubtful at this stage. I see no justifiable alternative conclusion in regard to many of these correlations [1974a, p. 24].

It is unfortunate that social policy is made on the basis of such shaky data. All of us who deal in the realm of ideas would do well to remember the philosopher Bradley's warning that "ink and paper can cut the throats of men." When we are concerned with the minds of children, that warning takes on added force.

[1] For an extensive discussion of this and related issues, see Block and Dworkin (in press, Part II).

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GERALD DWORKIN
Department of Philosophy
University of Illinois
at Chicago Circle
Visiting Fellow
Battelle Seattle Research
Center

The Republican Party and Santa Claus

I would like to congratulate Dworkin (June 1974) on his cogent analysis

of the shift of views by Jensen. Why didn't anybody think of it before? Back in 1967 there were many American children who believed in Santa Claus, but by 1969 they had completely shifted to disbelief in the whiskered gentleman. Could this shift be because of new information? Of course not—it was the changing political, intellectual, and social currents in American society.

The level of criticism aimed at Jensen has unfortunately sunk to personal attacks on the man and not at the research itself. Such attacks will shed no new light on the subject of genetic differences in intelligence nor give impetus to further research to prove or disprove Jensen's theory.

I would like to think that such thinly veiled cheap shots at the integrity of a researcher by impugning his motives has no place in psychology.

REFERENCE

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JOHN HARTNETT
Virginia Commonwealth University

NOTICE TO APA AUTHORS

The American Psychological Association announces publication of the second edition of the *Publication Manual* in August 1974. This new edition, which supersedes the 1967 Manual, will be adopted by the 14 APA journals in 1975.

The new Manual is more comprehensive than the previous edition. It updates APA publication policies and procedures and incorporates changes in editorial practice since 1967. For instance, APA now sends many authors their copy-edited manuscripts for review before they are set into type, and some editors now use blind review procedures. The second edition also includes up-to-date statements of the coverage of each APA journal including the *Journal of Experimental Psychology* which will be published in four separate sections in 1975.

The new *Publication Manual* initiates several changes in APA style. These changes are announced in the August 1974 *American Psychologist* and will be introduced in the APA journals in January 1975. During the period of transition to the new style, authors should note that (*a*) all manuscripts published in 1974 will be copy-edited according to the 1967 Manual, (*b*) manuscripts accepted in 1974 and published in 1975 will be copy-edited to conform to the new Manual. Starting in 1975, accepted manuscripts that depart significantly from the Manual will be returned to authors for correction.

Authors will be encouraged by the changes in the second edition. The new APA style simplifies reference forms; eliminates unnecessary underlines, brackets, and other devices; supports appropriate use of "I" and "we"; and generally clarifies typing requirements. Material is arranged for maximum convenience to authors and typists, and all sections are cross-referenced and indexed.

The new *Publication Manual* is available after August 1 for $3. Send orders to APA Publication Sales, 1200 Seventeenth Street, N.W., Washington, D. C. 20036. Orders of $15 or less must include payment unless they are submitted on institutional purchase order forms.

Announcements

Deaths

Terry Roger Anders, January 1974
George W. Angell, August 29, 1974
Jean H. Baer, September 14, 1974
Dorothy S. Balcom, date unknown
Joseph D. Blanchard, February 26, 1974
Leon Brody, August 19, 1974
Max Cooper, July 27, 1974
Donald H. Dietrich, August 27, 1974
Seymour H. Fuchsman, date unknown
Harry M. Grayson, August 4, 1974
Arthur M. Jordan, date unknown
Zelda S. Klapper, June 6, 1974
Ellen Leibowitz, July 1974
Aimee W. Marrs, date unknown
Richard H. McKenney, September 24, 1974
Herbert Z. Sanderson, October 1, 1974
Agnes S. Sharp, August 5, 1974
Verne Steward, December 19, 1972
Samuel Tenenbaum, date unknown
J. Gordon Tolmie, date unknown
Joseph R. Toven, January 29, 1973
John P. Umberger, date unknown
Douglas E. Wax, June 13, 1974
John C. Wilds, August 25, 1974
Paul Yaffe, May 9, 1974
Harry C. Yudin, April 8, 1974

Fellowships and Postdoctoral Programs

Devereux Foundation Institute of Clinical Training: Applications are available for the predoctoral internships and postdoctoral fellowships in clinical psychology. The 12-month internships provide training and experience with mentally and emotionally handicapped children, adolescents, and young adults. A combined stipend and housing allowance ranging from $5,100 to $7,500 ($3,600 is tax exempt) is available to qualified applicants who are U.S. citizens. For information write to Henry Platt, Director, The Devereux Foundation, Institute of Clinical Training, Devon, Pennsylvania 19333.

Eastern Virginia Medical School Department of Psychiatry and Behavioral Sciences and Psychiatric Associates of Tidewater, Norfolk, Virginia: Now accepting applications for pre- and postdoctoral internship in clinical psychology beginning September 1975. Program offers advanced, closely supervised experiences in a variety of clinical activities in both a comprehensive community mental health center and a private psychiatric facility offering both inpatient and outpatient services. Stipend: $5,000–$7,000. For further information write to William L. Scarpetti, Director, Division of Psychology Education and Services, Department of Psychiatry and Behavioral Sciences, Eastern Virginia Medical School, P.O. Box 1980, Norfolk, Virginia 23501.

Johns Hopkins University School of Medicine, Division of Psychology, John F. Kennedy Institute for Habilitation of the Mentally and Physically Handicapped Child: One- and two-year postdoctoral fellowships available, providing experience in the use of behavior modification techniques for identification and treatment of the exceptional child in a major interdisciplinary setting. Applicants must have a PhD in psychology or a related discipline prior to appointment. Stipends: $6,000–$7,000 for first year with $600 dependent allowance. For further information write to J. Dardano, Division of Psychology, John F. Kennedy Institute, 707 N. Broadway, Baltimore, Maryland 21205.

Langley Porter Neuropsychiatric Institute, University of California in San Francisco: Announces internship openings for clinical psychology PhD candidates. These internships are available to students who have completed at least two years in graduate clinical psychology programs in APA-approved universities. Uncertain and limited funding makes us particularly interested in qualified applicants who can bring their own funds. For further information write to Director of Training in Clinical Psychology, Langley Porter Neuropsychiatric Institute, University of California, San Francisco, California 94143.

University of Maryland School of Medicine: Now accepting applications for pre- and postdoctoral internships in pediatric and clinical child psychology presenting comprehensive field training in clinical child, pediatric psychology, and developmental disabilities. The pediatric program is administered by the Pediatric Psychology Staff of the Department of Pediatrics at the University of Maryland. Stipends: $5,000. For further information write to Thomas Kenny or Rudolph Bauer, Department of Pediatrics, University of Maryland Hospital, Medical School, Lombard and Greene Streets, Baltimore, Maryland 21201.

Meyer-Manhattan Psychiatric Center: Now accepting applications for internships in psychology for September 1975. Primary objective is to provide as broad a range as possible in psychological diagnosis, work on inpatient wards, day hospital, outpatient, and other mental health facilities. A specialization in community or behavior modification is also available. For further information write to Saul A. Grossman, Chief Psychologist, Meyer-Manhattan Psychiatric Center, Ward's Island, New York 10035.

Reiss-Davis Child Study Center: Now accepting applications for the two-year postdoctoral training pro-

gram in intensive psychoanalytically oriented clinical child psychology. Program includes supervised experience in the diagnostic and psychotherapeutic processes, related seminars, and opportunity for experience in brief psychotherapy. For further information write to Esther Lampl, Director of Psychological Services, Reiss-Davis Child Study Center, 9760 West Pico Boulevard, Los Angeles, California 90035.

University of Wisconsin Medical School: Announces a postdoctoral fellowship in clinical psychology offering advanced training in a variety of psychotherapeutic modalities with adults and children, community and hospital consultation, crisis intervention, and inpatient treatment. Fellowship is for 1 or 2 years. Basic stipend is $6,000, with possible supplementation to a maximum of $8,000; the first $3,600 is tax exempt. There is an allowance of $600 per dependent. Applicants must have a PhD in clinical psychology and a clinical internship. Preferred starting date is July 1, 1975. For further information write to David G. Rice, Department of Psychiatry, University of Wisconsin Medical School, 1300 University Avenue, Madison, Wisconsin 53706.

Distinguished Professional Contribution Award

The Distinguished Professional Contribution Award is presented annually to a member of the Association who has advanced psychology as a profession by distinguished contributions to knowledge or practice, subject to the following limitations: (a) members of the award committee, former recipients of the award, the President and the President-elect of APA are ineligible; (b) the committee shall seek diversity in selecting recipients, avoiding as far as possible the consecutive selection of more than one person representing a specialized topic, a specific material, a given method, a particular application, or a specific specialized service. The winner receives $1,000. The award was first given to Carl R. Rogers in 1972. In 1973 the winner was David Wechsler, and in 1974, Noble H. Kelley. Those wishing to submit a nomination are asked to write for nomination forms to: Professional Award, Department of Professional Affairs, APA, 1200 Seventeenth Street, N.W., Washington, D.C. 20036. ***Deadline for nominations is May 15, 1975.***

National Conventions

American Psychological Association: August 30–September 3, 1975, Chicago; 1976, Washington, D.C.; 1977, San Francisco; 1978, Toronto; 1979, Atlanta

For information write to:

Doris Entwisle
c/o Candy Won
American Psychological Association
1200 Seventeenth Street, N.W.
Washington, D.C. 20036

Southeastern Psychological Association: March 26–29, 1975, Atlanta; March 17–20, 1976, New Orleans; May 4–7, 1977, Hollywood, Florida

For information write to:

Edward H. Loveland
School of Psychology
Georgia Institute of Technology
Atlanta, Georgia 30332

Eastern Psychological Association: April 3–5, 1975, New York City; April 22–24, 1976, New York City

For information write to:

Murray Benimoff
Department of Psychology
Glassboro State College
Glassboro, New Jersey 08028

Southwestern Psychological Association: April 17–19, 1975; Houston, Texas

For information write to:

Southwestern Psychological Association
P.O. Box 7156
University Station
Austin, Texas 78712

Western Psychological Association: April 24–26, 1975; Sacramento, California

For information write to:

George Parrott
Department of Psychology
California State University, Sacramento
6000 J Street
Sacramento, California 95819

Midwestern Psychological Association: May 1–3, 1975; Chicago, Illinois

For information write to:

Rudolph W. Schulz
Department of Psychology
University of Iowa
Iowa City, Iowa 52242

Rocky Mountain Psychological Association: May 7–10, 1975, Salt Lake City; 1976, Arizona; 1977, Alberta; 1978, Albuquerque, New Mexico

For information write to:

William Prokasy
205 Spencer Hall
University of Utah
Salt Lake City, Utah 84112

Southern Society for Philosophy and Psychology: March 27–29, 1975, New Orleans; 1976, Atlanta; 1977, Nashville, Tennessee

For information write to:

Michel Loeb
Department of Psychology
University of Louisville
Louisville, Kentucky 40208

Conference on Problems in School Psychology: January 24, 1975; Flushing, New York

For information write to:

O. Bernard Leibman
Coordinator, Graduate Program in School Psychology
Queens College
Flushing, New York 11367

Emil Froeschel Conference on Communication Disorders: January 31, 1975; New York, New York

For information write to:

George L. O'Reilly
Pace University
New York Campus
Pace Plaza
New York, New York 10038

Conference about the Teaching of Group: Theory and Practice: February 20–22, 1975; New Brunswick, New Jersey

For information write to:

Kenneth G. Roy
Group Studies Consortium
c/o Counseling Center
50 College Avenue
New Brunswick, New Jersey 08903

American Psychopathological Association: March 6–7, 1975; New York City

For information write to:

Edward Sachar
Bronx Municipal Hospital Center
Eastchester Road and Pelham Parkway
Bronx, New York 10461

The American Society of Group Psychotherapy and Psychodrama: March 20–23, 1975; New York, New York

For information write to:

Stephen Wilson, ACSW
Program Coordinator
39 East 20th Street
New York, New York 10003

American Association of Sex Educators and Counselors: April 2–5, 1975; Washington, D.C.

For information write to:

AASEC
5010 Wisconsin Avenue, N.W.
Suite 304
Washington, D.C. 20016

Society for Research in Child Development: April 10–13, 1975; Denver, Colorado

For information write to:

Anne D. Pick
Institute of Child Development
University of Minnesota
Minneapolis, Minnesota 55455

Christian Association for Psychological Studies: April 13–15, 1975, Oklahoma City, Oklahoma

For information write to:

William L. Hiemstra
6850 South Division Avenue
Grand Rapids, Michigan 49508

National Center on Black Aged: "Aging: Black Women and Federal Policies: 1950–2080": April 13–15, 1975; Washington, D.C.

For information write to:

Jacquelyne J. Jackson
Box 3003
Duke University Medical Center
Durham, North Carolina 27710

New England Educational Research Organization: April 30–May 3, 1975; Provincetown, Maine

For information write to:

Marvin D. Lynch
College of Education
Northeastern University
102 The Fenway
Boston, Massachusetts 02115

International Conventions

International Conference on Psychology and Meditation: January 5–February 5, 1975; Kathmandu, Nepal

For information write to:

Henry Ganzler
507 Lincoln Street
Santa Cruz, California 95060

International Conference on Psychological Stress and Adjustment in Time of War and Peace: January 6–10, 1975; Tel Aviv, Israel

For information write to:

Norman Milgram
Department of Psychology
Tel Aviv University
P.O. Box 16271
Tel Aviv, Israel

National Institute for the Psychotherapies: January 22–26, 1975; San Juan, Puerto Rico

For information write to:

Henry Grayson or Clemens Loew
National Institute for the Psychotherapies
330 West 58th Street
New York, New York 10019

Fifth Annual International UAP Conference on Piagetian Theory and the Helping Professions: January 24, 1975; Los Angeles, California

For information write to:

Marie Poulsen
University Affiliated Program
Childrens Hospital of Los Angeles
P.O. Box 54700
Los Angeles, California 90054

Inter American Conference of Mental Health of Children and Youth: January 30–February 1, 1975, Mexico City, Mexico

For information write to

Julieta Tovar
Mexican Institution for Children's Welfare
3,700 Avenida de los Insurgentes Sur
Mexico City, Mexico

Ontario Psychological Association: February 6–8, 1975, Toronto, Ontario, Canada

For information write to:

Malcolm S. Weinstein
Counselling and Development Centre
York University
Downsview, Ontario

Seventh Banff International Conference on Behavior Modification: March 23–27, 1975; Banff, Alberta, Canada

For information write to:

Donna J. Fraser
Division of Continuing Education
University of Calgary
Calgary, Alberta
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Tenth International Congress of Gerontology (and Geriatrics): June 22–27, 1975; Jerusalem, Israel

For information write to:

The Congress of Gerontology
P.O. Box 16271
Tel Aviv, Israel

First Colloquium on School Psychology: June 26–28, 1975; Munich, Germany

For information write to:

Calvin D. Catterall
92 South Dawson Street
Columbus, Ohio 43209

International Conference on "Dimensions of Anxiety and Stress": June 29–July 3, 1975; Oslo, Norway

For information write to:

Irwin G. Sarason
Department of Psychology NI-25
University of Washington
Seattle, Washington 98195

Second International Congress of Collegium Internationale Activitatis Nervosae Superioris: June 30–July 3, 1975, Prague, Czechoslovakia

For information write to:

Second International Congress of CIANS
Czechoslovak Medical Society
J. E. Purkyně
Sokolská 31, 120 26 Praha 2
Czechoslovakia

Second International Congress of the Association for the Psychophysiological Study of Sleep: June 30–July 4, 1975; Edinburgh, Scotland

For information write to:

A. J. Hobson
Harvard Medical School
74 Fenwood Road
Boston, Massachusetts 02115

Third Biennial Conference of the International Society for the Study of Behavioral Development: July 13–17, 1975; Guildford, England

For information write to:

ISSBD Secretariat
Department of Psychology
University of Surrey
Guildford, Surrey GU2 5XH, England

Seventh International Congress of the International Association for the Study of Medical Psychology and Religion: July 14–18, 1975; Cincinnati, Ohio

For information write to:

Robert J. McDevitt
Secretary General, AIEMPR
Good Samaritan Hospital
Cincinnati, Ohio 45220

Seventh World Congress of the Deaf: July 31–August 8, 1975; Washington, D.C.

For information write to:

National Association of the Deaf
814 Thayer Avenue
Silver Spring, Maryland 20910

Thirty-Third Annual Conference of the International Council of Psychologists: August 30–September 3, 1975; Chicago, Illinois

For information write to:

Frances A. Mullen
4014 Cody Road
Sherman Oaks, California 91403

Third Congress of the International College of Psychosomatic Medicine: September 15–19, 1975; Rome, Italy

For information write to:

Ferruccio Antonelli
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00135 Rome, Italy

Second Pan-African Congress of the International Association for Cross-Cultural Psychology: December 29, 1975–January 1, 1976; Nairobi, Kenya

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